Once Again on the Ugaritic Ritual Texts.
II. Pardee's so-called "monumental blunders"

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[This article answers the retorts by D. Pardee to the criticisms I put forward in my review article of his work on the Ugaritic cultic texts. Pardee described some of the criticisms as "monumental blunders". As can be seen, his comments are for the most part based either on distortions of the meaning of the criticisms or on irrelevant and subjective subtleties that avoid the main points at issue. The answers are ordered according to the sequential numbering in KTU. For a fuller understanding of these remarks, the reader should have at hand both dOL's review and P.'s reply].

In his increasingly bad-tempered and blunt retort (P.)[1], Prof. D. Pardee imputes me with the commission of a series of "monumental blunders" in my review of his work on the Ugaritic cultic texts (dOL)[2]. Leaving aside the flow of accusations concerning my qualifications in that retort[3], let us now turn our attention to the texts themselves, in order to clarify my interpretations and verify to which extent such an imputation may be right. I will follow the chronological order in which the texts are presented in Pardee's work[4], in an attempt to answer the different problems they may raise and in order to ease

1. See D. Pardee, "G. del Olmo Lete's View on Ugaritic Epigraphy and Religion", *UF* 37, 2005, 767-815. It begins in a temperate and reasonable tone, but from p. 771 on his argumentation is blurred by the aggressivity of his personal use of adjectives and offensive appraisals ("off-the-cuff", "naïve", "willy-nilly readings", "masoretic" (!); fanatical: "if these photographs had been published, dOL would have certainly criticized their quality" [p. 777]; "The second (view) … reveals his antipathy to any results achieved by P." [779]; "affected his critical faculties" [779]; "his basic assumption is that CAT is correct and that any modification that I propose is incorrect"); and by a sort of blindness due to his self-conscious pride as an epigraphist that, in my opinion, prevents him from understanding what he is reading. For example, in p. 772 he indulges in a series of (three) obvious remarks and ignores the main proposed point ("It is the meaning which has to lead the reading"; see in this regard Kaufman's opinion on modern epigraphists, quoted in *UF* 36, 2006, 543 n. 15). Immediately afterwards he attributes to me repeatedly and with an aggressive denial of my qualifications (deciding what I and others have or have not done) the meaning "marge/bord", which I have never held, in an authentic and protracted exercise of "tilting at windmills" (see *AuOr* 24, 2006, 269).

2. G. del Olmo Lete, "The Ugaritic Ritual Texts. A New Edition and Commentary. A Critical Assessment", *UF* 36, 2004, 539-648; D. Pardee, *Les textes rituels. Fascicules 1-2* (Ras Shamra-Ougarit XII), Paris 2000.

3. See in this regard G. del Olmo Lete, "Once Again on the Ugaritic Ritual Texts. I. On D. Pardee's Epigraphy and Other Methodological Issues", *AuOr* 24, 2006, 265-274 (265-267).

4. P. decides to organise his retort according to thematic categories (P. 768) from the more objective (epigraphy) to the more subjective (history of religions (!)). But in fact, afterwards he does not follow this order, as can be seen from the headings under which the texts are set out. After the "monumental blunders" (4½ pages) and a general appraisal of my incompetence (19 pages), the retort is distributed under different headings: "misrepresentations" (13 pages), "epigraphy" (2 pages), "new readings …" (½ page), "astounding assertions" (5½ pages), with a final half-page on the characterisation of the ritual texts. He himself recognises

consultation by the reader interested in those texts while reaching a greater degree of objectivity in their overall interpretation.

Nevertheless, let us first glance at the texts from which such “monumental blunders” stem according to P.

KTU 1.39:17/RS 1.001:17 (P. 779f./dOL 556f.): P.’s retort is a simple repetition of his justification of the reading ⸢š⸣*rp*, which is also in KTU, but without going into my argumentation (see also *AuOr* 24, 2006, 270-271). The retort is completely useless and irrelevant, since I have manifested my readiness to accept the correctness of the reading. It is perhaps unacceptable to presume a scribal error/oversight in a Ugaritic text when there are ‘textual’ and ‘contextual’ reasons to sustain it, mostly when dealing with a reading “presque certain” = “almost/virtually certain”, finally simply “certain” (*TR* 780), “epigraphically and contextually probable”? It seems as if both scribe and epigrapher were demanding infallibility in unison. Of course, this is my “view of what the text should say”, as is always the case when a scribal error is presumed, just as the restorations and reconstruction P. proposes are his view of what the texts should say[5]. The great difference is that I propose my opinion as a working hypothesis to interpret the “text”, whereas P. forces it to fit his untouchable reading of the “sign”[6]. The ‘monument character’ of the error arises then only from P.’s inability to see a little beyond his epigraphic strictness[7]. (On this text see also below).

KTU 1.41:19/RS 1.003:19 (P. 781/dOL 568): Given the strict textual quasi-identity of KTU 1.41 and 1.87, as copies of the same text, in my opinion and that of many others, Pardee’s proposed reconstruction/reading {[tr]⸢mt⸣[m^{c}rb]}, based as always on his epigraphic interpretation of fragmentary signs of a broken text, is very improbable. Besides the textual support just quoted, the space left in the tablet seems too large to be filled by the two signs suggested alone and, moreover, the *hapax trmt* (KTU 1.43:3) has little to recommend it in this context according to P.’s own appreciation of this kind of lexeme. ‘Improbable’ implies certainly that it is possible, but less so. In this situation, in any case, when dealing with a colleague’s opinion, ‘fantaisiste’ is an adjective that could easily be replaced by another that is more polite, or at least neutral. But perhaps that is too much to ask of P.

KTU 1.65:9/RS 4.474:9 (P. 781/dOL 590): The reading *add* is such a common form in the whole NWS area[8] that it could easily be presumed also at Ugarit in a litany as a hypercorrect form of /ad/[9]; P. himself

that “the arrangements of the headings is somewhat at random”(!) (p. 792). This reply follows the page order of my review article that in fact is the order of P.’s own text edition, namely, the chronological order/numbering of the discovery of the texts.

5. Or should we speak of emendation? See n. 22.

6. This systematic strictness has a reflex in P.’s inability to ascertain the ‘metaphorical’ use of a saying (see p. 785 n. 52), concluding as if to me the text/letter were of no importance …

7. At least in this case it cannot be imputed that “his (dOL’s) basic assumption is that CAT (KTU) is correct and that any modification that I propose is incorrect” (P. 779). Nor that “It is only dOL’s view of what the text should say and his ill will towards my work in general that allows him to reject this reading” (P. 780f.; see above n. 1). This is an imputation of *parti pris* or fanatical enmity, radically opposed to any honest intellectual activity, which I have never experienced. Nor can I understand how an educated scholar in due command of his emotional equilibrium was able to word it. To introduce this kind of argumentation into an academic discussion must be labelled unsuitable and even ‘childish’. The inaccuracy of this imputation can be easily ascertained by looking at the large amount of P.’s proposals and readings that I accepted and enhanced, sometimes even emending my previous views. Alas! I do not have the self-confidence in my work that P. exhibits (see P. 770, n. 7) in his infallible achievements and method. This is how I ended my review article: “Beyond this discrepancy I am also very grateful to P. for the data and suggestions I found acceptable and that will aid me to improve my own analysis of these texts…” (*UF* 36, 2004, 648).

8. See for instance /adda:u/ in J.-M. Durand, “Les théonymes des textes de Mari”, *Annexe* to his contribution “La religion amorrite en Syrie à l’époque des archives de Mari”, in the collective work *Mythologie et Religion des Sémites Occidentaux*, vol. I, Leuven 2008, p. 633; H.B. Huffmon, *Amorite Personal Names in the Mari Texts*, Baltimore MD 1965, p. 20; for Emar see D.E. Fleming, *Time at Emar* …, Winona Lake IN 2000, p. 334 (index); R. Pruzsinszky, *Die Personennamen der Texte aus Emar*, Bethesda MD 2003, pp. 103ff., 185; and in general for the different transcriptions and normalisations (Haddu/Addu/Adad …) see

sometimes accepts a *hapax* in these texts. In any case, again my textual solution is preferable, I believe, to the strange reading and interpretation proposed by P. ("le dieu qui se lève"). It splits the clear sequence of divine names (with no attributes) and introduces a meaning for /ndd/ that is not certified either textually or lexicographically elsewhere, P.'s opinion[10] notwithstanding (may we call the whole exegesis of this line 'fantaisiste'?). How strict is P. in reading 'damaged' signs and how bold in contriving textual hypotheses - in this case, finding here the exaltation of the instruments of El the creator! (dOL 591). Once again, we have a conflict between the epigraphy of fragmentary signs and textual coherence. In principle, I am always in favour of the second alternative, until a clear-cut textual witness settles the problem.

KTU 1.109:21/RS 24.253:21 (P. 781f./dOL 608f.): this is a splendid case of P.'s biased way of reading other scholars' writings, seemingly obsessed by his intransigent polemical attitude of seeing adversaries everywhere and of taking advantage to insist in their overall lack of qualifications. So he assumes my denial of his reading of line 21, when this was precisely the reading singled out as significant and accepted, as is the case when P.'s readings are singled out in my review, at least as a respectable hypothesis. In this case the absence of any comment meant that I had no objection to it, and in fact considered KTU's reading, which I had previously accepted, to be a "mistaken reading". On the contrary, he interprets this absence as "symptomatic of his (dOL's) intransigence in epigraphic matters". But to accept this reading of my review would imply that P. is ready to renounce his obsession that "his (dOL's) basic assumption is that CAT (KTU) is correct and that any modification that I propose is incorrect" (P. 779). Also, that it is "only dOL's view of what the text should say and his ill will towards my work in general that allows him to reject the reading", as was said above[11]. Then the rest of his comments are completely out of place, having nothing to do with my review article. But P. takes the opportunity to criticise my assuming KTU's reading against Herdner's suggestion in other works. First, the assumption of creating "a new deity at Ugarit" in this way is nonsense, because *il* t^c*ḏr* will always be *il* t^c*ḏr* b^c*l*; on the other hand there are lists with variants or that are simply unique. Secondly, I was perfectly conscious of the reading (cf. *CR* 274 n. 56, not quoted by Pardee), but if nevertheless I preferred the haplography, it was for internal textual reasons not to be excluded altogether[12]. So my acceptance of KTU's reading was neither acritical nor pernicious[13], but simply reasonable, although perhaps mistaken. Thirdly, to quote KTU 1.162:12 here is sign of unmethodical reasoning and in no way supposes the presumed "two minds". This text presents no problem in its restoration and was known to many including myself long before B.-P.'s edition. Fourthly, the errors detected in the *DUL* entry are, alas!, correct and the authors promise the great master and father to give not only some but a great deal more attention in the transcription of this and the many other mistaken texts. In this regard, we thank Prof. L. Kogan, who along with his critical

D. Schwemer, *Die Wettergottgestalten* …, Wiesbaden 2001, *passim*. KTU 1.70:17 would simply be a reflex of this use (see P. 374 n. 59)

9. A PN such as *adb*^{c}*l* (DUL 16) is ambiguous; see later n. 41.

10. The alleged parallelism (//*qm*) is fictitious in my opinion. In fact, the /ndd/ forms always occur in syntagmatic constructions and their function, like that of *qm* in this position, is "inchoative", with the parallelism between the main verbs; see *DUL* 620f.

11. See n. 1. P. complains so strongly about my supposed "sarcastic treatment" of his opinions, but has nothing to say about his own "caustic" comments which he does not feel obliged to explain, considering such adjective to be an exaggerated interpretation. …

12. The new edition of n. 56 in *CR*[2] reads: "On a former reading (Herdner's) of *š* at the end of this line, which already Dijkstra, *UF* 16 (1984) 74, considers as superfluous, see Pardee, *TR*, p. 602; Del Olmo Lete, *URT*, p. 602. Excluded such reading, the recipients of these *šlmm*-offerings would be reduced to six, against standard practice; in this connexion to assume haplography should not be completely excluded; cf. below, n. 64".

13. On the perniciousness of following KTU, see *AuOr* 24, 2006, 267. I hope P.'s work will not be accepted uncritically in the future, either.

assessment of the dictionary[14] sent a complete list of misprints and faulty transcriptions, which we appreciate greatly. At the same time, at the end of his review he asserted: "the errors mentioned are not crucial and can be easily eliminated …", as is almost always the case with printing errors and imperfect readings. On the contrary, in his review of *DUL* in *JNES*[15], P. only mentioned a few errors, and not the ones pointed out here. Does it mean that P. did not pay enough attention to his duty as a reviewer? Of course, it was easier to criticise the alphabetic order used in *DUL*[16].

KTU 1.109:37/RS 24.253:37 (P. 782/dOL 608f): P.'s retort is a clear reflex of the extreme confidence he has in his (internal) model of the *ductus* of the tablet. But this confidence has sometimes turned out to be fallacious and has even been given up by P. himself (see *AuOr* 24, 2006, 271 on RS 94:2490)[17]. In addition, here the contextual meaning favours Virolleaud's reading. "I suppose that this is a case where the epigraphic evidence is rather subjective and interpretative". In any case, P.'s proposal (whereas he has nothing to say about my lexicographical criticism of it) is extremely improbable.

KTU 1.130:5/RS 24.284:5 (P. 783/dOL 625): This is also a clear case of what I call epigraphy "aux marges" of very dubious bearing and on which it is complete useless to waste time. There is only room for speculations on epigraphic debris (see above) and even my own proposal has no special guarantee. The resort to the hypothetical readings /kṯr/, /kṯrt/ is based only on the reconstruction of a known DN beginning with /k-/, not on any special attested sequence. The proposal aims at overcoming the contextual void in which P. leaves us; he suggests no alternative.

KTU 1.148:9/RS 24.643:9 (P. 783/dOL 630): Here the retort is just a lament for my inconsiderate behaviour at not mentioning his guess of a /š/ back in 1992[18], reduced by P. himself to a note and not included either in the transcription of the text or in the epigraphical remarks. Evidently, looking at the copy (P. 1291), the trace is so faint that his proposal is based more on contextual than on epigraphical reasons; until the appearance of the new text, P.'s proposal had no sufficient guarantee for it except for the contextual sense. In any case, my review, already too long, intended to record the disagreements to be criticised, taken for granted that on the rest I have no serious objection. In this case, a newly discovered text confirms the reading beyond any doubt: that is the kind of confirmation I am looking for. I confess myself guilty of not having quoted this 'genial' guess at the time. The reading is duly adopted in the new edition of *CR* and a note of recognition of its paternity will be added there.

KTU 1.164:10/RIH 77/02B:10 (P. 783f./dOL 635): As usual, P. reads and quotes what he likes; in this case the last phrase of my criticism, omitting any comment on the first, where the hermeneutical leap, so frequent in P.'s commentary, from possibility-to probability-to certainty is recorded. His retort detracts nothing from the probability of the divergent reading, which in fact has no special bearing on the

14. See *AuOr* 24, 2006, 135-140.

15. See *JNES* 65 2006, 232-234. Had P. read the prologue (xi-xii) attentively, he would have found out earlier the importance the authors gave to lexicographical isoglosses and the reason why they did not supply the semantic values for each language.

16. See *AuOr* 24, 2006, 145-148.

17. See also dOL 585 n. 147.

18. The concern for priority is obvious; it seems as if it were a psychological need for acknowledgement, even when it is not foreseen; see also P. 768 n. 3. My suggestion of vocalisation of the /yqtl/ Ugaritic names as an optional north-west Semitic transference of the Akkadian jussive, following a suggestion by Schwemer, has no relationship whatever with P.'s phonological-orthographical argumentation. The names transcribed with and without word-divider were discussed there. So, I am sorry, there in no "conversion to his view"; perhaps the "blurred" character of the references induced this misrepresentation. In any case, I would have had no problem in acknowledging P.'s good guess were that the case. By the way, P.'s reference seems to be incorrect, since in my quoted "Glosas Ugaríticas I" (*AuOr* 16, 2001, 295-299) nothing is said in this connexion. Surely he is referring to "Glosas ugaríticas III", *AuOr* 21, 2003, 145 n. 11.

interpretation of the text. The discussion in my quoted note is merely contextual, not epigraphical, and other possible readings are also suggested.

KTU 1.163/RS 78/14 (P. 784/dOL 637): Unlike P., I do not find myself qualified to decide whether the authors of KTU ever collated the tablet and have not spied on their movements in this connexion[19]. This kind of personal appraisal is quite intolerable; and my following its reading is not a "masoretic" acceptance or rejection of any other text edition. For me their epigraphical treatment was a sufficient guarantee in order to prepare a commentary, not a new textual edition[20]. If this behaviour is "sans critique", it may mean that a considerable amount of ancient Near Eastern studies are "sans critique" since they rely on previous textual editions.[21] But all that sounds childish in an academic discussion[22]. In this regard and as far as the quoted remarks go, P.'s reconstruction of the history of the edition of the tablet is completely irrelevant.[23]

But the *Conclusions* give the key to P.'s incredible, almost irrational, temperamental reaction. It was intended to confirm "that he (dOL) –an 'obtuse reviewer', see P. 792 n. 71, beyond being an incompetent epigrapher– has no standing to offer an assessment of epigraphic method or results … has no idea of the problems faced by Ugaritic epigraphers", implying, of course, that those epigraphers are embodied in D. Pardee and his art, on which, then, I completely agree. But to this and similar denials of my qualifications I have already answered in the first instalment of this retort, as well as to his assessment of my hermeneutics[24]. It is not worthwhile repeating them (*AuOr* 24, 2006, 269, 272-273) and it is better not to take them seriously. According to my own answers to all the points commented on above, I would conclude that the "monumental" dimension P. has found in the supposed "blunders" is due rather to a

19. It is incredible to speak of a 'mythical visit' (for which the tablet "reconstituted itself magically") of the authors of KTU, those patient editors of this retort, so exhibiting superior magnanimity in the face of the bad-tempered manners, so offensive to the host, shown by P. Are we reverting to the quarrel of 1990, or has that been superseded with sincerity?

20. On these detractions of personal qualifications, see already *AuOr* 24, 2006, 265-268.

21. A new collation or autopsy is always advisable, even unavoidable, when there are serious and well-founded suspicions of inadequate previous collations. But when a text has been edited and re-edited, after enduring and overcoming a vitriolic charge of incompetence, it may reasonably be credited as more adequate than previous editions. By the way, in tablets with only one column, it is usual to number *recto-verso* faces as columns (I/II). Was the Bordreuil-Caquot edition of KTU 1.163 "masoretic" for P.?

22. By the way and as a datum for P. to include in his spying on colleagues' movements: I should let it be known that the reprint of *CR* (2004) was just that, a mechanical reprint of the 1999 edition, of which I was completely unaware (I only learned about it from the publisher's catalogue), so that in no way can its date be used to trace my responsibility on taking some readings as repeated or not new. This fact was due to an unconscious oversight for which no publisher can be blamed. On the contrary. the publishers, both good friends of mine, deserve my recognition and I must even confess that I felt flattered by the speed with which they coped with a work that was out-of-print.

23. It was dictated apparently by his anger because due recognition of his superb merits in the reading of those texts had been omitted and by his fury against the acceptance of KTU as a reliable text edition, in fact his "bête noire" ("… but there are times when the truth must be told…", P. 785). That anybody should dare to give credit to this rival edition against which his entire editorial program was set up (see *AuOr* 24, 2006, 272) is unbearable to him. A positive result of this angry attack has been my personal and direct involvement in the reading and photographing of all the disputed readings and edges during my recent visits to Damascus and Paris. The results are presented in the second edition of *CR* now at the proof-reading stage.

24. He is unaware that the *parti pris* of his confessed model of two separate sets of texts and gods (P. 791) prevents him from seeing the 'hints' pointing in another direction: the strongest implication of the ancestor cult in the other kind of cult, which is, let us say, 'theological'. I do not look for 'hints' in order to found a thesis, I find those hints and try to incorporate them into a coherent and unified vision. The ancestor cult does not exclude the cult of personal gods, just as it not possible to separate the cult of Baal, provider of rain, from that of Baal, fighter against Mot.

visual distortion into which his obsession about the objectivity of his opinions and methods leads him so as not to accept any dissent[25].

But let us leave this more subjective level and get down to the analysis of the rest of the retorts that are not in the category of "monumental blunders". From the very beginning, one feels much affected facing 13 pages of misrepresentations (!), a proof for P. "indicative of the quality of the review as representative of the positions put forward in the work under review" (P. 793). To my surprise, I have found out a new facet in P.'s intellectual achievement: his exacerbated punctiliousness verging on the most extreme 'talmudism'. I confess that early on, I tired of this game of *Mäusemelker* and was on the point of giving it up, but little by little, it became entertaining as an exercise in mental fencing. In the following pages, I will collect all P.'s critical remarks, made under the headings "Misrepresentations", "Epigraphy", and "Astounding assertions", into a unified series in the order of the texts in *TR*., as stated above[26].

In general, the specific remarks on "Epigraphy" (P. 806f.), "the most objective level of analysis" (P. 768), are rather inconsistent, as could be ascertained by my comments. The quotations express only generally my personal appraisal of the epigraphical task carried out by P., with which he obviously cannot agree. In any case, I am surprised to see so few of my remarks on epigraphy objected to by P. Of course, he does not mention the great many of his readings that are positively recorded, even if they are not decisive for the understanding of the texts.

On the "new readings" in *KTU*/*TR* (pp. 807f.) and their authorship I will say nothing. The question belongs to the relationship linking P. with the authors of KTU, whose work was my reference source. Once more, P.'s despising attitude towards them is patent when he posed the question concerning their interpretation of KTU 1.161: "why did the authors of *KTU* botch so badly their reading thereof" (P. 807). Here the exact dates of publication of both works and also that of *RC*/*CR* must be taken into account, which made it impossible for one work to be consulted by the other. I do not want to jeopardize the pardean priority, even paternity, of these readings. When I propose a reading, it does not necessarily mean that I am the first to do so. I am rather unconcerned about priorities, a thing that obviously interests P., so fond of his own epigraphic efforts and results.

KTU 1.39:

Line 1 (*ṯ*^{c}) (P. 793/dOL 551): Irrelevant clarification because the case depends on my changing of mind on the meaning of /ṯ^c/ as either a sacrificial term or a royal title, not on whether the sacrifice is votive or expiatory. There is no sibylline or critical remark concerning P.'s position in the statement. In this case, as so often, P is fighting phantoms or tilting at windmills.

Line 1 (on the enclitic –*m* in /b^{c}l, b^{c}lm …/) (P. 808/dOL 548): See my forthcoming paper[27]. On the other hand, P.'s quoted criticism of Tropper (*b^{c}lm* = *b-^{c}lm*, "on the following day") refers to KTU 1.148 not to 1.39, to which my comment applies, and has nothing to do with the morph under discussion. This jumble is really astounding.

Lines 3-8 (**etymology of Thakamuna-Shanuma**) (P. 793/dOL 551): Once again irrelevant. In fact, in *UF* 20, P. shows himself in some way more in favour of the Cassite origin of Thakamuna, at least as can be deduced from such phrases as: "Nous proposons, en somme, d'identifier *Ṯukamuna* serait identique (sic) au dieu cassite *Šuqamuna* …" (*UF* 20, 1988, 199), although a hypothetical Semitic origin could be

25. Above all coming from an "obtuse" reviewer, unless one enters into his way of seeing things. Nevertheless, he believes "it could not be further from the truth" (P. 785, n. 54).

26. For the abbreviations "P." and "dOL", see above nn. 1 and 2.

27. "The postpositions in Semitic: the case of enclitic –*m* (with special attention to NWS)", delivered at the *II Symposium on Comparative Semitics* (Sitges-Barcelona), May-June 2006), to appear in *AuOr* 26, 2008.

invoked ("seraient d'origine ouest-sémitique"). I granted more significance to the first assessment than to the second hypothesis. On the other hand, the paragraph from which the quotation has been taken is not transcribed in *TR* 41, which made me believe in some change in favour of the Semitic character of the name. In no case is this a crime or academic sin, except for P., to whom a change of mind seems rather difficult to assume, taking into account the confidence in the work and method on which he bases his opinions (P. 770 n. 7). At most, the misinterpretation would concern the article in *UF*, read some time earlier and not checked against *TR*.

Line 8 (***urm***) (P. 793/dOL 554): Once again an almost talmudic subtlety. If P. numbers *three* sacrifices ("*šrp* …, *šlmm* … le coeur preparé d'une façon especiale") that implies that *urm* is one of those three, although perhaps as a part of the *šlmm*. Were it a type or just a sub-species of another type, it would be irrelevant for the structuration of the text, which is the question at issue here. See below on KTU 1.119.

Line 12 (structuring pattern) (P. 793/dOL 554)**:** Incredible punctiliousness ("is organised according to … the pattern" # "la structure ressemble à …"). In fact he uses the pattern mentioned in organising the text (see "Structure du text", *TR*, p. 21).

Line 12 (***šapšu*'s function**) (P. 793f./dOL 555f. n. 56): Incredible: first, to distinguish the function of a deity from that of her hypostasis (the hypostasis is precisely a specification of her functions; the function of a hypostasis is a function of the main character) is unacceptable, nor in any case is this distinction explained in the text. Second, at least P. must accept that his formulation in *TR* 68, n. 264, although in anticipation, was not the correct one: to attribute such a function to *Šapšu* without any explanation means to equate her with her hypostasis, which would be quite correct in the general opinion. In the third place, from where does P. deduce that the function of *Šapšu pagri* (unknown in myth and ritual) is to carry corpses to the netherworld? Does this retort mean that he accepts the rest of the criticism in n. 56?

Line 12 (**funerary character**) (P. 794/dOL 558): Irrelevant and too punctilious. It is clear *by implication* that for P. also the text is funerary (in my sense) in one way or the other, whether funerary or mortuary (the distinction is irrelevant in the long run), being nocturnal, chthonic, with the presence of psychopompic and mortuary deities. What else could it be? His interpretative 'materialism' hinders him from paying attention to my resort to implicit semantics.

Line 17 (epigraphy) (P. 806/dOL 557): The photograph (P. 814-815) shows nothing that contradicts the presence of an erasure, above all contrasting the blurred reproduction and interpretation with the actual and good original photograph in *MU*. But even accepting a clear sign shape, it does not affect my way of dealing with the text (see dOL 270; *UF* 36, 2004, 556f.).

Lines 20-23 (***kmm***) (P. 794/dOL 561 n. 77)**:** Pure attack of susceptibility. Nobody, whether P. or *anyone else*, can resort to *kmm* for a precise or simply probable reckoning of the amount of sacrificial victims: it is not sure that duplication of sacrifices always took place or that the correspondence between *šrp* and *šlmm* is always exact, as can be ascertained in some cases of explicit duplication.

Lines 20-23 (**sacrifice of two birds)** (P. 794/dOL 561): Although does P. not mention the Hittite custom of sacrificing birds to the "infernal deities", either on p. 87 or in n. 356 (only the type: sacrifice of birds), I have to admit nevertheless that he does so cursorily on the top of p. 88 (end of p. 87) ("en rapport…dans la culture hittite avec des divinités infernales"). The question is that from this evidence he draws no implications for the characterisation of the text.

Lines 20-23 (***Šapšu Pagri***) (P. 794/dOL 562): It is not the kind of connexion (of course it is indirect but at least it is lexical!) that is at issue, but the arguments P. puts forward to deny it. Once again, his reading here is selective and *non ad rem*.

KTU 1.40:
Line 3 and par. (on *ulp*) (P. 809/dOL 564f.)**:** My unawareness becomes immeasurable![28] But I am not interested in presenting all the opinions, even those of my venerable masters, above all when I do not follow them, even if they "had some knowledge of the Semitic languages". This P.'s astonishment is really astounding.
KTU 1.41:
Line 6 (**haplography of *šmn***) (P. 794/dOL 569): Since the text of KTU 1.87:7 is defective, there is no reason to imply or deny a haplography in it. The argument of unseemliness then is invalid. Why accept it in one copy and not in the other? I do not know to what extent one tablet is an exact copy of the other. To include the case of KTU 1.87 here was out of place in order to argue "le bien fondé de la haplographie" in KTU 1.41.
Line 6 (epigraphy) (P. 806/dOL 569, n. 104): Irrelevant: the length of the list does not detract from its internal sequence(s). So there is no "imposition of the author's structural notions" here.
Line 18 (on *ᶜntm*) (P. 809/dOL 580): To object to the implied use of the enclitic *–m* here does not imply its ignorance or negation in other texts. The three examples quoted by P. are in fact just the same (DN list) and of a specific syntactic structure (enumeration) for which he proposes a different meaning (see above).
Line 19 (on *dkr*) (P. 809f./dOL 580): Although I do not take KTU 1.86 to be a cultic text, P. does, so the lexical testimony must be valid for him, as nevertheless it is also for me. We are dealing with a general lexical determination belonging to the common lexicon. The remark is then completely out of place. I am unable to discover the astounding nature of my opinion.
Line 20 (*bt mlk*) (P. 794/dOL 570): I said "he seems to accept", in fact *bt mlk* is "une indication locale" in p. 170 and according to the "Structure du texte", p. 152, to which we are referred, lines 19-22 ("au palais du roi") contain one (B) of the "indications topographiques" of this structure. I felt authorised to propose this possibility. I do not see how I was mistaken.
Line 22 (*ġr*) (P. 794f./dOL 571): This place is expressly set in connexion with "farine", one of the "deux principaux productes agricoles". That and the possibility of being a location outside the wall suggest that it could have been imagined in connexion with a fertility rite in the fields. At least this is one of the images that emerges from P.'s interpretation and location of the *ġr*. Perhaps now P. is afraid of this deductive image.
Line 45 (*rgm yṯṯb*) (P. 795/dOL 573): I do not attribute to P. the word "prophetic", but only its meaning as opposed to "cultic" (oracle) in order to explain what I understand by the oracular function of the king.
Line 47 (*ṣbu špš*) (P. 795/dOL 574, n. 118): I cannot understand this punctiliousness of P., because the word in question was precisely *tgh* not *ṣbu*. Perhaps this was an oversight.
Line 47 (on *ṣbu špš*) (P. 809/dOL 574 n. 118): Modern terminology coincides precisely with the sun's course in ancient mentality, according to which the day begins in the morning/dawn/rising. The Semitic division of the day was the opposite and my analysis is based on that. "To march forth/on" is sufficiently neutral as to be able to signify the sun rising or setting. For the sun, both paths have similar significance.
Line 48 (*bym ḥdṯ*) (P. 795/dOL 574): Completely irrelevant and trivial. Bearing in mind that the evening is the beginning of the day in the Levant, in contrast to our usage, a 'day' may be seen as the end of one date and the beginning of another, without discontinuity. The aim was to translate the French term "charnière", i.e. "point of transition" (Sp. "bisagra").

28. Cf. *DUL* 63, 658, where the Hebrew use of /ph/ is recorded. By the way, the Hebrew use of *l-py* supposes the grammaticalisation of the lexeme with loss of its original nominal seme; as a functor it refers to action not to speech: "according to …" (See *HALOT* 915f.).

Line 48 (new intercalary month) (P. 795/dOL 574): Irrelevant. The intercalary month would have had a name and this would be the name applied to the new menology in its heading. Of course, the following reference is superfluous: it fits my interpretation, not his. On the month beginning with *š*[see dOL 271.
Line 54 (mh[) (P. 795/dOL 575): The resort to vocalisation does not usually replace morphological analysis in other cases in P.'s treatment of the texts. Where is the misrepresentation here?
KTU 1.43:
Line 9 (*g*<u>*t*</u>*r*(*m*)) (P. 795/dOL 578): Unacceptable mystification: in fact, P. considers *g*<u>*t*</u>*r* to be a DN, irrespective of who he might be, not a simple common denomination, as his listing with *Šapšu* and *Yarḫu* implies. It is curious that this is the only point he objects to in the whole criticism on his interpretation of the text. It is easy to understand his unease in this case, where the illogicality is so great.
Line 9 (on the astral identification of *g*<u>*t*</u>*r*) (P. 795/dOL 579, n. 130): The two following entries are nominalistic and useless. They do not contradict my own assessment.
Line 19 (on structure) (P. 814/dOL 580): "Inadequate" does not mean "false" (P. 814). I prefer to reserve the label "structure" for creative genres, for records and similarly I prefer to talk of surface organisation according to performance parameters (see *CR* 15f.). But this is largely a question of terminology. See below (p. 22) on my opinion and analysis of the literary structure.
Line 24 (*p*ᶜ*nm*) (P. 795/dOL 581): An elementary knowledge of the semantic shifts of the meaning of the lexemes in any language makes P.'s remark dispensable.
KTU 1.46:
Line 5 (*b* <u>*t*</u>*l*<u>*t*</u> *tmrm*) (P. 806/dOL 582): It is merely trivial, in my opinion, to define the presentation in this case as "philological" instead of "epigraphic" (P. 806).
Line 16 (*kddm*) (P. 795/dOL 583): Incredible non sequitur: "The" = "this/one", although it is not the principal or only argument according to P. It is really annoying to have to respond to such futilities.
KTU 1.47:
Line 2 (*ilib*) (P. 796/dOL 584)**:** My text says "dieu du/des pères" including P.'s quoted interpretation, in opposition to "dieu père". There is no room for complaint here.
Lines 5-11 (seven *b*ᶜ*lm*) (P. 796/dOL 585): P.'s interpretation is correct, but where is the misrepresentation here?
Line 4 (*dgn*) (P. 796/dOL 585): Once again, where is the misrepresentation? I would prefer to have the discussion here, that is all. He himself considers further discussion on *dgn*'s identity to be incomplete.
Line 19 (epigraphy) (P. 806/dOL 585): the term "evidence" indicates that there was no such evidence. This is particularly difficult for an epigrapher, seeing that a new text (my first criterion in reconstructing a text) forces the apparent "epigraphic evidence" previously accepted by P. himself to be abandoned. Here KTU 1.41 and 1.87 are dealt with together.
Line 19 (*ġrm w*ᶜ*mqt*) (P. 796/dOL 585): The text in question is discussed in P. 306, so this is the reference to be quoted. Of course, any reader of P.'s work knows that the text is explicitly laid out in the pages where it is transcribed and vocalised. Is this a misrepresentation of mine or just P.'s punctiliousness?
Line 29 (*dr il …*) (P. 796/dOL 586): P. feels the need of making explicit his opinion, which is not affected by my criticism. The misrepresentation is his ("dOL apparently …").
Line 29 (*u*<u>*t*</u>*ḫt*) (P. 796/dOL 586, n. 148): Here there is no misrepresentation but simply a rejection of P.'s opinion and of his linguistic explanation.
KTU 1.48:
(just astounding) (P. 810/dOL 589): No comment: P.'s astonishment is itself astounding.
KTU 6.13/14:
Line 3 (*mḫr*<u>*t*</u>*t*) (P. 796, 811/dOL 592, n. 163): What is misrepresented is my own opinion that P. kindly corrects (printing mistake *mḫr*<u>*t*</u>*t*/*mḥr*<u>*t*</u>*t*). Then this entry is out of place. Aside from thanking P. for the

correction (already pointed out earlier), on my behalf it is necessary to point out the unfounded nature of his astonishment, its *raison d'être* being based on a "more commonly", but not exclusive, supposed morphological hypothesis.

KTU 1.78:

(on the calendar) (P. 811/dOL 592, n. 165): A reader of "average intelligence" would have understood that the calendar intended here is not the Ugaritic one. From the calendar alluded to is derived the Christian celebration of "All Souls" in autumn. - I followed the sequence as established by M. Cohen[29].

Line 1 (*ṯṯ ymm*) (P. 796f./dOL 592): It is precisely P.'s implication (plural morph) that I find objectionable, which is not a misrepresentation.

KTU 1.86:

(state of the tablet/epigraphy) (P. 797/dOL 593): I do not quote any term worded by P. I simply assert my own opinion on the epigraphic status of the tablet. It would be strange if the damaged surface of a tablet did not affect the signs themselves. Some specifics of these signs are pointed out in *TR* 459.

KTU 1.90:

Line 23 (*ṯʿy*) (P. 797/dOL 594f.): Then there is no misrepresentation on my part, but inadequate evidence for his own opinion from P. One would expect the grammatical analysis to be clarified in the commentary (which it is not the case), as the matter is of some importance.

Line 1 (*id*) (P. 797/dOL 594, n. 172): I refer not to the function of the particle, but, as this is unclear, specifically to the confidence P. shows in the interpretation of the text, based on it.

Line 4 (*kmm*) (P. 797/dOL 595): I agree, but the explanation was expected in the commentary (at least with a cross-reference) since we are dealing with a textual parameter. Where is the misrepresentation?

KTU 1.91:

Line 1 (on wine as sacrificial material) (P. 811/dOL 596): Surely the assessment was inexact: actually there are *two* mentions of "wine" (KTU 1.41:23; 1.112: 12-13), rare in any case, as the texts collected in *TR* 1153 prove. One is in KTU 1.91, which mentions *yn* as an offering material but not as offered material, is not a cultic text, but a record of *yn* as cultic material (for P. himself it is an "administrative" text; see *RCU* 215ff.)[30]. Likewise, the mention of *ris yn* in KTU 1.41 cannot be taken into account in this connexion.

Line 10 (*dbḥ x*) (P. 797/dOL 596): The remark refers to the preceding lines. There was a confusion of numbers, as in the following comment (11 for 12), which was my mistake. In any case, the vocalisation *dabḥu ṣapāni* shows a clear genitival construction.

Line 29 (numerals) (P. 797f./dOL 597): The *normal* order is clear, although some exceptions exist, and to corroborate this situation the grammatical authority of Tropper is quoted. The *hapax* character refers to the only text adduced by P as supporting his interpretation. Is it so strange to use the label *hapax* for a text of which only one witness is quoted? In fact, it was P.'s inadequate quotation of Tropper's authority that provoked it. The norm would be to reconstruct the "dizaine" before *ṯn*, and the exception would be to place it afterwards. Nowhere is the reconstruction in question attributed to P., although his version "([X-dizaines et] deux)" could visually imply it. But this whole discussion would be unnecessary if the solution favoured in *UF* 24, 2006, 597 were accepted.

29. See M.E. Cohen, *The Cultic Calendars of the Ancient Near East*, Bethesda MD 1993, p. 378.

30. See D. Pardee, *Ritual and Cult at Ugarit* (Writings from the Ancient World, SBL, 10), Leiden 2002; also my article below "*gdlt/dqt*: ¿ganado o pan como materia sacrificial?", *AuOr* 25, 2007, 169-173.

KTU 1.102:
Lines 15-17 (Names in /y-/) (P. 798/dOL 599f.): Obviously, this is not a misrepresentation of P.'s opinions, but an inadequate quotation with two printing mistakes of which I declare myself guilty. It should have had to point out more explicitely the substitution of *théonimes* for anthroponimes. Anybody can check the text and its content shows my agreement with P.
KTU 1.103:
Line 1 (*ṯatt*) (P. 798/dOL 600): Obviously the collation I am waiting for is not his, even if now he has no doubt about the shape of the sign as was the case after his previous collation.
Line 12 (*rgm*) (P. 798/dOL 601): Even if made independently, I have no interest in priority; I only wished to point out our agreement, not to detract from his authorship. Evidently, "the preliminary edition" did not interest me at the time. Although published in 1992, my interpretation had been prepared independently long before.
Line 17 (*ṯnn*) (P. 798/dOL 601): The meaning of *ṯnn* "archer" is already to be found in *MLC* (1981), p. 643, and P. quotes *RC* (1992) p. 238 n. 73 (*TR* 556 n. 211), in support of it, I suppose. The agreement is literal ("le corps puissant des archers" // "(el jefe) del poderoso cuerpo de arqueros") ("the mighty archers", *AfO* 33, 1986, 125, with no reference in the commentary). See previous remark.
Line 36 (pronominal antecedent) (P. 798/dOL 601): Although nothing is said in the commentary on the topic, this remark was not necessary, I admit, since *–nn* could also be a 3 p. fem. verbal suffix.
Lines 39-40 (syntactic inversion) (P. 798/dOL 601): As far as the morphosyntactic pattern goes, the occurrence in the first or the second clause is irrelevant. On the other hand, I thank P. for certifying "Ici aussi del Olmo Lete s'écarte de la traduction de Dietrich et Loretz". I was beginning to think I was taken for their *pedisequus*.
KTU 1.104:
(**administrative character**) (P. 798/dOL 603): According to the objections P. himself raises against the ritual nature of the text (*TR* 568), I formed the impression that in some way he was in favour of the possibility of considering it to be administrative in nature. Perhaps I was wrong. Nevertheless, nowhere is the rejection of this alternative formally expressed, certainly not in *TR* 568.
KTU 1.105:
(order of the tablet faces) (P. 806/dOL 603): It is just my opinion that is expressed here in connexion with the arrangement of the faces of the tablet, which is contrary to P.'s opinion. Is that a pernicious sin?
Line 14 (/hz<p>/) (P. 776/dOL 648): P. considers it an error to see an *erratum* here, but according my own criterion of giving priority to textual witnesses, if the reconstruction is "necessary", even only, "à l'état actuel des données", I find it suitable to take it into account in the text transcription itself. Obviously it is a question of a difference in criteria.
KTU 1.106:
Line 1(([…])) (P. 799/dOL 605): Irrelevant. In any case, something is missing. The square brackets should have been outside the parentheses! This is a misrepresentation!
Line 11 (on *pdr(y)*) (P. 811/dOL 606): The unimportance of *pdr* appears not only in the cultic texts, as results from the *ratio* established by P. himself, but in general in the Ugaritic Pantheon[31] and also from the significance of the texts in which both deities appear. In fact, according to P.'s "index des mots", the mentions of *pdr* would occur only four times not six (KTU 1.28:3;1.130:20 and 1.134:4, adduced for both DN, suggest in fact the restoration /pdr(y)/; see P. 1197-1198, not 1155, as P. quotes; even Homer nods). The objection to the possible "presumption" of an erroneous word-divider is irrelevant and ignores the

31. Cf. *CR* 71, list of frequency.

coherence of the text. – Nowhere do I say "P. uses"; the word was intended to enhance an infrequent and daring use by P. His susceptibility is beyond measure.

Line 22 (on the palace garden) (P. 811/dOL 607): I was aware of the archaeologist's point of view and respect it; nevertheless, I feel authorised to dissent from her and from P.[32] Where is the astonishment? Perhaps it is in P.'s inability to accept discrepancy.

Line 30 (reading /ar⸢b⸣[ˤ]/) (P.799, 806/dOL 608): Exacerbated futility. I took "transcription" and "remarques" as a whole, mainly in a case like this where both occur on the same page. But in the remarks the /⸢b⸣/ is not discussed, only posited. Evidently for P. the strokes are not epigraphically ambiguous. But what else? (P. 806)

Line 30 (restoration) (P. 811/dOL 608): Irrelevant. Had P. always adopted the use of summaries, his book would have been half as long. A cross-reference would have been enough when dealing with an epigraphical question that is so decisive for him. In any case, I am perfectly entitled to show my preference. Where is the astonishment?

KTU 1.109:

Lines 24ff. (honest misunderstanding) (P. 799/dOL 609): Actually I understood that P. was refuting De Tarragon and dOL's theory, but the arguments were so poor that in the long run not even he put much credit in them, resorting to an overall negative argument. I took that as an act of intellectual honesty, but it seems that I was wrong. In this case, the misrepresentation failed by excess …

Line 35 (epigraphic possibilities) (P. 799f./dOL 610): Precisely in broken texts such as these, a blank space would be a good opportunity to propose or guess a reconstruction, especially if there is parallel material to recommend it. But evidently "dOL has no understanding of the epigraphic possibilities in breaks at the ends of lines". That is P.'s prerogative.

KTU 1.111:

Line 3 (*aṯḫlm*) (P. 800/dOL 611): Irrelevant. The quotation does not answer the objection of inconsistency; it simply confirms what I say, nor does it explain what P. asserts on p. 623, apparently the contrary.

Line 19 (*trḫt tar*) (P. 806/dOL 610): Once again, we have here the distinction between "philological" and "epigraphic", which is irrelevant for me as far as text reconstruction goes.

Line 23 (*bhmt*) (P. 811f./dOL 612): KTU 1.103 and 1.163 are not cultic texts (!), but "divination texts" (see *RCU*, table of contents), incomprehensibly catalogued by P. as "Texts of Sacrificial Cult".

32 According to her own description, the "garden" is more a private leisure spot than a cult place; see 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 1997, p. 53. (See already Schaeffer, *Ugaritica* IV, p. 15f). But the actual problem is not if there was a garden in the royal palace of Ugarit, but to what space does the term *gn* refer in the cultic texts. See in this regard Fr. Stavrakopoulou, "Exploring the Garden of Uzza: Death, Burial and Ideologies of Kingship", *Biblica* 67, 2006, 1-21. No mention of a palace garden is to be found in J. Margueron, *Recherches sur le palais mésopotamiens de l'age du Bronze* I/II, Paris 1982. For a similar problem in the Mari palace see id., "Du nouveau sur la cour du palmier", *MARI* 5, 1987, 463-482: "la fouille … a prouvé de façon certaine qu'aucune plante n'a jamais pris racine dans la partie de la cour 106 …". Already Y.M. Al-Khalesi, *The Court of the Palms: A Functional Interpretation of the Mari Palace* (Bibliotheca Mesopotamica, 8), Malibu CA 1978, p. 10f., excluded the existence of a palm garden in this court. On the contrary, J.-M. Durand suggests that the trees of this court "sont disposés dans des structures amovibles" (pots) and also that "la 'Cour du Palmier', elle même, est un lieu sacrificiel comme le montre le 'Rituel du *Kispum*'", namely, a place for the ancestors' cult of the dead fitting exactly the context I propose for KTU 1.106:22ff; see J.-M. Durand, "L'organisation de l'espace dans le palais de Mari: le témoignage des textes", in E. Lévy, éd., *Le système palatial en Orient, la Grèce et à Rome. Actes du Colloque de Strasbourg 19-22 juin 1988*, Strasbourg 1987, p. 57. On this item see lastly J. Pasquali, "Ancora sul teonimo eblaita ᵈ*Ga-na-na*: alcune osservazioni comparative", *NABU* 2007, nº 44.

KTU 1.119:
Line 8 (*ṯᶜy*) (P. 800/dOL 618): Let us leave aside the irrelevant typographical minuteness in the exactness of the transcription/translation: "maison de l'officiant /ṯᶜy/ sacrificateur"; the last specification only reproduces what follows in the quotation: "fera-t-on sacrifices". In this case, the actual question concerns translating *ṯᶜy* as "sacrificateur" and not as "sacrifice-*ṯᶜ*". There is no shadow of misrepresentation.
Line 14 (*išḫry*) (P. 800/dOL 618, n. 214): Effectively P. is right. I overlooked his description, as I did not see the word actually transcribed as is P.'s usual practice.
Line 13 (*urm*) (P. 800/dOL 619): It is clear, at least to me, that the version "flammes" does not indicate a special kind of sacrifice but just a mode of combustion. As we have seen (cf. above) P. does not recognise *urm* as the third kind of sacrifice in the text KTU 1.39:8; the semantic development will appear in KTU 1.119:13. In the comment on this text, nothing is said about *urm* (*TR*. 676) and there is not even a cross-reference.
Lines 22-25 (morphology) (P. 812/dOL 620): Completely irrelevant. Pure nominalism (distinction between morphology and morphosyntax, once again undergraduate teaching). Let Semitists and grammarians be surprised, provided they are people of "average intelligence" (P. 812). It is not the morphological categories but their concrete application that is criticised here.
Line 28 (restoration) (P. 812/dOL 617): I find the reconstruction hardly justified epigraphically, but contextually acceptable. The first aspect could be disregarded.
Line 34 (epigraphy/philology) (P. 806/dOL 617 and n. 211): Is the "astonishment" his or mine? I can understand that my remarks in this case may offend P. in connexion with the relationship I establish between philology and epigraphy. On the other hand, I reaffirm (see above on KTU 1.164:10) my criticism on the way P. behaves not infrequently in his reasoning, shifting from possibility to probability to certainty. By the simple incomplete quotation does he mean that his suggestions are always "objective"?
KTU 1.123:
Lines 1-3 (on methodology) (P. 812/dOL 621): Irrelevant remark: I do not exclude rationalisation but its excess. This is my appreciation in this case, but perhaps I am wrong.
KTU 1.127:
(text division) (P. 800/dOL 623): Excess of susceptibility. Any interpreter of any kind also works "in quest" of something. The pretension of the absolute neutrality of the mirror that "reflects" the object to be interpreted is a naïve position turning into a dogmatic attitude. The expression is not derogatory, at least for me.
(general appraisal) (P. 800f./dOL 624): The distinction that now P. introduces is nowhere to be seen: normally his "Conclusion" refers to the whole text commented upon. As for the rest, dOL was not ruffled at all by P.'s interpretation of his classification of the ritual text, but was perhaps when he is called "obtuse".
KTU 1.132:
Lines 1-3 (*št*) (P. 801/dOL 626 and n. 225): The resort to the authority of previous authors to justify his options is not the normal attitude of an exegete who thinks of himself as a first-hand worker and wants to revise the entire hermeneutical tradition. If the discovery was made by others, he made the discovery his own by accepting (perhaps uncritically?) the opinion of others, the pernicious "masoretic" sin he imputes to me. My dictionaries and those of the translator were good enough and the meaning is adequately interpreted: it is always related to "bed", as is obvious from the text (*ᶜrš*) and the comment ("bed-clothing"), which can be read. In fact "set of trappings" (according to the Oxford Dictionary: "ornaments, dress in external, superficial and trifling decoration") was intended as a translation of French "ensemble des objects qui composent un lit (matelas, couverture, etc)", according to Larousse. P. himself quotes Herdner, saying that "the term may refer to a piece of *cloth*, not to a garment". Furthermore, the following

remark is also irrelevant, mere repetition: “le rapprochement” is there, even if it is considered that “n’est pas évident”. Once again, where is the misrepresentation?

Line 2 (rbd) (P. 801/dOL 627): Out of place. The question is about /nᶜr/ not about /rbd/, which is only mentioned as a contrastive pair.

KTU 1.141:

(philology/epigraphy) (P. 806/dOL 628): I maintain the suitability of philology as against an indefinite epigraphy. See *passim* above.

(on slavery) (P. 812/dOL 628): This is precisely what is in question here. In any case, slave transactions were certainly more complicated than P. seems to suppose[33].

KTU 1.143:

Line 3 (/*ptḫ*/) (P. 801, 806f./dOL 629): *Non ad rem*. I simply consider unfounded the conclusion drawn. On the other hand, P.’s susceptibility is incomprehensible (P. 806): he himself recognises the reading *yr*[*ẖ*] to be dubious and points out various possibilities (*TR* 774). That is simply what I meant. Has P. really read the page he quotes? Because the answer is obvious. The whole of his remark is out of place.

KTU 1.148:

(general appraisal) (P. 801f.dOL 630): An irrelevant, sophistical and merely subjective expression. The question is not about the ‘discovery’, that in fact is recognised, but about P.’s acid attack against other text editions and their authors, which P. seems to find normal.

(pantheon-ritual relationship) (P. 801f/dOL 630): Of course, his attitude toward the origin of the pantheon list is discussed on p. 546: I do not say “objected” but “that was not, however, his open opinion”[34]. I do not grasp exactly what “the theogonic principal (!) is clear for the first part of the list” means. If it refers to the theogonic principle, which I excluded, I will ask how many generations are supposed to be involved.

Line 1 (date of *dbḥ ṣpn*) (P. 802/dOL 631): P. feels it necessary to record the misprint 191 [sic!] for 1.91. Many thanks! Surely this is a misrepresentation of his own views. In such a significant text as RS 26.643, the revision of the date of *dbḥ ṣpn* (*TR* 500 n. 44) is more than a mere possibility, in my opinion, even if the tablet order is followed. It is a matter of personal judgement, scarcely a misrepresentation.

Line 6 (on objective epigraphy) (P. 807/dOL 630): This is not the only case; see above on KTU 1.119:34, also *UF* 36, 2004, 617, and more generally *AuOr* 24, 2006, 271. Rather than an expression of a generality, this remark attempts to expose a symptom of inadequate methodology (P. 807).

Lines 23ff. (order of tablet faces) (P. 802/dOL 631): Out of place. I am sorry that P. could be misled by my explanation; contrary to what P. says, KTU 1.148 was meant. Perhaps with a little good will he could have overcome the misinformation, since RS 26.146 was embedded in KTU 1.148, the tablet being discussed. I assumed the change in order, which in no way means that I was the first to propose it. This is a preoccupation that bothers P. In any case, it would be a misrepresentation of my own view, not of P.’s.

Line 45 (continuation) (P. 812/dOL 630): *Petitio principii*: because the text is as it is, no other tablet is needed. But if another tablet followed, the text would be longer and more explicit…

KTU 1.161:

(general appraisal) (P. 802/dOL 632): What I clearly mean when mentioning the apparent uneasiness (“a little”) of P. with this text, according to my understanding, is the fact that he foregoes drawing the implications it has, far beyond its immediate ‘funerary’ sense, for the general comprehension of and

33. See RS 34.170:2-4: ⌈IR⌉ ⌈*iš*⌉-*tu muh-hi* LU.MES.DAM.GAR *a-na* SAM *al-te-qa-a* (RSO VII 23, p. 56).

34. He seems obsessively reluctant to accept any unevenness and variation in his opinions. This attitude is certainly not ‘scientific’, especially in such a volatile field as Ugaritology, where any new text forces a revision of former opinions, proving that the neutral interpretation from the epigraphical point of view alone is not guaranteed.

importance for royal ideology in Ugaritic cultic life. From the viewpoint of that text, it is unacceptable to maintain that the royal funerary ritual was modelled on the common people's, as P. states ("I concluded that the royal [ancestors] cult would have been structurally similar to the cult of commoners", P. 788). I say: "... his opposition to the *dominant position* of the cult and celebration occupied by the cult and celebration of the deified dead Ugaritic kings". Can it be asserted that this is a misrepresentation of his point of view? I find P. repeats it *passim* in his works. I would be very glad to know that I am wrong.

(denial of my own qualifications) (P. 807/dOL 632): If only any other Ugarit tablet were in such a state of conservation (compare my deplorable photo in *RC* pl. XL and above all the very good digital one in *MU* no. 13). Thankfully, in this case philology makes much epigraphical trouble unnecessary. In any case, it is only a personal appraisal. - On my lack of qualifications as an epigrapher (a sort of permanent sneeze in P.'s retort) I have nothing to add (P. 907; see *AuOr* 24, 2006, 255-258). Let me only repeat that I was writing a review, not defending a personal textual edition, that I was appraising results, not efforts, however serious they may be (that then depends to a great extent on personal ability; some writers have more talent than others to compose a novel)[35].

Lines 2-12 (on verbal morphology) (P. 8132f./dOL 632): The form *qra* is not necessarily an imperative form, but in a series such as this, the first member(s) can be the absolute infinitive with a jussive meaning, to end with a finite form (imperative or precative qal)[36]. So there is no shift of form in the presentation. In this respect, *qra.u* in line 12 is perhaps highly significant as a hypercorrect form of *qru* (?)[37]. In the solution proposed by P., lines 4-7 would be expected in the 2. person ("vous êtes appelés" > "tu es appelé").

Lines 13, 18 (*išḫn*) (P. 813/dOL 633): A text like this may reveal the confluence of the WS family in respect of this morph, with prosthesis in the imperative[38]. The morphological difficulty I think is less than the one supposed by P.'s solution: a passive N-form of a stative base. Actually, that is Arabic usage!

Lines 14f. (morphosyntax) (P. 813/dOL 633): I do not entertain the opinion that the latter forms are passive at all, but wonder about the change effected in P.'s solution (passive/impersonal). The problem is obviated in the uniform active version of those forms. On the other hand, the meaning "verser des larmes sur son marchepied" for *ydm*^{c} *hdm p*^{c}*nh* requires a morphosyntactic explanation for the government of *dm*^{c}. I suppose that here /hidāma/ functions as an accusative-locative (?).

Lines 18-26 (precative character) (P. 802f./dOL 633): Leaving aside the charming ironic tone used by P in this case, I have to say that I spent all my energy reading these prolix tomes (an heroic deed for which I hope P. will be grateful, since he will not have many readers prepared to do the same). The question is that here I am reviewing *TR*, not P.'s other works. I feel that the precative character (I mean "precative" on behalf of the characters addressed) is not reflected in the version of lines 1-2 given by P. (although elsewhere he may defend that character). The version "vous êtes convoqués" exhibits no precative character on behalf of the "convoqués", in my opinion.

35. To attack a supposed colleague in this unusual way leaves only one thing clear: "a lack of self control". To an incompetent epigrapher may be opposed a phantasmagoric epigrapher who creates a field of work of his own, in which he reigns unopposed, in fact overshadowing in this way his inability to understand a text in its whole ideological and historical bearing. In any case, this behaviour seems unbecoming to a Professor of a major American University. I cannot imagine any other member of that honourable academic institution behaving in this manner.

36. See Tropper, *UG* 492f.

37. For the ambiguity of the particle here see *TR* 821 n. 27. As a conjunction, its position would be at the head of the clause.

38. See Tropper UG 426f.; also P.'s purely negative criticism in the on-line-edition of his review-article (*AfO* 50, 2003-2004, 89, 113f., 120, 270f.).

Line 20 (/l/) (P. 803/dOL 633): Precisely because P. translates /l/ by “depuis”, I say “he ignores the presence of the particle”. It is difficult to imagine that a dead king may descend to the netherworld “depuis (son) trone”[39]. If there is no objection to see an allusion to the descent of the throne in lines 13-16 (*TR* 825 n. 53), it would be normal to see it confirmed in line 20.

Line 26 (*nqmd*’s descent) (P. 803/dOL 633 n. 238): Out of place. Complete misrepresentation of my view and manifest lack of sense of humour to understand what is said. By “difficult position” I simply meant that it is not easy for anybody (not even the deified king *Niqmaddu*) to occupy a position beneath himself.

KTU 1.163:

Line 14 (P. 803/dOL 637): P.’s reference to other works is a way of escaping the proper responsibility for explaining things clearly when dealing with a text. The misrepresentation, if any (in this case there is none, just a wording of a doubt: “one may ask…”), is due to the author’s inability to express clearly his thought or supply a cross-reference. Implicitness is the last thing to be expected from such a verbose author as P.

KTU 1.164:

Lines 6-19 (transcription system) (P. 807/dOL 635): There is no objection here whatever, but a declaration of my acceptance of the different ways of transliteration/transcription used by epigraphers (<>, {}, ⸢ ⸣ …) and Assyriologists (h/ḫ, â/ā…), which are incorrect only when they are inconsistent[40]. Does P. claim to impose his system on everybody?

KTU 1.168:

Lines 5-6 (P. 803/dOL 607f.): Irrelevant subjective assessment: “…leaves the impression …”.

KTU 1.169:

Line 1 (on /tg/) (P. 813/dOL 638): As it is a unique and hypothetical derivative form that coincides with another lexeme, it would have been advisable to record it according to its form and not under the supposed base, at least with a cross-reference and a discussion in the commentary –as is done, for instance, with *ṯtm* on p. 892 (vocalized *ṯâtim*-)– and not left to be deduced from the vocalization (*tôgâ*). In any case, in principle I do not pay much attention or give credit to vocalization (see *UF* 36, 2004, 541, n. 9), which must be considered a result of information from the opinions adopted, not its source. In P.’s retort, he frequently voices his concern for clarity.

Line 3 (on the serpent attack) (P. 813/dOL 638): P. is astounded at the serpent attacking the lower part of the pillar in order “to coil it up”. Of course, this was a translation mistake for “coil up it”.

Line 17 (/ṯwy/) (P. 813/dOL 641): Certainly a dubious and strange base (*hapax*), even in the place quoted (in KTU 1.16 VI 44; see *DUL* 938). The form *ṯt* in KTU 1.104:17; 5.11:7 and PRU V 59:24 [see *TR* 571] is also very dubious, both morphologically and semantically. Why is this astounding?

Line 18 (Semitic construction) (P. 813/dOL 641): The formula is too obvious and simple (and therefore too easy to reproduce) to warrant the authenticity of an inscription under suspicion (Arslan Tash)[41]. The alleged obscurity does not refer to the meaning of the clause in itself but in the text.

KTU 9.435:

(denial of my own qualifications) (P. 807/dOL 635): Mere judgement of intentions, the fruit of his obsession (P. 807) and of my being judged a fanatic and opposed to him by bad will (see above n. 7).

39. For a possible justification of such a meaning see *MU* 76.
40. Compare for instance the transcription of RIH 78/(0)14 in *Syria* 57, 1980, 352f. and in *TR* 859.
41. See below n. 47.

General negative appraisals, mostly of my conclusions

(on vocalisation) (p. 808/dOL 541, n. 9): Here the reader of "average intelligence" was at work! Indeed a school/scholar's exercise was meant. Of course, the astonishment is monumental! But even "scholarly" in the sense of old-fashioned "erudite" and "pedantic" could be appropriate.

(on prescriptive character) (P. 813/dOL 546, n. 20): see *CR* 12ff. P. simply reaffirms his opinion in order to justify his versions in the second person of some very few and incidental morphemes taken as finite verbal forms (P. 813f.). Were he correct, a more frequent and obvious use would be expected in those texts.

(a mistaken remark and on my carelessness) (P. 808/dOL p. 547, n. 24): As can be seen from the whole text of the quoted note, the objection was Gianto's. In order to demonstrate my carelessness (P. 808, n. 85) in preparing this review (four months of agonised reading, almost with a binocular lens, deserves more consideration on behalf of its ungrateful author!) P. makes a juggling distinction between 'quotation' and 'reference'. In any case, generalisation from a single instance is criticised by P. himself (P. 807). The error pointed out came from a conflation of two numbers (320, the issue number of *BASOR*, and the page-number 66) from the heading of the paper, a well-known scribal error. Of course, once again that causes great astonishment.

(on general hermeneutics) (P. 808f./dOL 549): This is a general admonition to modesty in a field where there are so many uncertainties, which is valid for everyone. Other fields are more certain. Time will set things right.

(on text structure) (P. 809/dOL 555): On structure as one of my fundamental concerns in the understanding of a text, see *Interpretación de la Mitología Ugarítica* (Valencia 1984), pp. 20ff. Already my doctoral dissertation (1973) was a study on structure ("Formgeschichte"). The question addressed here concerns the parameters of structuration and the degree of formalisation that can be expected from a record of this kind with no literary significance. Evidently, this criticism is out of context.

(on the stative N-morph) (P. 809/dOL 564): "Such a formulation seems to reveal unawareness" of the exceptionality of the phenomenon, of the different value of the N-morphemes and the lexicalisation of some stative bases in the different Semitic languages.

(on the authority of Philo and of the Arslan Tash amulets) (P. 810/dOL 584, n. 144): The great argument of Philo's witness is an argument from authority[42]. But the Attridge-Oden edition is of no help in this regard and Baumgartner's is rather reticent on the historical use of this source (p. 6). Instead, P. seems to have remained at the second stage of Philo's evaluation according to that author's classification of the interpretative trends of the Hellenistic writer[43]. On the other hand, P. omits to quote others who are more critical of Philo, for example, Ribichini[44]. I do not deny in any way the flow of transmission of tradition in the NWS Levant; on the contrary I defend the *Cultural Continuum* there, but I maintain a critical not a superficial assumption of it as P. does. The agreement in order mentioned is surprising, but can be explained by the hierarchy of the figures when ordered genealogically. But this genealogical importance is completely Euhemeristic and dOL does not need this "witness" to cast doubt on the absence

42. The elementary bibliography provided by P. has been in my possession since the seventies and I have perused and quoted it many times. This mania of coming to the rescue of colleagues with data suitable for undergraduates is rather childish. My knowlege and use of the Philo tradition is extensive: I have incorporated his fragments in one of my books (*El* Continuum *cultural cananeo* … (AuOrSuppl. 14), Sabadell (Barcelona) 1996, pp. 141-160: "Filón de Biblos. La Historia fenicia", traducción, introducción y notas por J. Cors Meya), I have written several articles quoting him extensively and most recently I have contributed a new one on his cosmogony to the *Fs. N. Wyatt* ("El caos y la muerte en la concepción siro-cananea").

43. See A. Baumgartner, *The* Phoenician History *of Philo of Byblos* (Études Préliminaires … 89), Leiden 1981, pp. 2ff.

44. See S. Ribichini, "Rileggendo Filone di Biblo. Questioni di sincretismo nei culti fenici", in *Les synchrétismes religieux dans le monde méditerranéen antique. Actes du Colloque International* …, Rome 1998, pp. 149-177.

of such a structure in the Ugaritic pantheon. Can P. provide proofs to the contrary? –As for the Arslan Tash amulets[45], I was simply repeating a rumour spread in academic circles in Paris that reinforced my first impression when I held this artifact in my hands in the Aleppo Museum on the nineties. My archaeological experience[46] suggested that a piece like this would hardly appear in a Syrian excavation, nor would its material survive the geological conditions there in such a perfect state of preservation; that would be more appropriate to Egyptian mastabas or tombs. In fact, the piece appeared in the antiquities market (1933) outside any archaeological context and therefore was not recorded in the official report of the Mission (1931). Today that is serious handicap to accept the authenticity of any piece, as professed for instance by the Israel Antiquities Authority[47]. So in principle I am in favour of Teixidor and Amiet's analysis that I accepted in *AuOr* (1, 1983, 105-109), which casts serious doubts on the authenticity of the pieces. The rumour mentioned spread precisely in answer to the objection from the unusual state of preservation and the material. Talking about "cast", I understood that the discourse was about a fake or perhaps an imitation of the original in a soft material or a reproduction by moulage[48]. In any case, all such technicalities are not the central point of my incidental criticism of the acritical confidence P. shows in ancient sources.

(on other epigraphers) (P. 796/dOL 589): The retort (related in a general way to his assessment of the erroneous epigraphical work of other scholars in respect of KTU 1.68) sounds like a self-justification /apology. In any case, it is irrelevant: my own expression was altogether modest and subjective ("insinuating a certain …").

(occasional general assessment) (P. 798/dOL 600): The relations of P. with the KTU edition are so entangled that it is better not to discuss that issue. "Complain" is meant as a mild term; certainly it is not the most suitable. Again, nowhere I attribute to P. what is only my assessment. He does not agree with it. I find the "gratification" P. feels on seeing his readings adopted to be symptomatic (see above nn.18, 23).

(on the distinction "infernal"/"souterrain") (P. 798f./dOL 605): Irrelevant commentary concerning KTU 1.105. It comes from his well-known position concerning the ancestral nature of the Ugaritic cult, so that he feels obliged to make matters precise; it was certainly not necessary. Terms relating to the netherworld and its dwellers are sometimes used as synonyms, and sometimes they are nuanced ("souterrain, infernal, chtonien") provided they exclude the aforementioned meaning. I have no objection to his explanation, but do not see any misrepresentation by me.

45. Of course, like any other scholar having to tackle NWS text anthologies such as that of Gibson, I know that there are two Arslan Tash amulets; Donner-Röllig's anthology records only one. The reference to the 'second' was not appropriate, I admit.

46. I do not consider myself an archaeologist, although I have set up and managed an archaeological Mission in Syria for 12 years and taken part in the actual excavations. In this connexion, G. Garbini, commenting on Pardee's edition of "Le papyrus du marzeaḥ" (*Semitica* 38, 1988, 49-68), asserts: "il cui ottimo stato di conservazione era suficiente a dimostrare la falsità". See G. Garbini, *Introduzione a l'epigrafia semitica* (Studi sul Vicino Oriente Antico, 4) Brescia 2006, p. 41, n. 1. This author considers the Arslantash Tablets to be a forgery (see *op. cit.* p. 96).

47. On NWS fakes in general, see the illuminating articles by Ch.A. Rollston, "Non-Provenance Epigraphs Antiquities I: Pillaged Antiquities, Northwest Semitic Forgeries, and Protocols for Laboratory Texts", *Maarav* 10, 2003, 135-193 ("The more data that is present for a language or script series, the more definitive the declaration of unauthenticity can be"); "II: The Status of Non-Provenanced Epigraphs within the Broader Corpus of Northwest Semitic", *Maarav* 11, 2004, 57-79. A few months ago, I had the opportunity of checking a great number of *Ugaritic* tablets and minor artifacts coming from a private collection in Latakia/Ugarit (!). Even an incompetent epigrapher such as myself was able to realise they were plain forgeries. Some years ago, too I had the opportunity of publishing two copies of Mose's "Tables of the Law" engraved in stone in *the most classic Old Hebrew script*, forged at the beginning of the twentieth century (see *AnFil* 4, 1978,247-256; 9, 1983, 183-185).

48. In principle, a limestone cast is impossible, because normally a cast supposes molten material running into a mould. Perhaps this is not a correct use of the English word 'cast'.

(on the contributors to HUS) (P. 811/dOL 606f., n. 194): Of course, I read the introduction and the whole of P.'s review article, but the explanation given there does not convince me. In any case, I only wanted to express my disappointment at P.'s absence, not to apportion blame.

(on the transcription H/Addu) (P. 812/dOL 631, n. 233): If P. had perceived what was intended by the normalisation of the DN, he would have understood the function of the final *–u*, as a hypercorrect NWS nominative vocalisation (/hadad/ is an Akkadian absolute form)[49]. The elementary forms have been shaped according to elementary Akkadian and Ugaritic transcriptions. –Besides that, the *hamza* is implied in my transcription of the Ug. /a,i,u/ signs, so the reference to phonetics intended for undergraduates could have been avoided.

(on quantification) (P. 803f./dOL 642): Irrelevant. P.'s quotation does not contradict my own appraisal and the quotation adduced ("quantification"/"proportions relatives"). His remark on the calculation of "unnamed or plural" (pp. 803f.) is also irrelevant. The astonishment is mine, seeing the juggling P. needs to calculate the incalculable. He himself acknowledges "the fragility of such calculations". Where is the misrepresentation here?

(on P.'s lists) (P. 804/dOL 643): A subjective assessment of what "misleading" may import, depending largely on what one is looking for in such lists. There is no point in discussing such subtleties. –The resort to a "benevolent reader" (had P. been such a reader much of his retort would have been unnecessary) means that readers with "average intelligence" like myself may be entitled to ask for a clarification, for instance on the meaning of the "offrandes doublement attribuées". An offering for two deities is a shared or joint offering rather than "doublement attribuée".

(on inflation) (P. 804/dOL 643): By "inflationist" I meant only unnecessary remarks, whose volume does not correspond to their content, with a price higher that their value; words can be used legitimately in all their semantic possibilities. There is no misrepresentation in my remark, but only a personal appraisal of the significance of the tabulations; I neither discuss nor aim at "presenting" them concretely. This complaint is pure subjectivity.

(on translation) (P. 804/dOL 643, n. 254): When a translation is provided along with the original, there is no misrepresentation, only at the most inability on behalf of the translator/corrector.

(meaning of sacrifice) (P. 804/dOL 643): No comment. I do not see any misrepresentation, but only an ironic description. Of course, "sacrifice" implies much more than the alimentary function. I have to confess that P.'s remark is nevertheless for me of extraordinary importance as a possible answer to public distribution in the case of the large number of victims, a problem that troubles P. as it does any other scholar[50].

(*ratio* of male to female victims) (P. 804f./dOL 643): There is no misrepresentation whatever here. What actually we do not know is the age of the animals from which the reproductive rhythm is to be taken into account. As for the prices of the ovines, one could extrapolate the price *ratio* suggested for the bovines.

(on blood in sacrifice) (P. 805/dOL 644): Irrelevant. This is a general statement not specifically aimed at any of P.'s formulations. It is pure nominalism ("problem"/"question"). The same can be said in relation to the following remark "conclusion"/"assessment".

(importance of KTU 1.161) (P. 646/dOL 805): Subtlety: French "occasionnel" according to Larousse: "qui se produit par occasion, par hazard", according to P. "having to do with a particular occasion". In archaeology, all findings are "occasionnel". From such remarks it seems sometimes that P. has a private

49. As for the attestation of Adad/(*H*)*addu*, $^{(d)}$*ad-du* in Ugarit see Grøndahl *PTU* pp. 78f., 81, 318, etc. See above n. 8 on KTU 1.65:9.

50. See *TR* 84 n. 337; Tropper, *UF* 33, 2001, 551; see below in this fascicle: *AuOr* 25, 2007, 171-175.

French idiolect. In any case, the surprise comes from P.'s inability to draw the implications of a text (besides waiting for new clarifying texts), now confirmed according to him (I myself am not so sure about this confirmation and the unequivocal meaning of the marks on text RS 94.2518).
(list order) (P. 805/dOL 647, n. 264): "Hierarchical" is a word used by P. in *BASOR* 320, 2000, 68, in relation to my list of the Ugaritic gods. The last part of my note refers to this point.
(*errata*) (P. 805/dOL 648): P. is correct in rejecting the supposed errors pointed out by me, even though instead of "voiyons" one would expect the present tense. I hope that the rest of the *errata* not objected to are such. They were intended as an offer to improve the text and indirectly as a proof of my having gone through the whole text. On my part, I am grateful for the errors and misprint pointed out in my paper by P.
({dd/td}) (P. 805/dOL 648). This was indicated with a question mark: I was not sure of the author's intention, as in several other cases to which P. does not object.
(RIH text numbering) (P. 805/dOL 648): Consistency is always desirable when the system comes from the same authors in a publication that presents itself as officially from the Mission in a general revisionist programme. Authors outside the programme must not be required to be aware of the conventions adopted each time. Consistency is what is expected.

Coming to the end of this answer to P.'s own retort on my critical appraisal of his work, I can sum up the result. In half a dozen cases or so, I have accepted P.'s well-founded remarks: in these cases I feel guilty for not having quoted his statements correctly, even of having been wrong in my appraisals (see above). Otherwise, for 99 per cent of the criticisms made by him in my review I find that it is P. who is wrong; he misrepresents, or rather, misinterprets, not infrequently in a rude manner, my remarks on a book of some 1300 pages of which possibly I am the only person to have read it in its entirety. That is "indicative of the quality of the 'Replik' as representative of the positions put forward" (P. 793) in my review[51]. Without assuming that it has "affected his critical faculties" (P. 779), one gets nevertheless the impression that the concentration on his painstaking and isolating epigraphic task has prevented him from having a broader religious-historical appraisal of the texts. That is all.

51. Another egregious example of P.'s lack of objectivity, not to say of his inability to represent and appraise other colleagues' contributions, when they do not conform to his own views on the topic, can be seen in his online review (*RBL* 03/2007) of M.S. Smith, *The Rituals and Myths of the Feast of the Goodly Gods of KTU/CAT 1.23…*, (SBL Resources for Biblical Studies, 51), Atlanta GA 2006. P. gets lost in a couple of epigraphical and lexical discussions, well-known and of rather limited significance, without attempting to appraise the main religious-historical thesis of the author. From his conclusions can be deduced that such overall hermeneutical treatments seem irelevant to him or simply that he is not aware of their importance.