

## Remarks on J. T.'s "Epigraphische Anmerkungen"

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[A critique of two recent sets of remarks on a collection of Ugaritic texts. The critique is epigraphic in nature and claims that, in order to advance the discussion on finer problems of Ugaritic epigraphy, the epigrapher should resort to autopsy.]

As a further illustration<sup>1</sup> of the pitfalls of doing epigraphy from photographs, I offer the following remarks and corrections to J. Tropper's epigraphic remarks on the new edition of *KTU*.<sup>2</sup>

Though the epigraphic quality of *CAT* is lamentable, better than that of *KTU* but still unreliable,<sup>3</sup> and though many of Tropper's remarks are correct (see my concluding remarks) and we may only be grateful to him for making the attempt to correct the deficiencies of *CAT*, his observations are insufficient on two scores: (1) because they are not based on autopsy,<sup>4</sup> his readings have no greater claim to validity than do those in *CAT* (only typographical errors that may be verified against published photographs may be independently verified by someone who does not have access to the originals); (2) again because he has not examined the originals, Tropper gives an unwarrantedly positive view of the epigraphic quality of *CAT*: he has overtly accepted some false readings while not mentioning many others.<sup>5</sup> Because of the vast number

1. See my previous studies cited below under "Abbreviations".

2. J. Tropper, "Epigraphische Anmerkungen zur Neuauflage von *KTU*," *AuOr* 13 (1995) 231-39. Tropper's review of the new edition of *KTU*, in *AfO* 42-43 (1995-96) 264-74, also includes many "epigraphische Anmerkungen" and I will remark on these as well. The new edition of *KTU*, authored as was the first by M. Dietrich, O. Loretz, and J. Sanmartín, is entitled *The Cuneiform Alphabetic Texts from Ugarit, Ras Ibn Hani and Other Places (KTU: second, enlarged edition) (Abhandlungen zur Literatur Alt-Syrien-Palästinas 8)*, Münster 1995). Because of the acronymic character of most abbreviated titles, I prefer *CAT* to that of *KTU*(2) suggested by the authors (see my review in *JSS* 42 [1997] 132-27, esp. 132).

3. See my review (reference in preceding note).

4. In his article in *AuOr* (p. 231), Tropper states explicitly that his readings are based on examination of photographs in the photographic archives of Münster and Edinburgh.

5. Some of Tropper's readings seem to be based on wishful thinking rather than on data from his graphic sources (e.g., *CAT* 1.83:8; 1.109:12; 1.148:10; 2.44:5—see my remarks below on these texts). As I prepared these remarks, I noticed that most of these cases of wishful thinking involve texts which contain difficulties of one sort or another, difficulties that Tropper attempts to remove by re-reading the difficult passage (e.g., *kbdm* for *kkdm* in 1.109:12 or *tgrk* for *tgr* in 2.44:5). Because the ambiguities of photographs leave latitude for such wishful thinking, the rigor imposed by careful examination of the original is necessary for deciding such cases. On the problems of reading what "should be" in an ancient text, see B. Zuckerman, "On Being 'Damned Certain': The Story of a Curse in the Sefire Inscription and Its Interpretation" in: A.B. Beck *et al.*, ed., *Fortunate the Eyes that See*.

of my epigraphic disagreements with *CAT*, which can only be described in the requisite detail in the full edition of a text or group of texts, I only mentioned a few egregious examples in my own review. Here also, I will only remark on Tropper's remarks, rather than proposing a list of corrections for all texts I have collated—such a list would perforce be insufficiently argued and documented, and would still make a very long article. I have hesitated for a long time as to whether I should once again enter the epigraphic lists in this partial fashion, eventually deciding that I have a responsibility in those cases where I possess primary data to make an effort to keep those readings of Tropper's that are incorrect from being accorded instant acceptance.

Only readings from texts of which I have myself studied the original (often in collaboration with Pierre Bordreuil) are offered here. It appears of little use for me to claim that I see something on a photograph in contradiction to what someone else has seen, and I do not, therefore, comment on texts that I have only studied from photographs. Though a cast adds the third dimension to a reproduction, a cast has not gone through the same aging process as has the tablet, and it is often difficult even on a cast to distinguish between a break in the tablet and a partially preserved sign. For that reason, I do not include here comments on texts of which the cast is kept in Paris unless I have studied the original. References to my publications (or joint publications with others, in particular Pierre Bordreuil) are provided because they are often omitted by Tropper even though it is clear from the occasional explicit reference that he is aware of the readings I have proposed. In cases of as yet unpublished collations, I provide the projected title of the forthcoming work. These references are placed in square brackets at the end of each remark. To cut down on length, I refer to Tropper in my remarks as "T" and cite only the abbreviated title of his two contributions cited in note 2 as well as of works by myself or myself jointly with P. Bordreuil or others (see list of abbreviations). Tropper's format, i.e., texts as organized in *KTU/CAT*, is followed here, though I do add the RS-number in order to provide the universally accepted designation of the tablets. I do not react to corrections of typographical errors unless the reading of the signs themselves are at issue (e.g., the proper spacing between transliterated signs and the period used to transliterate the Ugaritic word-divider is not discussed here). Nor do I react to Tropper's suggested emendations/restorations nor to his corrections of emendations/restorations in *CAT* unless there is a clear epigraphic component to the suggestion. Nor do I react to Tropper's listing of Ugaritic signs that consist of an abnormally large number of wedges (e.g., {1} with four wedges)—this is a topic that deserves further study but that study needs to be as exhaustive as possible and, to have any validity, needs to be based on collation. I confess that I have not in the past given sufficient explicit attention to this phenomenon and thus do not care in every case to state categorically whether Tropper's observation is correct or not.

So as to avoid needless repetition of the point below, let it be said once here: it is astounding that a given reading can in Tropper's remarks be flatly asserted to be correct and an opposing reading incorrect, even in cases where the latter is based on collation of the tablet and an epigraphic commentary is provided. The example of *KTU/CAT* 1.109:21 is perhaps the most striking, all the more so as I have cited it in the past<sup>6</sup> as a striking example of epigraphic error based on doing epigraphy from casts and photographs.<sup>7</sup> I

*Essays in Honor of David Noel Freedman in Celebration of His Seventieth Birthday*, Grand Rapids MI 1995, 422-435. The only statement that I would criticize in Zuckerman's presentation is the following: "Perhaps someone will get back to Damascus some day and take the photograph that will decide the matter" (p. 435). It would have been preferable to include the methodological step of looking at the inscription with the human eye, as well as with that of the camera.

6. *Syria* 69 (1992) 165, n. 39; *BSOAS* 58 (1995) 234; *AfO* 42-43 (1995-96) 275; *JSS* 42 (1997) 136. Only the first two of these references were available to Tropper.

7. On the cast in Paris the reading is indeed unclear, which explains at least in part that the editor of this text, Ch. Virolleaud, created the error, for he apparently did not work from the tablet itself; that does not explain, on the other hand, how A. Herdner got the reading right, for, to the best of my knowledge, she did not have an opportunity to examine the tablet either.

know from having examined the tablet on several occasions that there is no {š} at the end of the line, but Tropper asserts, against my published assertion to the contrary, that the sign is indeed there. It strikes me as methodologically unsound to make such an assertion without having seen the original, against the explicit assertion of someone who has done so.<sup>8</sup>

— 1.3 i 11 (RS 2.[014]+). T's {krpn!} (*AfO*) is essentially correct, as compared with {krp[[m]]nm} ("[[x]]" denotes an erased sign in *CAT*): the scribe wrote a four-wedged {n} over two wedges, only the bottom tips of which are visible today; the tips are too small to determine what the sign was, though T's hypothesis that it was a partial {b} may not be ruled out. One may note the fact of the over-write, but there is no need for the exclamation mark, for the scribe's intention to write {n} is certain. *CAT*'s interpretation of the last wedge of the four-wedged {n} as the horizontal of {m} and of the second over-written vertical wedge as the vertical of {m} clearly does not reflect the scribe's final intention. [*Trial Cut*]<sup>9</sup>

— 1.3 v 4 (RS 2.[014]+). T's correction of {p}°n to {p}°nm (*AuOr*) is partially correct: the top wedge of the {p} is partially preserved whereas both the horizontal wedge and the top edge of vertical of the {m} are clearly visible: read {p}°nm.<sup>10</sup>

— 1.3 v 10 (RS 2.[014]+). T is correct in reading {y}°[n]y° (*AuOr*)<sup>11</sup> for {y}°[n] . rî [I].<sup>12</sup>

— 1.40 (RS 1.002). T notes (*AfO*) that the authors of *CAT* have adopted without question my reading of this text, but does not himself propose new readings or reconstructions. [*Leslau, Rituels*]

— 1.41 (RS 1.003):9. T is incorrect (*AuOr*) in accepting *CAT*'s reading of the second sign as {°} without doubt: the sign is damaged here and in RS 18.056 (the text closely parallel to this one), and the reading {t} is as plausible as {°}; indeed, by its relatively short left edge in this text {t} is to be preferred. [*Rituels*]

— 1.41 (RS 1.003):22. T is correct (*AfO*) that the encircled sign is {t}, against what he properly describes as an "unverständlich" comment in *CAT* to the effect that the sign is "t written as an encircled °." The {t} is the form dubbed "un trilobe" by P. Bordreuil and A. Caquot,<sup>13</sup> i.e., created by inserting the stylus in the clay and rotating it a few degrees, producing thereby three rounded corners rather than six sharp points. This text has the peculiarity of having been written by two scribes, one of whom (l. 1-30a) produced the {t} in the form of a *trilobe*, the other (lines 30b-55) the six-pointed form. [*Rituels*]

— 1.41 (RS 1.003):45 // 1.87 (RS 18.056):49-50. T's failure (*AuOr*) to correct *CAT*'s arbitrary

Because of her modest and unassuming character and because she worked in the shadow of others, the high quality of Herdner's work, both epigraphic and philological, is rarely acknowledged (cf., however, A. Caquot's assessment of Herdner in her work on *CTA* as "aussi scrupuleuse que modeste": *Congress Volume Paris 1992*, SVT 61 [1995] 1).

8. This formulation is intended to make the distinction between a transliteration that is based on collation, into which typographical error may creep and for the elaboration of which every detail of every sign may not have been checked (human fallibility being what it is, an epigrapher may neglect to check a reading closely unless it has been called into question by another scholar), and an explicit statement to the effect that a given reading has been checked and a particular result attained.

9. Though I collated the entirety of RS 2.[014]\* in 1981, I have only published col. I [*Trial Cut*], where there are few differences to previous editions, and for the present have no immediate plans for publishing the rest of the text.

10. For reasons unclear to me, Tropper prefers the convention for damaged signs observed in *KTU*, i.e., indicating a damaged sign by a following asterisk. I follow the Assyriological convention of using half-brackets; the authors of *CAT* decided to follow Herdner's convention in *CTA* of indicating undamaged letters in Italic script, damaged letters in Roman script.

11. Tropper's actual transliteration is "{y\*[n]y\*}," a typographical error for {y\*[n]y\*}.

12. Tropper follows the convention used by many Ugaritologists of using {a}, {i}, and {u} to transliterate the three Ugaritic *alif*-signs. To avoid confusion on the part of beginners and others who might think that the Ugaritic writing system notes vowels and because modern computer technology makes it easy to indicate that the letter *alif* stands apart from the other consonants in having three forms to indicate the consonant followed by one of three vowel classes, I prefer to use {á}, {î}, and {û}.

13. *Syria* 56 (1979), p. 296.

emendation of {l} to {d} in 1.87:50<sup>14</sup> has led to his accepting that the reconstruction at the end of 1.41:45 and 1.87:49 should be a measure proper for oil. In fact, the {l} before {šmn} in 1.87:50 indicates that {šmn} denotes the deity *šmn* and that the proper reconstruction at the end of the preceding line in each text is the offering that was presented to that deity. I restore {šm}, “two rams” for reasons of space and because the formula *tn šm* is common in the ritual texts. Elsewhere the deity *šmn* receives two birds (RIH 77/2B+:8-9), and that restoration is possible here but not preferred because of the extreme rarity of the formula *tn šrm* (normally the dual is used, without *tn*). [*Rituels*]

— 1.41 (RS 1.003):54. T is correct (*AuOr*) in observing that the lacuna at the beginning of the lines is too short for {[bš]} (if normally written, one might add), incorrect in denying the presence of a word-divider before the {š}, and correct in accepting that the sign after the word-divider is {š} (it was read as {y} in *KTU*; the text that I sent to the authors of *KTU/CAT* on 10 viii 94<sup>15</sup> had {š} at this point). [*Rituels*]

— 1.43 (RS 1.005):22. T is incorrect (*AuOr*) in asserting that only one sign may be restored in the lacuna after two partially preserved signs at the beginning of the line: such a decision depends on the restoration of the second partially preserved sign and of that of the sign following the break. More space is obviously available if the last mentioned sign is {h} rather than {y}; moreover, the width of the restored letters must be taken into account (from this perspective, the restoration in *CAT* of two {k}s and two word-dividers is implausible, as comparing the space occupied by other {k}s in this text will show). Therefore, even though the likelihood is that the lacuna held only one sign, the correct notation is {<sup>r</sup>-<sup>r</sup>[-(-)]<sup>r</sup>-<sup>r</sup>y}. [*Ritual, Rituels*]

— 1.46 (RS 1.009):11. I can only classify as correct T's acceptance (*AfO*) of my reading (explicitly credited) of {y<sup>r</sup>q<sup>r</sup>l<sup>n</sup> . ā<sup>r</sup>l<sup>p</sup>m<sup>r</sup>} rather than {y<sup>r</sup>q<sup>r</sup>[ln]. <sup>r</sup>l<sup>n</sup> . ā<sup>r</sup>l<sup>p</sup>m<sup>r</sup>}. T's notation of the reading in *CAT* (“{y\*q\*[ln]. <sup>r</sup>\*n . al\*p\*m}”) is, however, incorrect on two counts: the {y} is indicated as certain in *CAT* and T's asterisk is thus incorrect, and the {m} is indicated as uncertain in *CAT* and the sign should have had the asterisk. [*BSOAS 58, Rituels*]

— 1.46 (RS 1.009):16. I can only classify as incorrect T's assertion that *CAT*'s reading {k<sup>r</sup>b<sup>r</sup>[d]m} is right, as opposed to my {<sup>r</sup>k<sup>r</sup>k<sup>r</sup>d<sup>r</sup>m}. Moreover, T states neither here nor in his note to 1.109 (RS 24.253):12 that the two texts are parallel and that the other text has {kkdm}. (T's attempt to read {k b d m} in the other text is unacceptable—see below.) In point of fact, the lacuna is too small to accommodate the end of {b} and the entirety of {d}, as a glance at the very same sequence of letters in line 13 will show ({bd} there occupy about 21 mm of space, while only 16 or 17 mm are available here). It appears far more plausible to me to accept at face value the witness of RS 24.253 and to accept that the spacing between the {d} and the {m} was a bit more generous than usual or that the horizontal wedge of {m} was a bit longer than is usual in this text (the space occupied by {<sup>r</sup>d<sup>r</sup>m}, about 27 mm, is only slightly greater than expected in this section of the text where the signs are rather widely spaced: the following {wn} occupy about 23 mm, {<sup>r</sup>d<sup>r</sup>m} about 26 mm). [*BSOAS 58, Rituels*]

— 1.48 (RS 1.019):6-12. Though the ascription of ends of lines to heads of lines here poses problems, T's solution (*AuOr*) of positing an additional line between lines 10 and 11 is untenable, unless he wishes that line to have begun to the right of the left margin within what is presently lacuna. There is

14. *CAT* 1.87:51 must have had several more signs than line 49 and there is thus no reason to think that the word *dd* would have been split across two lines for reasons of space; therefore, to explain the splitting of *dd*, one would have to posit that the end of a line from the obverse had extended to the reverse and forced the split. The ends of lines 10-12, the area where the end of line 49 would have touched, are broken, but there is no reason to reconstruct any of the three as having so many signs as to trigger the split (the only one of these three lines for which the number of signs may be reconstructed is line 12, which once consisted of approximately sixteen signs; line 7, where a split occurs similar to the one assumed in *CAT*, has nineteen).

15. Cf. *CAT*, p. vi; Tropper, *AfO* 42-43 (1995-96), p. 265; Pardee, *JSS* 42 (1997), p. 136, n. 7.

simply no space between the first sign of line 10 and the first sign of line 11 to permit positing the presence of an additional line between them; indeed, the tip of the bottom wedge of the {h} in line 10 almost touches one of the points of the {t} in line 11. Restoring a line here would require positing, therefore, that line 10 sloped upwards, that a line of writing was begun somewhere to the right of the present margin, and that the line presently numbered 11 was begun on the left margin immediately under the first sign of line 10 and was written on a downward slope. Is all that plausible? The only epigraphically plausible form of such a hypothesis would be that the additional line was only a partial line intended as the end of line 10, a scribal practice well illustrated by RS 24.247+; but the structure of this text is not like that of RS 24.247+, an omen text, and I do not find the idea convincing. It appears to me far more plausible to assume that the scribe allowed lines 10-12 to slope upwards (as is clearly the case with line 5) then corrected the upward slant in line 13. [*Syria* 65, *Rituels*]

— 1.50 (RS 1.023):2'. T is correct in observing both that more than two signs are to be reconstructed at the beginning of the line (*AfO*), for the same space is in the following lines occupied by three signs, and that the reconstruction {[k]sù} is grammatically unacceptable (*AuOr*), for, if {l} represents the preposition, the correct orthography would be {l ksi}. [*Rituels*]

— 1.71 (RS 5.300):2, 3. T accepts (*AuOr*) my reconstruction of {ʿwʿ} in line 2 but prefers reading {š h} to my {šš} in line 3. The latter reading must be judged very likely, however, for in my original collation (28, 29 vii 1981) of the second sign I perceived the tip of the vertical and the tips of two of the right-hand row of horizontals of {š}. These wedges are not shown on the published hand copy, and I can only apologize for not checking more carefully this copy, kindly executed by P. Bordreuil at a time when I had not yet worked up the courage to do my own. [*Hippiatriques*]

— 1.72 (RS 5.285+):21. T is correct (*AfO*) in rejecting *CAT*'s reading {árl}. [*Hippiatriques*]

— 1.78 (RS 12.061):5. T is correct (*AuOr*) in noting that the {b} of *CAT*'s {k b d m} is not certain, but he should have made the same remark with regard to the {k}, which is more plausibly {w}. [*JAOS*, *Nature*, *Rituels*]

— 1.83 (RS 16.266):3. T is incorrect (*AfO*) in asserting that the reading of {ú} is uncertain: portions of all four wedges are visible. [Pitard]<sup>16</sup>

— 1.83 (RS 16.266):4. Again T is incorrect (*AfO*) in asserting the need for the asterisk: both wedges of the first {m} are partially preserved and no other reading is possible. On the other hand, he is correct in asserting that the fifth sign should be read as {t}: though the two wedges were not placed so as to form a symmetrical six-pointed star, there certainly are two wedges and the reading {°} is out of the question. As he observes, the same correction is to be made in lines 6 and 11, each time the fifth sign, each time read as {°} in *CAT*. [Pitard]

— 1.83 (RS 16.266):8. Whatever one may want to do with the word {tán}, the second sign is certainly {á}, not {n} as T would have it (*AfO*). [Pitard]

— 1.83 (RS 16.266):11. The last word of the line is {yymm}, neither {ʿšštʿ} as in *CAT*, nor {ššm/yšm} as T proposes (*AfO*). [Pitard]

— 1.83 (RS 16.266):12. T is correct (*AfO*) that there is a word-divider after {hmlt}, incorrect to doubt the {t} in {ht}, and correct in seeing the first two signs of the last word as {yn} (the reading is in fact {ynhr}). [Pitard]

16. This is another text that I have collated (once in 1981, again with Pierre Bordreuil in 1996 at the request of W. Pitard), but do not have plans to publish. The epigraphic need for me to do so will in any case be obviated by W. Pitard's forthcoming publication of the text in *JNES*, for Pitard and I are in almost total agreement on the reading of the text (some of the readings indicated below are in fact new readings of Pitard's that I had not perceived in 1981 but that Bordreuil and I confirmed by collation in 1996). Note that the reading of this text in *KTU* was replete with errors and that few of these have been corrected in *CAT*.

— 1.83 (RS 16.266):13. T is correct (*AfO*) in observing that there are additional signs after {ltp}, but only partially correct in his reading of them (instead of {ltp<sup>r-1</sup> . m k/pt} read {ltp<sup>r-1</sup> . mk}. [Pitard]

— 1.83 (RS 16.266):14. T is correct (*AfO*) in reading the fourth sign as {r}; the sign after this {r} appears, however, to be a word-divider, which, if correct, makes his division {t<sup>m</sup> r<sup>r-1</sup>} unlikely. [Pitard]

— 1.83 (RS 16.266):15. T is correct (*AfO*) that there are traces of another line here, but in fact only one sign is visible, a certain {r} (T speaks of “*Zeichenreste*”). Be it noted in passing that there are a few signs from the reverse preserved on the lower right edge (as viewed from the *verso*), indicating that the text continued on the reverse at least almost to the preserved height of the tablet. [Pitard]

— 1.84 (RS 17.100A+B):25.<sup>17</sup> T is incorrect (*AfO*) in asserting that the first three signs of this line are all uncertain: *CAT* correctly indicates that only the first sign is so damaged that the reading is epigraphically uncertain. [*Rituels*]

— 1.90 (RS 19.013):1. T's correction (*AfO*) of *CAT*'s {id .} to {i<sup>r</sup>d<sup>r</sup> [.]} is only partially correct: the word-divider has indeed entirely disappeared, but all six wedges of the {d} are partially visible and that reading may be deemed certain. [*Rituels*]

— 1.91 (RS 19.015):26-34. T's description (*AfO*) of the segments of these lines that indicate the amounts of wine as being aligned vertically is correct, as one may see in the hand copy of the *editio princeps*. Whether editors should be held to reproducing precisely in the transliteration—as opposed to the hand copy—such details of formatting is, however, debatable, for, because of the differences between transliterated characters and characters of the original documents, it is virtually impossible always to get the spatial relationships right, as anyone knows who has ever tried to do so. In this case, it must be admitted, it would have been easy to represent the vertical relationship correctly since it was achieved on the tablet by spacing. T's reference to “*Personennamen*” at the head of these lines is certainly a *lapsus calami*, since they are geographical names. Also, T makes no reference to the horizontal line inscribed in the clay between these geographical names and the word-divider preceding the indications of amounts of wine; these were not reproduced in *CAT*. Because in administrative texts the lines of writing were often inscribed over a horizontal line in the clay, it is again virtually impossible to represent the line in transliteration; but when that horizontal line fills an empty space in a line it can be indicated in the transliteration as functioning much as do similar lines (solid, dotted ...) in modern usage, i.e., to facilitate the reader's task of properly ascribing ends of lines to beginnings of lines. For another problem with horizontal lines, see below on 1.111 (RS 24.255):1-3. [*Rituels*]

— 1.91 (RS 19.015):29. T is correct in remarking (*AuOr*) that the word *hsp*, which in *CAT* is printed on the left margin, in fact belongs to the end of the preceding line. (This type of printing error is fairly common in *CAT* and should indeed be remedied—in this case, the user can check the transliteration against the published hand copy, but in cases where hand copy and/or photograph have not been published the reader has no way of ascertaining the correct lineation.) T's tacit acceptance of the reading {k<sup>r</sup>d<sup>r</sup>m} before the word *hsp* cannot, on the other hand, go unchallenged. The trace visible on the edge of the break is the tip of a horizontal wedge on a line with the horizontal wedge of the {m}. The reading {<sup>r</sup>d<sup>r</sup>} must therefore be judged unlikely.

— 1.98 (RS 24.229):7'. T is correct (*AuOr*) that the first letter may be read {<sup>r</sup>b<sup>r</sup>} and he is equally correct in noting it with a question mark, for it consists of the upper right corner of a horizontal wedge that appears larger than that of the {b} in the preceding line; *CAT*'s reading {g} is thus not out of the question. His acceptance of the reading of {<sup>c</sup>} as certain must, on the other hand, be judged hasty: all that is left of the sign is the upper outline, for the break here follows that outline. Moreover, the angle of this outline is

17. *CAT* 1.84:25 corresponds to line 62' in my re-edition of the text to appear in *Rituels*.

not identical to that of the {<sup>c</sup>} in line 4'. {<sup>c</sup>} appears to me to be the most likely reading (the {t} in this text is *trilobé*, and the angle of this trace is not identical to that of the {t}), but it should be noted as uncertain.<sup>18</sup>

— 1.98 (RS 24.229):8'. T is correct (*AuOr*) in observing that {<sup>b</sup>'<sup>l</sup>.} is not present at the beginning of this line, but he tacitly admits *CAT*'s reading of four lines on the reverse of the tablet, including a certain {r} at the beginning of their line "11." In fact, only the traces of the first sign of two lines are visible, and neither sign is preserved completely enough to be deciphered.<sup>19</sup>

— 1.100 (RS 24.244): 4, 20. Contrary to T's assertion (*AuOr*), there are no encircled {t}s in these lines. The small segment of an arc to the upper left of the {t} in line 4 is too small to qualify as such (it is probably only a by-product of the technique of producing the {t} *trilobé*, viz., by rotation<sup>20</sup>) and neither of the {t}s in line 20 has any sort of encirclement whatever, just two somewhat awkwardly produced versions of the sign (here my copies are better reproductions of the actual forms). [*Para-mythologiques*]

— 1.100 (RS 24.244): 20. Contrary to T's assertion (*AuOr*), there is no smaller {n} inscribed immediately under the last {n} in this line. [*Para-mythologiques*]

— 1.101 (RS 24.245). T is correct (*AfO*) in observing the presence of a horizontal line at the beginning of this text, omitted in *CAT*. [*Para-mythologiques*]

— 1.103 (RS 24.247+):1. T is correct in observing (*AfO*) that the reading in *CAT* of the first sign as {Ø} deserved a note of comment. Indeed, it constitutes one of the more egregious errors preserved in *CAT* from an earlier publication.<sup>21</sup> [*AfO* 33, *JAOS*, *Rituels*]

— 1.103 (RS 24.247+):6. T correctly observes (*AuOr*) that the extraneous word-divider indicated in *CAT* as appearing in the word *m<sub>1</sub>n* is not in fact a word-divider, but a large vertical wedge over which the head of the first wedge of {n} was written. The origin and function of that vertical wedge, of which the head is lost in the lacuna, are uncertain. [*AfO* 33, *Rituels*]

— 1.103 (RS 24.247+):7. From a purely epigraphic perspective, T is correct (*AfO*) in observing that the signs on the right edge of the tablet that are ascribed in *CAT* to this line could be ascribed to line 6. Such an ascription involves, however, either moving the signs above them to line 5, something that cannot be judged likely because line 5 quite clearly ends with the last word preserved today, or else positing that *mrhy mlk tdl<sub>1</sub>n* follow upon *m<sub>1</sub>n r<sub>1</sub>m* in the preceding omen. His proposal surely, therefore, deserves comment. [*AfO* 33, *Rituels*]

— 1.103 (RS 24.247+):34'. T correctly observes (*AuOr*) that the space in the lacuna is insufficient for the restoration {bh'm<sup>1</sup>[t ib t<sub>1</sub>lq]} and he suggests restoring {bh'm<sup>1</sup>[t t<sub>1</sub>lq]}. This would imply a negative omen, for when the owners of the "cattle" are not identified, the reference is to cattle of the homeland. That interpretation is not at odds with the apparent meaning of the damaged previous section of the omen

18. I collated this tablet on 10 ii 81 but have no plans for immediate publication.

19. Here *KTU/CAT* appear to be following the *editio princeps* (A. Herdner, *Ugaritica* VII, Paris 1978, 68-69), rather than supplying the readings of the tablet itself. The editor seems to have mistaken two casts from the same fragment as representing two fragments. As proof of that surmise, I offer the following remarks: (1) the beginnings of lines of her fragment "B" correspond precisely to the first signs of lines 2-6 on fragment "A" —note particularly the small horizontal wedge above the {w} in B:3, which is not to be seen on her copy of A:4, but which is in fact above the letter {w} in RS 24.229:4'; (2) the inventory of the twenty-fourth campaign deposited in Damascus indicates only a single fragment for RS 24.229; (3) when Bordreuil and I were preparing *TEO*, we found only one fragment bearing the number RS 24.229 (see p. 299).

20. My attempt to reproduce this sign in my hand copy in *Para-mythologiques* (p. 198 and, by Murphy's law, reproduced on the cover of the book) was not particularly successful: the sign does indeed consist of three "lobes" with the arc segment joining two of them.

21. The reading was already present in M. Dietrich, O. Loretz, *Mantik in Ugarit. Keilalphabetische Texte der Opferschau-Omensammlungen-Nekromantie* (mit Beiträgen von H.W. Duerbeck, J.-W. Meyer, W.C. Seitter) (*Abhandlungen zur Literatur Alt-Syrien-Palästinas* 3), Münster 1990, 92 (n. 24), 102, and was criticized in my review of that volume, *JAOS* 113 (1993) 615.

apodosis, but T does not note that from it one must infer that *CAT*'s reading {b<sup>o</sup>lhn} in the same line should be {b<sup>o</sup>l hn}, i.e., *hn* is not the pronominal suffix referring to an unknown feminine entity (always a dubious interpretation), but the presentative particle. [I have not proposed a reconstruction of this line in either *AfO* 33 or *Rituels*]

— 1.103 (RS 24.247+):46'-47', 49', 51', 55'. T correctly observes (*AuOr*) that too few signs are restored at the beginnings of these lines in *CAT*; the same must be said, however, of lines 48' and 54'. [*Rituels*]

— 1.103 (RS 24.247+):46'. T is incorrect (*AuOr*) in accepting that a word-divider has been preserved at the beginning of the line. [*AfO* 33, *Rituels*]

— 1.103 (RS 24.247+):51'. T is incorrect (*AuOr*) in accepting that the {b} at the beginning of the line is uncertain: the tablet is preserved at the lower left corner of the sign and there is no trace of the wedges necessary for {d}. [*AfO* 33, *Rituels*]<sup>22</sup>

— 1.104 (RS 24.248):19. Contrary to T's assertion (*AfO*), the presence of two small vertical "incisions" ("*Einstiche*") before the {h} is at best dubious. I have never noted them in any of my collations and on my photograph I see only damage. [*Rituels*]

— 1.104 (RS 24.248):21. T correctly observes (*AfO*) that there is no word-divider after {wm}. [*Rituels*]

— 1.105 (RS 24.249). T correctly observes (*AfO*) that the notes in *CAT* arguing that the text "has to be understood as a scribal exercise"<sup>23</sup> were written before the *recto/verso* orientation of the tablet was reversed (as compared with *KTU* and the *editio princeps*); the new orientation was that of the transcription that I sent to the authors of *CAT* in 1994 (see above, note 15). [*Rituels*]

— 1.106 (RS 24.250):14. T correctly observes (*AfO*) that the deletion in *CAT* of {ʃ<sup>o</sup> .} before {nkl} is correct. On the other hand, he accepts that the {m} of the preceding word is epigraphically dubious, which is not the case. [*Rituels*]

— 1.107 (RS 24.251). T is correct (*AfO* [at 1.101]) in observing the presence of a horizontal line at the beginning of this text, omitted in *CAT*. [*Para-mythologiques*]

— 1.109 (RS 24.253):5. T is correct (*AuOr*) in reading {ʃrt} for *CAT*'s {ʃrt}, probably a simple typographical error. [*BSOAS* 58, *Rituels*]

— 1.109 (RS 24.253):12. T's proposal (*AfO*) to read, i.e., not emend, {kkdm} as {kb!dm} may only be described as epigraphic fantasy: the second sign is a perfectly normal {k}, with nothing "hybrid" about it. [*BSOAS* 58, *Rituels*]

— 1.109 (RS 24.253):21. T's acceptance (*AfO*) of the presence of a word-divider and a {š} (the latter of which he marks as damaged, *CAT* as complete) at the end of the line is incorrect (see above, introductory remarks). [*Syria* 69, *BSOAS* 58, *AfO* 42-43, *JSS*, *Rituels*]

— 1.111 (RS 24.255). T rightly observes (*AuOr*) that this text is incorrectly identified in *CAT* simply as "Hurrian." [*Bilinguisme*, *Rituels*]

— 1.111 (RS 24.255):1-3. T correctly saw (*AuOr*) on his photographs that there is a horizontal line below {m y<sup>o</sup>rb}, between lines 2 and 3. What he did not mention was that the line was inscribed all across the tablet and that its function was plausibly intended to be identical to that of the following horizontal lines, i.e., that of a divider between lines of writing, even though the scribe inscribed the tops of the signs of most of line 3 on the horizontal line, thus effacing it except between letters. That being the case, it appears proper

22. Two corrections to my 1986 transliteration of this line have been made in *Rituels*: there is indeed a word-divider after {bh} and there is not one after {yspù}. Both situations are correctly represented in *CAT*, as they were in *Mantik*.

23. I find this ascription highly implausible.



to note this line in transliteration as a full dividing line between lines 2 and 3 (on the general problem, especially in administrative texts, see above, remark to *CAT* 1.91:26-34). In turn, that being the case, it appears proper to provide the same notation between lines 1 and 2, for there also a horizontal tracing preceded the inscription of the signs of line 2, the tops of which in this case were written on the horizontal line along its entire length.<sup>24</sup> Whatever notation one adopts for these two tracings, the one above line 1 was certainly left unscathed by the inscription of that line, but it is noted neither in *CAT* nor by T. It appears clear to me that the scribe intended to use line-dividers everywhere on the *recto* of the tablet, not abandoning the usage until line 16 on the *verso*. [*Bilinguisme, Rituels*]<sup>25</sup>

— 1.111 (RS 24.255):19 (20). T is correct (*AuOr*) in denying the presence of a word-divider at the end of this line (the reading is new in *CAT* as compared with *KTU*,<sup>26</sup> perhaps the result of mistaking on a photograph the word-divider at the end of line 6, which arrives here on the *verso*, for one belonging to this line). [*Bilinguisme, Rituels*]

— 1.114 (RS 24.258):7-8. T is correct (*AfO*) in observing the presence of a partial horizontal line between these two lines of writing. He does not, however, mention a possible relationship between this tracing and the few small signs also written between these two lines. In my re-edition of the text, I ascribed to these signs the line number "7a" and placed the partial horizontal line between lines "7" and "7a." [*Para-mythologiques*]

— 1.114 (RS 24.258):15. T correctly observes (*AuOr*) that the space between {k'} and {i} allows for the restoration of only two full signs. He might also have referred to the presence of the tip of a small vertical wedge in the right edge of the break, which I interpreted as the word-divider.<sup>27</sup> One might give the authors of *CAT*, who indicate {xxx} in the break, the benefit of the doubt and assume that they were referring to the two signs that I restored plus the word-divider that I copied and read. [*Para-mythologiques*]

— 1.114 (RS 24.258):31. T is correct (*AfO*) in reading the second-last sign as {à} rather than {n}. [*Para-mythologiques*]

— 1.116 (RS 24.261):2. Though I would need to check the tablet to be certain, I cannot presently accept T's new reading {gm't'} (*AuOr*) for the generally accepted {gm}: I have never perceived the trace

24. Could it be these traces of the horizontal line mingled with the still quite clear signs that have led to such diversity of readings ever since the *editio princeps*? The line clearly reads {tlt ymm . l lğz y'rb} and the recent attempt to see instead of {ğz} the signs {ym} (M. Dietrich, W. Mayer, "Sprache und Kultur der Hurriter in Ugarit," in: M. Dietrich-O. Loretz, eds., *Ugarit. Ein ostmediterranes Kulturzentrum im Alten Orient. Ergebnisse und Perspektiven der Forschung*. Band I *Ugarit und seine altorientalische Umwelt [Abhandlungen zur Literatur Alt-Syrien-Palästinas 7/1]*, Münster 1995, 7-42, esp. p. 17; adopted by Dietrich and Loretz in their list of "New Readings" in *Word-List of the Cuneiform Alphabetic Texts from Ugarit, Ras Ibn Hani and Other Places [KTU: second, enlarged edition] [Abhandlungen zur Literatur Alt-Syrien-Palästinas und Mesopotamiens 12]*, Münster 1996, 226) is no more successful than its several predecessors.

25. Quite by oversight I omitted the horizontal line above line 13 from my transliteration in *Bilinguisme*. Not by oversight was my omission of a horizontal line after line 12 and of a line 13 on the *recto*. In *CAT*, both are indicated, though line 13 is indicated as erased; no horizontal line is indicated at the beginning of the *verso*; and the first line on the *verso* is numbered as "14." In fact, both the horizontal line after line 12 and the signs inscribed below that line were rubbed out by the scribe. He took up his text again on the *verso* after tracing a horizontal line across the top. Because the effaced signs on the *recto* seem to be identical to those that begin the first line on the *verso*, one may surmise either that the scribe changed his mind about inscribing line 13 on the *recto* (perhaps to provide space between the events of one day and the next—though that is not the usual practice of scribes) or else that he committed a dittography and decided to retain the line as inscribed on the *verso*. In any case, it appears improper to number the erased line on the *recto* as part of the text since the scribe also erased the horizontal tracing above it and clearly did not, therefore, wish to retain that line.

26. I know of the reading previous to *CAT* only in a transliteration of lines 16-23 (17-24) proposed by J. C. de Moor in *UF* 11 (1979) 652.

27. Dietrich and Loretz now present {ăšk[r]} as a "new reading" here (*Word-List* [1996] 226).

of a letter there when collating the tablet, and the angle of writing of the {n} is not favorable to the reading. That letter was written slanting downward and one might expect a following {t} to have been written at the same angle; but the tablet is well preserved to the immediate right of this {n} and any {t} would have to have been written higher on the tablet and at an upward angle for the present lacuna to have removed it, even partially.<sup>28</sup> [*Bilinguisme, Rituels*]

— 1.116 (RS 24.261):25. T is correct in observing (*AuOr*) that there was only one sign before {hr̄tt}, incorrect in not perceiving that this sign is partially preserved. The authors of *CAT* register the partially preserved sign but reconstruct another one before it. The line is to be read { 't'hr̄tt}, "for the THR (pl.)." [*Rituels*]

— 1.119 (RS 24.266). T chides (*AfO*) the authors of *CAT* for not pointing out in the introductory section under "Ge[nre]," where this text is classified as "ritual, list (sacrifices)," that it contains a prayer (lines 26'-36'). That is certainly true, but there is no reason to doubt that the prayer was part of the ritual. [*Verse, Rituels*]

— 1.119 (RS 24.266):18'-25'. T correctly observes (*AuOr*) that the authors of *CAT* have overlooked the dividing lines between lines 18-24, incorrect in that he has himself missed the one between lines 24 and 25 (it has been partially effaced when line 25 was inscribed, but, as in 1.111:1-3, one can doubt neither the presence nor the function of the line). [*Verse, Rituels*]

— 1.123 (RS 24.271). T fails to include this text in his list (*AfO* [at 1.101]) of texts in which horizontal tracings that serve as line-dividers are not indicated in *CAT*. There is in fact a line traced between each line of writing in RS 24.271 except between lines 1-2, lines 4-5 and between the first two lines on the *verso* (lines 17'-18'). There is also a line traced above line 1. Here, as in other cases, the writing has at times been partially effaced by signs, but there can be no doubt that the scribe intended these lines as separations between lines, not as tracings on which to inscribe the line. [*Rituels*]

— 1.123 (RS 24.271):1. T properly accepts (*AfO*) the correction of { . il} in *KTU* to {âb} in *CAT*. He does not note that the text in *KTU* was perhaps a typographical error rather than a false reading, at least as regards {il} for {âb}, since the authors had given the correct reading of those two signs one year earlier.<sup>29</sup> He also accepts the absence of a reconstruction at the beginning of the line, where they now indicate {[xx(.)]} instead of {šlm}; the correct notation here, however, is {--r̄-1}, for one cannot rule out the possibility of three signs in the lacuna. Finally, he accepts the reading of the last sign as { 'm' }; had the sign been {m}, however, one might expect the tip of the vertical wedge to have been visible where the break curves off to the right. The reading here of {t}, followed or not by other signs, cannot be ruled out. [*Rituels*]

— 1.130 (RS 24.284). T rightly criticizes (*AfO*) *CAT* for not accepting the new *recto-verso* orientation of this text that was first proposed in print by M. Dijkstra<sup>30</sup> (and which was reflected in the

28. Similar considerations weaken the "new reading" here of {šrdm} (*ibid.*; the reading is credited to M. Dietrich and W. Mayer in a forthcoming publication in *Subartu* 4): there is absolutely no trace of the vertical wedges of {d} along the well-preserved upper edge of the {n}, and the likelihood of {m} appearing after this {n} must be judged even slimmer than in the case of {t} because the vertical wedge of {m} should descend lower than {t} and should have been visible in the well-preserved expanse of surface to the right of the {n}. As for the first sign, the reading {š} may be said to be out of the question; {s}, on the other hand, would at least be a reasonable proposal, for the general outline of that sign is similar to that of {g}, but I have never perceived the presence of more than one wedge when collating the tablet and do not see more today on my photograph.

29. Dietrich, Loretz, and Sanmartín, *UF* 7 (1975) 542: {âb}.

30. *UF* 16 (1984) 75. The same proposal was made independently by my research assistant Donna Freilich sometime in the years 1983-1985 when she was preparing preliminary interpretations of the ritual texts I had collated in 1980-1981. I must confess that I remained sceptical about the reversal until I compared RS 24.284 closely with RS 24.253 (see *BSOAS* 58 [1995] 235, n. 12; 239-40).

transliteration that I sent to the authors of *CAT* on 10 viii 94). [*BSOAS* 58, *Rituels*]

— 1.130 (RS 24.284):22 (9). T also rightly criticizes (*AfO*) *CAT* for the reading {il<i>ʿbʿ} in line 22 (line 9 in *KTU/CAT*), preferring the notation {ilʿiʿ[b]}. Only below, however, in his note to the abecedary *CAT* 5.19 (RS 23.492):4, does he indicate the epigraphic basis for his reading, viz., that the scribe of both texts placed the vertical wedge of the {i} before the horizontals. The observation is almost certainly correct in both cases; but one may doubt that the scribe of the two texts was the same, for there are certain differences between the two, not the least of which is the size of the vertical wedge of {i}: as big as a {g} in RS 24.284, more like a word-divider in RS 23.492.<sup>31</sup> If this wedge was as big in the second case of {i} in RS 24.284:22 as in the first case, the reading {ilʿiʿ[b]} must be preferred, for one may perceive in the break the left edge of a vertical wedge, with no sign of the lower wedge of {b}. [*BSOAS* 58,<sup>32</sup> *Rituels*]

— 1.133 (RS 24.293). T is correct (*AfO* [at 1.101]) in observing the presence of a horizontal line at the beginning of this text, omitted in *CAT*. [*Para-mythologiques*]

— 1.133 (RS 24.293):2. T correctly denies (*AfO*) the presence of {m} at the end of the line and credits my edition. [*Para-mythologiques*]

— 1.133 (RS 24.293):5. T correctly asserts (*AfO*) the presence of a word-divider after {áhr}. [*Para-mythologiques*]

— 1.133 (RS 24.293):18. T correctly considers (*AfO*) the {b} of *ábn* to be more damaged than the {á}: the large crack before the {b} could conceivably have removed the first wedges of {d}, whereas the two partially-preserved wedges of the preceding sign may not be read as anything but {á}. [*Para-mythologiques*]

— 1.148 (RS 24.643):10. T does no better (*AfO*) than *CAT* in his attempt to read the middle section of this line. Indeed, this example of preferring readings from photographs over readings from the original is almost as striking as that of RS 24.253:21 (see above at 1.109 and introduction); in this case I have described the traces in some detail and provided a hand copy based on my study of the tablet itself.<sup>33</sup> It is simply impossible to read/restore *álp* after *ágn*: (1) space is insufficient; (2) traces of three wedges of {b} are extant, including, of course, one of the verticals, and there is simply no way to construe these traces as a {p}; (3) in the break to the left of this sign, the tips of all three wedges of {š} are visible and may not be ignored, as do T and the authors of *CAT*. A fourth argument may be cited, which, though not epigraphic in nature, fits the epigraphic data and may be mentioned here: in this list of sacrifices, as in the list on the *verso*, only manifestations of *Baʿlu* receive “a bull and a ram” (*álp w š*) and no divinity receives just “a bull” (*álp*). After the {ʿbʿ}, there is a very large wedge of which the left side is vertically oriented. T wishes to call it an over-sized word-divider, but since certain {ʿ}s in this text have precisely this form, I preferred to read that sign here. That decision still appears to be necessary, for the {ʿ} that T reads between the following {b} and {l} may only be ascribed to wishful thinking, as there is certainly no sign there (T refers to “ein kleiner Keil, der als {ʿ} zu lesen ist”). It appears to me that the epigraphic data require that we

31. Indeed, in his edition of RS 23.492 (*Semitica* 32 [1982] 9-10), Bordreuil took the small vertical wedge for a divider between the first twenty-seven letters of the alphabet and the last three which are of secondary origin. The fact that the following {i} would consist of only three wedges makes the interpretation of the vertical followed by three horizontals as a complete {i} the more plausible, however.

32. My reading {ghlʿ-ʿ} on p. 236 is the strictly epigraphic one in terms of the standard Ugaritic sign forms, while my translation on p. 237 (“ʿluʿi{b}i”) reflects the interpretation of the wedges explicitated above. Unfortunately, there are no other well-preserved {i}s in RS 24.284 by which to verify this interpretation of the wedges, and I know of no other text where {i} is so written (except the abecedary to which reference is made in the text).

33. *Syria* 69 (1992), p. 159, 163.

recognize here the presence of a faulty text: the scribe wrote {b<sup>o</sup>bl} for {b<sup>o</sup>l}. [*Syria* 69,<sup>34</sup> *Rituels*]

— 1.148 (RS 24.643):23. T correctly points out (*AuOr*) the presence of a horizontal line above this line, the first of the *verso*; since he has been somewhat inflexible in matters of lines, it is unclear why he says nothing about the vertical line that divides the *verso* from top to bottom slightly to the right of middle. Because there is no clear relationship between this line and the text as preserved on the tablet (i.e., some lines of writing extend beyond this tracing and there is no preserved head of a line that was placed to the right of it), its meaning for the scribe is uncertain. [*Syria* 69, *Rituels*]

— 1.161 (RS 34.126):32. It is debatable whether the length of the {á} is a factor in determining whether it should be emended (*AfO*) to {t} or {n}: did the scribe think of {t} as shorter than {n} or simply as consisting of fewer wedges? [*Syria* 59, *Bibliothèque, Rituels*]

— 1.162 (RS [Varia 20]):3. T is correct (*AfO*) that there is no word-divider after {b}.

— 1.162 (RS [Varia 20]):4. T is incorrect (*AfO*) in denying to the vertical wedge in this line the function of a word-divider. Though this scribe did not use the word-divider elsewhere, the presence of a vertical wedge between the two principal lists of offerings, the *šrp*-offerings and the *šlmm*-offerings, can only be seen as a purposeful division between words, inserted because that division also constitutes a division between sections of the text and of the liturgy reflected in the text. [*Semitica, Rituels*]

— 1.162 (RS [Varia 20]):5. T is correct (*AfO*) in asserting that {mw} in the middle of the line are partially preserved. [*Semitica, Rituels*]

— 1.162 (RS [Varia 20]):11. T is correct (*AfO*) in asserting that {š} at the end of the line is partially preserved. [*Semitica, Rituels*]

— 1.162 (RS [Varia 20]):16. T is correct (*AfO*) in asserting that there is no doubt concerning the reading of {i}. [*Semitica, Rituels*]

— 1.162 (RS [Varia 20]):20. T is correct (*AfO*) in asserting that the central word in this line is *pámt* and that there is no doubt regarding the reading of the {á}. [*Semitica, Rituels*]

— 1.163 (RIH 78/14). T is kind enough (*AfO*) to refer to my brief refutation of *CAT*'s version of this text<sup>35</sup> as “*Erwähnenswert*,” but does not himself propose any readings. [*JAOS, JSS, Rituels*]

— 1.168 (RIH 77/10B+):21-25. T points out (*AfO*) that the organization of the text—not to mention, one might add, the readings—are here very different from those in the *editio princeps*. The new ordering of the lines as well as most of the specific readings were in the transcription that I sent to the authors on 10 viii 94.

— 2.14 (RS [Varia 4]):3. T asserts (*AuOr*) that the first word is {bny} rather than {bry}. Though, because the text is in a private collection, I have never collated it personally, I see no reason to doubt P. Bordreuil's explicit assertion that the second sign is {n}.<sup>36</sup>

— 2.16 (RS 15.008):12. T rightly points out (*AfO*) that the correct reading of the verb in this line is {twḥln}, with only an outside chance that the second sign is {r}—there is no chance whatever that it is {d} (*CAT*: “*d* looks like *r*”). [*AfO* 31, *UF* 19, *BSOAS* 50, *Épistolaires*]

34. While preparing these remarks, I noticed for the first time a typographical error in this article: on p. 162 the last two signs in line 9 of the reconstructed text should be enclosed by full brackets, rather than by half brackets. Tropper says nothing in either of his articles about the reading of {š<sup>o</sup>[rp]}, absolutely essential for understanding the structure of this text (*Syria* 69 [1992] 161-62), but ignored by the authors of *CAT* (cf. *JSS* 42 [1997] 136-37). I hope the typographical error did not contribute to his not coming to terms with the reading, which should have been clear from the primary transliteration (p. 156) and from the hand copy (p. 159).

35. The readings in *CAT* were present in *Mantik* (ref. above, note 21); they were criticized in my review of that book (*JAOS* 113 [1993] 615-16) and have since been mentioned again in my review of *CAT* (*JSS* 42 [1997] 135-36). The detailed presentation of my readings, accompanied by a hand copy, will appear in *Rituels*.

36. *Semitica* 32 (1982) 8.

— 2.17 (RS 15.098). Without himself taking a position, T criticizes (*AfO*) *CAT* for not noting that the *recto/verso* orientation of this tablet is in doubt. There are in fact excellent reasons to follow M. Dijkstra's proposal<sup>37</sup> to reverse the order proposed in the *editio princeps* and followed in *CAT*. [*Épistolaires*]

— 2.21 (RS 15.174):17. T is right to query (*AuOr*) *CAT*'s reading of the last word of this line ({*ât<sup>3</sup>nx*}), but his own proposal ({*ât<sup>3</sup>nx*} or {*tt<sup>3</sup>nx*}) does not reflect what is on the tablet. The reading is difficult, for the tablet was burned and the wedges are still partially clogged with hardened black soot; moreover, the writing continued, as it did on the preceding line, around the corner of the lower edge onto the right edge, where it was squeezed in between the end of line 16 and the sharp angle of the tablet where the right edge becomes the *verso*. Line 17 reads {*w . l . t<sup>3</sup>nn t<sup>3</sup>nn*}, "and you must give it". I mark the first sign of the verbal form with half-brackets because the head of the single horizontal wedge is damaged, but in point of fact there is little chance that the sign was {*â*} for the lacuna appears too small for the first wedge of {*â*} ever to have been there. The upper left corner of the word-divider is preserved; it marked the division between the verb and the objective pronominal suffix, a common practice when the suffix consists of two or three consonants. [*Épistolaires*]

— 2.31 (RS 16.394). I have no light to shed on the incorrect numbering, pointed out by T (*AfO*), of lines 35 and following in *CAT* (the line numbers of the transliteration that I sent to the authors on 10 viii 94 diverged from those in *CAT* beginning with line 20). [*Épistolaires*]

— 2.32 (RS 16.401):4. The authors of *CAT* propose that the word-divider between {*îšâl*} and {*hm*} be removed by emendation, T (*AuOr*) that it be retained (regarding the use of the word-divider in such situations, cf. remark above to 2.21:17). In point of fact, the scribe already did the job for *CAT*: the word-divider that once was inscribed there has been flattened severely, surely as an attempt at erasure. [*Épistolaires*]

— 2.38 (RS 18.031):21. We have here another example of a reading based on collation that "ist nicht möglich," whereas one based on examination of photographs is preferred. This portion of the tablet is badly swollen and cracked from the heat of the fire that destroyed the city, and deciphering it from the original is difficult enough; in any case, it is T's reading that is (partially) impossible. T criticizes (*AuOr*) *CAT*'s reading {*k<sup>3</sup>lklhm*}, preferring {*w<sup>3</sup> [ . k<sup>3</sup> ] t<sup>3</sup>kl<sup>3</sup>h<sup>3</sup>m*}. T is correct that the first sign is {*w*} (all four wedges are partially visible and {*k*} is out of the question), incorrect in attempting to read two signs between the word-divider (which is partially preserved) and {*kl*}. In fact, the upper left edge of a sign consisting of at least two horizontal wedges is preserved after the word-divider, which may in theory be read as {*â, n, k, w, r*}, but there is simply no room between these traces and the {*k*} to place a {*l*}: there are about 7 mm between the word-divider and the {*k*}, where T would place {*kl*}, whereas the space between {*k*} and {*h*} further down the line, occupied by {*l*} alone, measures nearly 8 mm. T apparently did not use the ruler in this case that allowed him in other instances to make very astute remarks regarding available space. In any case, T represents incorrectly the visible traces, for he would reconstruct a {*k*} between the word-divider and his first {*l*} and he presents that {*l*} as partially visible. To my mind, {*w t<sup>3</sup> . â<sup>3</sup>klhm*} is the only plausible reading of the line as preserved. [*Monde, Épistolaires*]

— 2.39 (RS 18.038):31. T would correct (*AfO*) {*ib<sup>3</sup>ltn . [ ]*} in *CAT* to {*ib . ltn . â<sup>3</sup>[ ]*}. The first correction is highly dubious, for the word-divider was erased in antiquity and anyone wishing to maintain it thereby ignores the express wish of the scribe or an ancient corrector of the text. The second is a matter of method: the editor placed the {*â*} in his transliteration on the basis of a field photograph,<sup>38</sup> it is not

37. *UF* 19 (1987) 37-38.

38. Ch. Viroilleaud, *Textes en cunéiformes alphabétiques des Archives Sud, Sud-Ouest et du Petit Palais (Palais royal d'Ugarit V)*, Paris 1965, 86; the photograph, with the {*â*} clearly visible, was published later: *Ugaritica* IV, Paris 1962, 59, fig. 41; *Ugaritica* V, Paris 1968, 723, fig. 41 (a photograph of the cast, made after the sign had disappeared, is published on the

indicated on his own copy, executed after the sign had disappeared. [*UF* 13, *Épistolaires*]

— 2.41 (RS 18.075):15'.<sup>39</sup> T would correct (*AfO*) {ʿyʿrš} in *CAT* to {ʿrʿš}. I have only perceived the tip of a vertical wedge and have not seen anything that would allow a definitive decision between the two readings. [*Épistolaires*]

— 2.44 (RS 18.134):3. T is incorrect (*AuOr*) in criticizing *CAT*'s reading of {ʿṭḥm} and preferring {<ṭḥm}: the scribe at first forgot the {t}, but then inscribed it on the left edge of the tablet, where subsequent damage has made it difficult to spot.<sup>40</sup> [*Épistolaires*]

— 2.44 (RS 18.134):4-5. T is incorrect (*AuOr*) in all instances where he claims damage not represented in *CAT*: sufficient diagnostic features of all the purportedly damaged signs (four in T's estimation) remain for the readings to be epigraphically certain. On the other hand, both *CAT* and T have misread the beginning of line 5 and T has misestimated the space available in the break: *CAT* indicates {tgrk . tšlmk}, a reading taken over from *KTU*, corrected by T to {tgr.k . [t]ʿšʿlmk}. The line in fact reads {tgr . tšlmk}, i.e., the scribe omitted the pronominal suffix on the first verb (*CAT*'s notation should, therefore, have been {tgr<k>}). The sign after the word-divider is indubitably {t}, not {k}, and T's proposal to restore {t} in the following lacuna is in any case absolutely eliminated by considerations of space—as it is, the tip of the {t} that he reads as {k} would virtually have touched the {š}. Another case of T forgetting his ruler and of allowing the desire for a “correct” text to influence his epigraphy. [*Épistolaires*]

— 2.46 (RS 18.147):4. T is incorrect (*AfO*) in asserting that the {l} of *ilm* has entirely disappeared: the tips of two of the three wedges are visible. [*Épistolaires*]

— 2.46 (RS 18.147):8. T is correct (*AfO*) in one correction of *CAT*, incorrect in three others: the {n} is indeed sufficiently damaged to be epigraphically uncertain (though the traces in context make the reading certain); on the other hand, the {l} is epigraphically certain because all three tips are visible, the following word-divider has entirely disappeared, and the {g} can be no other sign. Both are wrong in that they represent the {š} as partially preserved when in fact no copiable trace remains (though the break does follow the V-shaped outline of that sign). [*Épistolaires*]

— 2.46 (RS 18.147):14. As the top wedge of the first row of verticals of {y} is preserved, there can be no doubt about the reading, in agreement with *CAT*, against T (*AfO*).<sup>41</sup> [*Épistolaires*]

— 2.47 (RS 18.148):5. T is incorrect (*AuOr*) in claiming that the sign after {tškn} is a word-divider: it is certainly a {n} and *CAT*'s indication of damage—which T fails to cite—is not even necessary. [*Épistolaires*]

— 2.47 (RS 18.148):17.<sup>42</sup> T is correct (*AfO*) that a horizontal line separates this line from the following one. [*Épistolaires*]

— 2.65 (RS 19.158B):2-5. T is correct (*AuOr*) that there is no text missing at the beginning of these

following page of *Ugaritica* V).

39. This line will be numbered “16” in my edition (the authors of *CAT* missed a line on the *recto*).

40. The notation {ʿṭḥm}, superior to {[t]ḥm} indicated in *KTU*, was present in the transliteration that I sent to the authors of *CAT* in 1994 (see above, note 15).

41. T's notation is “{y\*m.}.” According to his system, the absence of a space between the {m} and the {y} should indicate that *ym* and the following signs form a single word. In the absence of an explanation on T's part, it is impossible to know if the absence of a space was intentional or a typographical error. I, in any case, do not see what {ym.yšrʿ[...]} would mean as a single word.

42. This line will be numbered “20” in my edition: the lower right corner fragment that the editor indicated for the *verso* only (beginning with the {w} after a break in *PRU* V [ref. note 38], p. 88) joins to the principal fragment thereby permitting the reconstruction of the total number of lines on the original tablet.

lines. He should also have pointed out that the left margin of line 1<sup>43</sup> is incorrectly indicated in *CAT*: the {y} is not situated above the first letter in the following line as in *CAT*, but three signs to the right, i.e., above the word-divider that follows the first word, and some three signs may therefore be reconstructed before this letter. Thus the difficulty that line "1" would appear in *CAT* to begin with {y} disappears. As to the actual readings, T is incorrect in claiming that the {t} in line "2" has disappeared (the tip of one of the wedges is preserved), correct that there is no doubt about the reading of {n} in the following line, incorrect in restoring {wr} at the beginning of line "5" (there is room only for {r}), incorrect in indicating {g} without doubt (only the upper right corner is preserved, which could in theory belong to any vertical wedge), correct that the {m} may be indicated as epigraphically certain (both wedges are partially preserved and there can be no doubt about the reading), and correct that both {t}s may be indicated as certain (the upper half of each is preserved and again there can be no doubt about the readings). Both *CAT* and T have missed the traces of two wedges of a {l} following the word-divider. [*Épistolaires*]

— 2.72 (RS 34.124). The unusual system of line-numbering adopted for this text in *CAT* and duly pointed out by T (*AfO*) is that of the definitive edition of the text by Bordreuil/Pardee (*Bibliothèque*) and a correction of Pardee's original reconstruction (*BiOr*).

— 2.82 (RIH 78/12):7. It is debatable whether the authors of *CAT* should have been required to note "{s/link}" as T claims (*AuOr*), for {s} is the probable reading. [*AfO* 31, *Épistolaires*]

— 3.1 (RS 11.772+). Though I have only collated this tablet very summarily, I really must react to T's assertion (*AuOr*) that its genre should be described as epistolary. In spite of a recent attempt to prove the epistolary character of this text,<sup>44</sup> there is not a single element in "der ersten Zeilen" that is certainly indicative of epistolary character: the words in lines 2' (*m*) and 4' (*šps*) appear in all genres. Indeed, there are counter-indications: Knoppers takes *mgy* in line 3' and *ql* in line 5' as indications that the document is a letter, but anyone who has spent any time at all with Ugaritic letters knows that the epistolary prostration formula is invariably expressed in the first person, either singular (*qlt*, "I have fallen") or dual (*qlny*, "we two have fallen"); and anyone who has studied this formula knows that the verb *MGY* never precedes it. On the other hand, there is nothing in the text as preserved to preclude its interpretation as an historical prologue to the imposition of tribute expressly described in lines 16' and following. There is no particular reason to expect that this historical prologue should be expressed identically to those known from Akkadian versions—indeed there is no proof that the historical situation that gave rise to this document was identical to one or the other of those behind the known Akkadian texts.<sup>45</sup>

— 4.729 (RS 24.301):12. T's reading (*AuOr*) of {'k'} at the end of the line can be judged no more likely than that of {'š'} in *CAT*; indeed, the one time I collated this text, I thought I saw three tips of wedges in the break.<sup>46</sup>

— 5.19 (RS 23.492):4 (*AfO*). See above, remark on 1.130:22 (9).

— 7.41 (RS 1.009[A]):5'. T observes correctly (*AfO*) that the sign after the word-divider is {t}, not {m}: what the authors of *KTU/CAT* took for the vertical wedge of {m} is too far to the left and in any case

43. In fact line 2': at least one partially preserved sign is visible above the horizontal line that was inscribed above the first fully preserved signs.

44. Gary N. Knoppers, "Treaty, Tribute List, or Diplomatic Letter: *KTU* 3.1 Reexamined," *BASOR* 289 (1993) 81-94.

45. For a discussion of some of the parallels, with previous bibliography, see W. van Soldt, *UF* 22 (1990) 341-45, 354-57. As van Soldt shows, by the order of mention of the two principal textiles, viz., *phm* – *ūqnū*, the Ugaritic text resembles the text written under Muršili II, not those written under Šupiluliuma I, as is usually assumed.

46. Our re-edition of the administrative texts has only begun, and its completion is too far off to be cited even as "in preparation."

only a break-line.<sup>47</sup> [*BSOAS* 58, *Rituels*]

By a very rough calculation, Tropper is correct in about half of his epigraphic remarks that I have been able to compare with the results of my collations. Though some fourscore corrections of *CAT* (counting only those that I have been able to verify) certainly constitute an important number, and Tropper is to be congratulated for his perspicacity in proposing these corrections, I wonder whether this result is worth his time and effort. It must not be forgotten that there were also some fourscore incorrect proposals, and the wheat could be separated from the chaff only by someone who had consulted the originals. I, for my part, must conclude that also only about half of the proposals made with regard to the texts that I have not collated are correct. Furthermore, I, like all other Ugaritologists, have no way of knowing in most cases, without undertaking the collation personally, which proposals are correct and which are incorrect.<sup>48</sup> If only Tropper could be persuaded to devote what is obviously a very sharp pair of eyes to the study of the tablets themselves! Viewing positively the studies that I have just examined critically, the initiative and perspicacity shown there bode well for the author's future in epigraphy. Moreover, we may all be grateful for the excellence of his contributions not only to Ugaritic grammar, but to the epigraphy and grammar of the Zinjirli Aramaic and Phoenician inscriptions, as well as to the other Semitic languages and in comparative Semitics. I wish to stress that the criticisms voiced in the above remarks should not be taken *ad hominem* but as directed against the method that consists of doing Ugaritic epigraphy from photographs, of which the two articles criticized are but tokens.

#### Abbreviations

<i>AfO</i>	J. Tropper, Review, <i>AfO</i> 42-43 (1995-96) 264-274.
<i>AfO</i> 31	D. Pardee, "Further Studies in Ugaritic Epistolography", <i>AfO</i> 31 (1984) 213-230.
<i>AfO</i> 33	D. Pardee, "Ugaritic: The Ugaritic <i>šumma izbu</i> Text", <i>AfO</i> 33 (1986) 117-147.
<i>AfO</i> 42-43	D. Pardee, Review, <i>AfO</i> 42-43 (1995-96) 274-277.
<i>AuOr</i>	J. Tropper, "Epigraphische Anmerkungen zur Neuauflage von <i>KTU</i> ", <i>AuOr</i> 13 (1995) 231-239.
<i>Bibliothèque</i>	P. Bordreuil, D. Pardee, "Les textes ougaritiques", in: P. Bordreuil, ed., <i>Une bibliothèque au sud de la ville (Ras Shamra-Ougarit VII)</i> , Paris 1991, 139-172.
<i>BiOr</i>	D. Pardee, "A New Ugaritic Letter", <i>BiOr</i> 34 (1977) 3-20.
<i>Bilinguisme</i>	D. Pardee, "L'ougaritique et le hourrite dans les textes rituels de Ras Shamra-Ougarit", <i>Mosaïque de langues, mosaïque culturelle. Le bilinguisme dans le Proche-Orient ancien. Actes de la Table-Ronde du 18 Novembre 1995 organisée par l'URA 1062 "Etudes Sémitiques" (Antiquités Sémitiques 1)</i> , Paris 1996, 63-80.
<i>BSOAS</i> 50	D. Pardee, R. Whiting, "Aspects of Epistolary Verbal Usage in Ugaritic and Akkadian", <i>BSOAS</i> 50 (1987) 1-31.
<i>BSOAS</i> 58	D. Pardee, "RS 1.009 ( <i>CTA</i> 36, <i>KTU</i> 1.46): Reconstructing a Ugaritic Ritual", <i>BSOAS</i> 58 (1995) 229-242.
<i>CAT</i>	M. Dietrich, O. Loretz, and J. Sanmartín, <i>The Cuneiform Alphabetic Texts from Ugarit</i> ,

47. Herdner correctly read {t} (*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 1963), text 36a.

48. On this problem of unverifiability for most users of an edition of texts, see my review of *CAT* (*JSS* 42 [1997] 133-34, 137).



- Ras Ibn Hani and Other Places (KTU: second, enlarged edition) (Abhandlungen zur Literatur Alt-Syrien-Palästinas und Mesopotamiens 8)*, Münster 1995.
- Épistolaires* D. Pardee, *Les textes épistolaires (Ras Shamra-Ougarit)*, Paris (in preparation).<sup>49</sup>
- Hippiatriques* D. Pardee, *Les textes hippiatriques (Ras Shamra-Ougarit II)*, Paris 1985.<sup>50</sup>
- JAOS* D. Pardee, Review, *JAOS* 113 (1993) 614-617.
- JSS* D. Pardee, Review, *JSS* 42 (1997) 132-137.
- KTU* M. Dietrich, O. Loretz, and J. Sanmartín, *Die keilalphabetischen Texte aus Ugarit einschließlich der keilalphabetischen Texte außerhalb Ugarits. Teil I Transkription (Alter Orient und Altes Testament 24/1)*, Neukirchen-Vluyn 1976.
- Leslau* D. Pardee, "The Structure of RS 1.002", in: A.S. Kaye, ed., *Semitic Studies in Honor of Wolf Leslau on the Occasion of his Eighty-fifth Birthday November 14th, 1991*, Wiesbaden 1991, vol. II, pp. 1181-1196.
- Monde* D. Pardee, "La vie sur les tablettes", *Le Monde de la Bible* 48 (1987) 29-31.
- Nature* D. Pardee, N. Swerdlow, "Not the Earliest Solar Eclipse", *Nature* 363/6428 (3 June 1993) 406.
- Para-mythologiques* D. Pardee, *Les textes para-mythologiques de la 24<sup>e</sup> campagne (1961) (Ras Shamra-Ougarit IV)*, Paris 1988.<sup>51</sup>
- Pitard* W. T. Pitard, "KTU 1.83, The Binding of Yamm: A New Edition of the Text", *JNES* forthcoming.
- Ritual* D. Pardee, "RS 1.005 and the Identification of the *grm*," in: J. Quaegebeur, ed., *Ritual and Sacrifice in the Ancient Near East. Proceedings of the International Conference Organized by the Katholieke Universiteit Leuven from the 17th to the 20th of April 1991 (Orientalia Lovaniensia Analecta 55)*, Leuven 1993, 301-318.
- Rituels* D. Pardee, *Les textes rituels (Ras Shamra-Ougarit)*, Paris (forthcoming).<sup>52</sup>
- Semitica* P. Bordreuil, D. Pardee, "Textes ougaritiques oubliés et 'transfuges'", *Semitica* 41-42 (1991-1992) 23-58.
- Syria 59* P. Bordreuil, D. Pardee, "Le rituel funéraire ougaritique RS. 34.126", *Syria* 59 (1982) 121-128.
- Syria 65* D. Pardee, "Troisième réassemblage de RS 1.019", *Syria* 65 (1988) 173-191.
- Syria 69* D. Pardee, "RS 24.643: Texte et Structure", *Syria* 69 (1992) 153-170.
- TEO* P. Bordreuil, D. Pardee, *La trouvaille épigraphique de l'Ougarit (Ras Shamra-Ougarit V/1)*, Paris 1989.
- Trial Cut* D. Pardee, *Ugaritic and Hebrew Poetic Parallelism. A Trial Cut (‘nt I and Proverbs 2) (Supplements to Vetus Testamentum 39)*, Leiden 1988.

49. It should be noted with regard to these texts that, though Pierre Bordreuil and I cooperate on various aspects of our publications in matters Ugaritic, *Les textes épistolaires* will appear under my name and will not be a joint publication as is indicated consistently in *CAT*, contrary to the information provided with the transliterations sent to the authors in 1994 (see above, note 15). Properly acknowledging cooperation and collaboration between Bordreuil and myself (the subject of a letter to the authors of *CAT* dated 8 September 1994) may not be deemed a sufficient basis for assigning joint authorship of an edition, as appears automatically to have been done in *CAT* for all the texts of which I sent a transliteration to the authors (even for those for which no further re-edition is intended—see two following notes).

50. It should be noted that these texts will not be republished by Bordreuil and Pardee, as is indicated for each of them in *CAT*. No such indication was present in the transliterations sent to the authors of *CAT* in 1994 (see above, note 15) and no reason is evident for why they should have extrapolated this indication from the same indication for other texts.

51. Neither will these texts be republished by Bordreuil and Pardee.

52. Neither will these texts be published by Bordreuil and myself (see note 49).

- UF* 13            D. Pardee, "A Further Note on *PRU* V, No. 60, Epigraphic in Nature", *UF* 13 (1981) 151-156.
- UF* 19            D. Pardee, "Epigraphic and Philological Notes", *UF* 19 (1987) 199-217.
- Verse*            D. Pardee, "Poetry in Ugaritic Ritual Texts", in: J.C. de Moor, W.G.E. Watson, eds., *Verse in Ancient Near Eastern Prose (Alter Orient und Altes Testament 42)*, Neukirchen-Vluyn 1993, 207-218.