

Notas

Once Again on the Ugaritic Ritual Texts I. On D. Pardee's Epigraphy and Other Methodological Issues

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During the onerous task of reading through the thousand pages of Pardee's (henceforth, "P.") *magnum opus* on the ritual Ugaritic texts I carefully avoided any personal qualification of his opinions, using mainly adjectives such as 'likely/unlikely', 'hypothetical', 'speculative' or at most 'irrelevant', when the line of argument seemed to me to be tenuous or lacking in actual content, and repeatedly adding the restrictive note 'in my opinion'¹. I deliberately adopted this approach because throughout the text I came across dismissive qualifications of my opinions and of those of others, frequently verging on insult or at least likely to be perceived as such by their authors (opinions have parents). But this was nothing new: we are well acquainted with P.'s record of occasionally offensive quarrelling with colleagues, ending sometimes in a more or less sincere reconciliation². I decided nevertheless not to take this behaviour too seriously (at a certain age a zero-adrenaline temper is compulsory) and to answer in a slightly ironical tone, since it is universally accepted that irony is not offence. If somebody lacks a sense of humour, then that is evidently his problem.

Nevertheless, P. defines my review as "characterised by sarcasm, mockery and personal invective of all sort", fruit of my "exasperation" (My God!) (P. 769). I was at a loss to understand exactly what he meant. Fortunately he quotes as "a revelatory example" my comment on his review in my contribution to HUS (n. 6). I invite interested readers to see if they can find any sarcasm there³. At one level P's extreme susceptibility is comprehensible, given the tremendous effort he put into his task, which a clearly incompetent fellow such as myself now dares to criticise; but what he deems 'mockery' I simply presented as a negative assessment of his work. P. feels entitled to attribute to me an academic and even ethically perverse attitude towards him: "scorn" and "mockery" (n. 6, p. 771, 773 and *passim*), "ill will" (p. 780), "epigraphic inflexibility", "semi-canoncity", "insensitivity", "intransigence", "naivety" (p. 769, 771 779, n. 8), "personal antipathy" (p. 779), even more or less "perturbed critical faculties" (P. 779), "exasperation" (p. 769), being sometimes "cruel" (P. 785) and "obtuse" (p. 792, n. 71), in a sort of *in crescendo furor mentis* of persecution mania.

¹ Sometimes I label P.'s opinion as 'materialistic' or 'fundamentalistic', extrapolating his own self-description as "minimalist" in a subject of the highest significance for the interpretation of the cultic text, concrete but programmatic in my opinion. This self-definition describes well P.'s constant position in other simpler subjects (P.'s 790, n. 67; p. 774; these figures here and in the following I refer to P.'s reply). I use the three terms as synonyms to describe this position. Incidentally, I used simple marks to enhance a concept, reserving double quotation marks for literal quotation. This is quite straightforward, though it constitutes a problem for P. (!); cf. p. 767, n. 1). I am sorry if this does not fit the American practice.

² See *UF* 23, 1991, 1-8 ("Declaration of Reconciliation"). I am sure he is just unaware of his own attitude. A propos, on 2 June 2007 I received an e-mail of a colleague, whom I do not know personally, who, commenting on my review-articles, says: "It is a pity, Dennis Pardee is making courteous scholarly debate virtually impossible". See also the harsh reaction by M. Dietrich and O. Loretz to P.' first discrediting insinuations on the epigraphic inadequacy of KTU (*UF* 22, 1990, 1-4) which I fear is still maintained.

³ On the other hand I do not understand the intention of n. 5. It is obvious that each language has its own syntactical structures.

In any case Pardee feels offended and claims that he has no wish to engage in a ‘personal’, offensive and unproductive dispute (p. 767); evidently we have different ideas of what ‘personal’ means. To demonstrate his assumed ‘impersonal’, “reasoned, non-emotional reaction” (P. 767) he began by personalising his reply (“G. Del Olmo Lete’s views on ...”), a model I have felt constrained to follow⁴. This means that from the very beginning his aim was not simply, despite his solemn pronouncements, to lay out and defend his own opinions, but to disqualify the reviewer, a task that he carries out, mainly but not exclusively, in the 13 introductory pages and the 8 pages of his first conclusions (see p. 769, and n. 4; p. 771, and n. 8; 772, 785 etc.). This is a very effective way of preventive war: if the fellow is incompetent, everything he says is irrelevant; his reply can be forestalled (see especially p. 773-74)⁵. In my long career as a reviewer, with hundred of works to my credit (sometimes I may have been a little harsh, I must confess), I have never experienced such a reaction on the part of the authors of the works under review; nor do I believe that this has happened frequently to other colleagues. I have also accepted the criticisms of my own works, recognising the reviewer’s right to find fault and disagree with my ideas. With his attitude, in my opinion, Pardee has descended to the lowest levels of acceptable behaviour in the academic world, seeking to discredit me as a critic by revealing my mental prejudice, incompetence, and personal ill will towards him. Unavoidably we all have opinions on the merits and scientific significance of our colleagues, but these opinions should remain private. P., nevertheless, feels qualified to make a public award of credentials of competence and incompetence to others. According to him, I am not an epigrapher, but, in compensation, I am a lexicographer and exegete (p. 4, and n. 4). I do not know the exact word English to describe this exercise of arrogance and baseness; in boxing the blow would be considered “below the belt”. In Spanish there are a couple of rather strong semi-synonyms which I will not use so as not to descend to the level of abuse reached by some.

But let us follow the lines of thought that P. opens up. If he is an epigrapher and others are not, does this mean that he is just that (maybe the best or the only one) and, consequently, is incompetent in other areas? Does it mean, for instance, that he has little sense of the text and its intertextuality, and even less of what the Royal Ideology means for the social and cultic organisation of the Ugaritic kingship and society? I would never have said that, but now, accepting P.’s self-definition and the disqualification of others (I am afraid that I am not the only one disqualified in this respect: some others are quoted [p. 74], others alluded to) the question may be worth considering.

I have to confess that I am in no way affected by P.’s disqualification of me as an epigrapher, knowing his idea and exercise of this art, nor do I feel flattered by his considering me as a lexicographer and exegete⁶.

⁴ Initially I thought of calling this note *Tahāfut al-tahāfut*, but maybe some Ugaritists lack sufficient knowledge of the Arabic language and literature to catch the reference.

⁵ “As he has never worked with tablets ...”. A couple of authors, editor of HUS, are also involved in this assessment. P.’s knowledge of the academic biography of his colleagues is intriguing.

⁶ Let me be rather sceptical about this attribution, at least as (in P.’s opinion) a reliable lexicographer. Leaving aside discrediting gossip in private academic circles, there is an objective datum: notwithstanding his generally positive appraisal of the value of the *Dictionary of Ugaritic Language*, by Prof. Sanmartín and myself (“so fundamental a work for Ugaritic studies”; see *JNES* 65, 2006, 234), the work does not appear in his summary bibliography of the *Manuel d’Ougaritique* (Paris 2004). Of course, everyone is at liberty to choose their bibliography and, in an elementary handbook like the one quoted, space is of the essence: beginners need only the most important references. But how do we understand his praise for this dictionary if he then fails to quote it? So let me be slightly sceptical about his qualification of me as a lexicographer. As for my exegetic (!) capabilities, these seem to be reduced to mere speculations (“the basis was not the examination of the texts...”; p. 786ff.). For my part I do not aim to convince P. (n. 7); on the contrary in his work I have found data that have obliged me to nuance or even change my earlier points of view, for which I thank him.

But let us come to the subject matter. Is P.'s flat description of the epigraphical data too cryptic and esoteric for a common Semitist to be able to grasp their meaning and significance, even if he does not indulge in the practice that generated them? Literary critics are not normally novelists; art critics are not painters. P. dismisses my criticisms on the grounds that I have not taken part in the epigraphical activity of the sort he has carried out; I cannot understand the effort involved and am therefore not competent to judge its results. This criterion would, at a stroke, invalidate almost all critical activity. My review-article was not a critical textual edition, but an appraisal of the epigraphical work carried out by an epigrapher. I was evaluating not effort but results. Furthermore I do not know any Ugaritologist, or Assyriologist for that matter, who is unable to collate (and who in fact does not collate) the texts on which he is working, if he believes it necessary, thus acting as an epigrapher. In fact the Assyriologist is an epigrapher from the very beginning of his training, struggling with the syllabary; the Ugaritologist, in contrast, is spared this troublesome task, thanks to the simplicity of the graphic system, for whose mastery he needs only the basic description of the alphabet and its variations provided in the Ugaritic grammars and the exercise of reading tablets and autocopies in the first year of university, as P. points out himself (p. 778). We do not have (indeed the homogeneity of the materials may not allow it) a *Manual of Ugaritic Epigraphy*, as we have for instance for the different stages of Northwest Semitic (Cross-Fredmann, Peckham, Naveh, leaving aside manuals of Hebrew and Arabic epigraphy). Maybe P. will some day surprise us with a manual of this kind. In any case, even if Assyriologists and Ugaritologists practise as epigraphers I do not know any who indulge in the practice P. carries out (and in the case of the assyriologists possible extra wedges are not the main reason); above all in the dilatoriness with which he presents the *verbatim* description of the slightest remains of a broken sign, discrediting any other interpretation of them. He deems this task to be obligatory, a position he accuses me of dismissing with "scorn" (p. 771f), another example of his hypersensitivity towards any form of dissent. In fact this self-clarification is practised by all authors as a basic step in the compilation of their material without explicitly describing it afterwards (a lack of explicitation about which P. complains, p. 772, n. 10). This is, to my knowledge, the general practice of any textual critic, as seen in the *apparatus* he provides. In this regard P. emerges as the only genuine epigrapher, head and shoulders above a horde of incompetent practitioners. But what he considers 'honest', others will label simply 'unnecessary', the consequence of a personal temperament (p. 772).

In this regard my mortal sin was to trust the KTU textual edition, P.'s 'bête noire', the brunt of his attacks, dismissed in its day as inadequate (see above n. 2). I had the opportunity of collating the tablets myself in my yearly travels to Syria over a fifteen-year period. But more interested in *texts* (indeed, I am interested in them, in spite of what P. says!) than in *broken signs*, after having dealt with the literary texts on the basis of the official edition of the 'Mission Archéologique Française', recognised as reliable by P. himself (see later), I thought I could trust the re-edition of the ritual texts, carried out by persons who deserved my highest respect as Ugaritologists, but who failed to meet the standards of reliability that P. requires of epigraphers. In his words, this was an 'acritical' move maintained with "extreme inflexibility" (p. 769). Consequently, to overcome this serious imputation, I have had to revise my new edition of the Ugaritic Cultic Texts (*Canaanite Religion*), collating the debatable spots in Damascus and Paris, photographing all the tablets from different light angles and taking advantage of any other transcription and autocopy, including those by P. which are invaluable, I freely admit⁷. In this case the very good digital

⁷ P. blames the Editors for the lack of photographs in his work. But he does not seem free from responsibility: he could have balanced better the elements in a critical textual edition ("La longueur du manuscrit interdisait ..."), leaving out the redundant description and diffuse general and well-known commentaries and summaries of basic cultic notions. The case seems to have been repeated with P.'s immense review of Tropper's grammar, rejected in its full form by AfO. In this same "Reply", P. offers a "selection" of quotations for the reader not interested in the

photographs provided by the *Manuel Ougaritique* are paradigmatic and support the fundamental validity (at least for well preserved texts; p. 778) of this instrument for text study and edition, as I maintain (p. 777 n. 26)⁸ – as do all Orientalists working in museums, copying, photographing and compiling data bases of them. But this does not mean that the autopsy of the tablets should not continue to be considered the primary source of information.

In fact in this regard my view is not far removed from P.'s although he has charged me with a fanatical appraisal of the value of photographs as the supreme form of epigraphic practice, not as a subsidiary but as a standard instrument. The function assigned by P. to photographs – namely, testing and contrasting the different autopsies, P.'s and any other else's – is entirely valid. Photographs provide a fixed record and can be compared with those of different tablets; by definition, the autopsy is subjective, a one-off event⁹.

Incidentally, did P. collate the literary Ugaritic texts with the same zealous and painstaking accuracy when carrying out his translation in *The Context of Scripture* (1997)? The notes do not allow us to answer this query. Is he maybe preparing a new epigraphic edition or can the texts, so often reedited, manage without it? It seems that the project is underway, as the culmination of the textual revision. It will always be possible to detect the presence of an overlooked word separator or to certify the actual value of a broken sign more accurately¹⁰. In fact P. recognises that “the texts discovered before World War II were edited according to proper epigraphic methodology” (p. 774). That is to say, on the one hand text editions can be made without having recourse to P.'s methods, which is what the normal epigraphers do; and, on the other, he himself has relied on editions by others. Nevertheless, as P. points out, “advances can still be made in the basic epigraphic analysis of even these texts”¹¹. For others, “the epigraphic basis for the

details of the controversy (though this “selection” is almost as long as the original text). Summarising is not one of P.'s abilities. On the other hand it is absolutely improper and malicious to assert that “if these photographs had been published, dOL would certainly have criticized their quality, as he does the quality of the few that are provided in TR” (P. 777). I do not know if P. realises how ignoble it is to project one's own attitude upon others. He ascribes to me a fanatical *parti pris*. I deplore the poor resolution of a photograph, beginning with my own, when they are no use to us in our attempts to examine the text, but greatly appreciate those that present good resolution, as is the case of the photos in the *Manuel*. However the photographs in *TR* are not good enough for a critical text edition.

⁸ But, be careful, they are maybe a bad example for beginners who could be induced to think that such artefacts are enough to read and treat Ugaritic tables, above all to edit them. It would have been more coherent to promote the granting of scholarships to travel to Damascus to collate the tablets.

⁹ Here P.'s persecution mania reaches new heights, attributing to me “antipathy to any result achieved by P., a stance that he is certainly entitled to take but one that lays bare his absence of objectivity given that he grants semi-canonical status to CAT in spite of the large number of new readings that are to be found there as compared with KTU”. This is completely unacceptable and false, as any reader of my review can ascertain: in it I record many new readings by P. as significant and worthwhile at the start of my comments on each text. And the attribution of the semi-canonical status to my use of *CAT/KTU*² is a plain stupidity: “canonicity”, that is, an unconditional faith in a text, is a category absent from any facet of my intellectual activity. Evidently I had to rely on the corrected version *KTU*², precisely the one which, in some cases, provides the new readings offered by P. I used it as my starting point because it was the most reliable text collection and collation available at the time, long before P. published his work, but in any case I do not consider it to be definitive. To attribute such an attitude to any researcher in our world is childish and offensive. My disagreement with the authors of *KTU* goes far beyond the text, as can be ascertained for instance in *DUL*. A new case can be seen in my review of their work *AOAT* 268/1 to appear in the next vol. of *AuOr*.

¹⁰ Of course this is a “sarcastic” insinuation. But for a typical paradigm of what the analysis of those texts would be like, see now *UF* 37, 2005, 479-489.

¹¹ I imagine that Smith's study on the Baal Cycle would not be deemed adequate either, in spite of the formidable collection of pre-digital photographs it provides.

various collections of texts (underlining mine) has been either inadequate or non-existent” (*intelligenti pauca*).

As a matter of fact I consider P.’s epigraphy inflated, unreliable, and of little use. When I label it an “epigraphy of the edges” I do not mean the edges of the tablet, a place as suited to support writing as any other (cf. 773), as P. attributes to me, but the edge of the text, the place where the text breaks off leaving only some small traces. Normally text editors record these traces in the autocopied and provide transcriptions; in the clearer cases, they give the most likely sign reconstruction, sometimes fairly certain, others accompanied by a question mark or an alternative option. To insist on an apodictic defence of the reconstruction of the likely sign by itself and to seek to defend it against any other proposal is what I consider out of place, a pointless chasing after ghosts, mistaking windmills for giants. It creates an impression of scientific endeavour in something that I deem to be mere fictional science. I can imagine and respect the tremendous effort put into such a task, but at the same time cannot avoid recalling the verse of the Latin poet: *parturient montes ...* Of course, P. spares us from reading his epigraphical explanations (p. 772); this dispensation is precisely the reason why I call it an inflated, pointless epigraphy. In such explanations the ‘interested reader’ will normally find P.’s personal perception of the fragmented signs already rendered in the transcription and copy, as is standard epigraphical practice. These explanations may be illuminating, but only sporadically.

P. is right that I worked with a ‘model’ in my reconstruction of the royal ancestor cult (gathering together the slightest allusive data, no more zealously, in fact, than he does when gathering the smallest traces of signs; we will come back to this point). But I am fully aware that this model is hypothetical and may therefore be disproved by new evidence; I would never allow myself to discredit other models and deem them out of place. Now, in epigraphic practice we inevitably also have, between perception and interpretation/transcription, the interaction of an epigraphical model, of what signs should look like according to our perception of the ‘typical’ whole element. But cuneiform script is not typography and some variation is unavoidable; absolute certainty cannot be achieved, above all in administrative texts of a possibly careless scribal ductus. The only sound criterion for reconstruction/restoration of broken signs/texts is a parallel text or, in its absence, the sense of the text itself: the scribe is writing a meaningful discourse, not just scratching cunei on a blackboard. In this case the reconstruction may only be approximate, and not epigraphically warranted, but at least consistent with the text’s internal and external intertextuality.

Of course the reading of Ugaritic texts differs from the reading of medieval and other writing systems, but all of them share the basic principle of being the reading of a meaningful text and not the elucidation of the shape of the signs that embody it. It is a question of methodological perspective of both aspects, form and contents, which both imply each other. Equally, the use of a ‘loupe binoculaire’ to detect the profile of a cut or to exclude material excrescences in a piece of art is quite different from its use to determine the nature of a scrape or a sign (p. 770) where it is just as likely to mislead as to clarify. In this regard, the view provided by the naked eye is closer to that of the scribe than the view provided by this implement. The argument is unconvincing, as are other frequent *ad hominem* charges that P. makes; as his refutation of my insinuation of his unease with text KTU 1.161 he states that he made its *editio princeps* (p. 172, n. 71) and has dealt with it a couple of times, but never, to my knowledge, has he drawn the implication that this ‘funerary’ text may have for the ‘mortuary’ royal ideology. The *funus* of a king is not created *ex novo*, but represents a general practice and conception that overrides the concrete use. It is this general interpretation and extrapolation of the significant ideology involved here that I find missing in P.’s interpretation of this and other texts; herein, apparently, lies my ‘obtuseness’ (n. 71).

Another example of *ad hominem* argumentation can be seen in his reproaching me for using the Mari tradition instead of the Emar (p. 789, 790, n. 65). On the one hand I would assume from this that he himself uses the Emar tradition to analyse the Ugaritic cultic practice. But there is no trace of this in his

work, not even a reference to my reliance on this tradition to elucidate some Ugaritic linguistic data. And to doubt the predominantly ancestral character of the Emariote cult can only be attributed to the fact that he is only speaking ‘from memory’¹². On the other hand, Emar tradition, although geographically and historically closer to the Ugaritic than the Mariote, is nevertheless, because of its family and tribal structure, socially and culturally far more distant from the urbanite Ugaritic organisation, leaving aside the possible Hittite influence. Therefore, except for some basic data the ritual content is very different¹³.

It is precisely the example adduced by P. and orchestrated with an abundant photographic array to support his reading of the spot which best illustrates our differences in perspective (p. 779f., 814f.). No objective reader would say that there we have an unequivocal /šrp/, and P.’s projection in fact proves this¹⁴. My conclusion is that there is a superposition of signs, an emendation, the /šrp/ probably being the last intended reading. So there is no “ill will” (P. 780) whatever on my part towards P.’s reading, which I am ready to accept. But this is not the point, as P. knows – he quotes it at length, but disregards it. If this reading is correct, I postulate that the scribe carelessly committed an error of metathesis of a sign, a more

¹² See for instance the summary by D. Arnaud in the collective work mentioned in the following note.

¹³ Furthermore he supposes (apparently he has not taken the trouble to verify this) that my only source of information about the Mari cultic system was an article by J.-M. Durand that I translated and edited in Spanish in 1995 in a collective work: *Mitología y Religión del Oriente Antiguo* (n. 65). In fact this ‘article’ is a *monograph of over 400 pages, based for the most part on unpublished texts*, of which an expanded edition is about to appear in French in a collective work on the West Semites. Maybe this time P. will feel the urge to read it. *Hispanica non leguntur?* But he knows Spanish, and the book was in the library of the Chicago OI. Why an author so keen to quote and gather bibliography of any kind, however trifling, has not read this book escapes me completely (or perhaps not); he accuses me of not having reading all the reviews ... (n. 7). Durand’s monograph on the highly detailed cultic Mari system was drawn from “administrative texts, including letters”, a masterly work, and for the moment the only one, of analysis and systematisation of the data to be read in such inadequate sources. The ritornello that the author hears in those texts sounds: *lā’ ’ilāh ’ilā ’abūna*. For P. this source, apparently not read and certainly not quoted by him, is secondary, a neat way of avoiding having to read it. Realising the parsimony and politeness with which he normally treats his French colleagues, even when dissenting from their opinions (a deference that he does not show towards others), his attitude in this case is particularly surprising.

¹⁴ I have a multiple digitalised photograph provided by West Semitic Research Project, obverse and edges. In this case I could not look at the tablet itself because (in March 2007) it was on loan in Damascus. In any case I would not have been able to look at it with a ‘loupe binoculaire’, as I do not have one. I have nothing against this and I think it is a useful working tool, but I do not think it has revelatory powers (see p. 770f. and n. 8); a simple magnifying glass has always been used and I have frequently used one myself. Accordingly, not only am I discredited (but then I am evidently completely incompetent and ‘insensitive’ to the task of reading fragmentary tablets) but the rest of epigraphers as well who have reached different perceptions without such an implement. My acceptance of their result and rejection of P.’s is once again defined as a ‘mockery’ (sic). But the basis for my rejection is not that the reading is P.’s, but because all are more or less equally hypothetical, in my opinion, and for textual reasons I prefer one to another (see P.’s long digression p. 770f and n. 8). Furthermore, my “lack of interest in epigraphy” and “misunderstanding of the epigraphic process” (n. 9) depends evidently (as P. knows very well, since he has criticised me mercilessly for it) on the fact that I did not want to make a critical textual edition but accepted (naively of course) an already fixed text. The same kind of naivety of which, *mutatis mutandis*, e.g. all the biblical exegetes that analyse biblical texts are guilty; even if KTU is not a massoretic text (p. 777), needless to say (!). All of them rely on the Kittel-Kahle’s edition and do not collate the Aleppo codex, the basis of the new Jerusalem critical edition. Any ancient text can be newly collated and improved. I was primarily interested in the study, not in the fixation, of the text, which I presumed to be basically valid. Not even for the biblical text do I maintain this ‘respect’, as my work on it proves. In both cases it is the coherence of the meaning that allows any change. I had no knowledge of P.’s formidable epigraphic enterprise which was underway at that time. Nevertheless P. frequently treats my studies as if they were epigraphic in nature, when in this regard they were not independent as he stubbornly states, thus unnecessarily inflating his epigraphic notes.

frequent and simpler error than to suppose he was so careless (and this scribe certainly was) as to commit a double omission in the previous lines (p. 776)¹⁵, leaving plenty of space free, and generating an incomprehensible text (two offerings to one deity, which would be highly unusual), as P. assumes. It is text coherence that recommends this solution beyond material epigraphic ‘evidence’, which in this case shows signs of scribal disarray. In this way we achieve a smooth text, in full accordance with its standard parallel (see my comments on lin 17, *UF* 36, 2004, 556), and obtain a comprehensible textual sequence¹⁶. Intertextuality must be given priority over pure epigraphic consideration, and text over isolated signs. Sadly, this explanation was already given in my article but P. does not appear to have read it, obsessed by the necessity of affirming the correctness of the material reading which I eventually accepted. This is but one example of what I call the ‘materialistic’, namely, clinging strictly to the graphic appearance, attitude P. constantly maintains in his epigraphic and textual interpretation and which authorised me to generalise his self-proclamation of ‘minimalist’ on one specific issue, as said above (n. 1)¹⁷.

In fact P. maintains this same criterion of the prevalence of the textual sense and philology, and I already pointed out in my review three cases in which he accepts the reconstruction of a sign against his own modelled perception (KTU 1.65:10; 1.119:34; 1.168:9), forced by text evidence. Now we can add a new case. Reading KTU 1.87:54 P. dismisses Herdner’s reading *yrh š*[i¹] as impossible (“le {i} de l’*editio princeps* aussi bien que le {m} de KTU² nous paraissent donc tous les deux hors de propos”; cf. *TR* 471), in favour of his own *yrh š*[. Here his epigraphic ‘model’ is fully at work (see above). The new discovered syllabic text RS 94.2490 now provides us with the month *ša’iya*, which seems to certify the correctness of Herdner’s perception¹⁸. This is the kind of evidence on which I base myself to label P.’s epigraphy of the edges as ‘unreliable’. Four cases may not be statistically significant, but represent a serious indication; if we had more parallel texts, the cases would probably multiply in a significant way.

Of course, nobody will deny that a *princeps*, an official or just a critical edition of a text must be carried out on the basis of the original autopsy. But in the long run this is a subjective activity that forces others to trust the eyes or lens of the editor, whoever it may be, and his epigraphic model, a matter we have discussed above. Photographs are the technical means that can overcome these shortcomings, above all digitalized photos taken from different angles and under different light focussing, while the mobile handling of the tablet and use of magnifying implements always remains a personal and subjective activity, which cannot be verified by others. Computerised treatment of these products allows a resolution as faithful and clear as that obtained by other optic means, an easy, repeatable text management, and the verification of the interpretation offered by the editor. This is why photographic data bases have proliferated; the field of Oriental text studies is no exception¹⁹. Their advantages as an instrument in text edition are evident²⁰.

¹⁵ I have no difficulty in distinguishing between emendation and restoration: I restore/reconstruct a text, I emend/correct a sign; it is as simple as that (n. 22).

¹⁶ In this way I provide the required “explanation” to support the emendation (p. 775), I do not propose a willy-nilly reading (p. 773)

¹⁷ I quoted his review of Tropper’s grammar because our Institute receives *AfO* regularly, and so I was able to read it. I learnt of the *Manuel* much later, on a journey to Paris. I have discussed the photographs in the *Manuel* above.

¹⁸ The case is presented by Fl. Malbran-Labat and C. Roche in their contribution to the Lyon Congress (2002).

¹⁹ On the reasons for omitting photographs in his edition, see n. 7 above. But it is well known that P. had not until now demonstrated a great appraisal of this means of text control (see Spronk’s opinion in the spot quoted in the following note).

²⁰ See in this regard K. Spronk’s review of P.’s book for some good examples of the usefulness of photographs in text editing (“It shows that a good photograph can correct the drawing”).

P. warns Ugaritologists not to be contaminated by my attitude to Ugaritic epigraphy, since the spread of my ideas would cause serious damage to Ugaritology in general (see p. 767: “dangerous”). This depiction of me as a *corruptor ugaritologorum* made me laugh out loud: I never imagined I could have so strong an influence on my colleagues or that they would be so feeble minded as to be taken in by the mistaken epigraphical theory of an incompetent bungler from southern Europe. Forgive me, but this childish smear deserves no further comment.

P. also feels the need to refute my assertion that he has written his book against me (P. 787, n. 59). The fact that throughout the book I am the main opponent ‘à battre’ is evident, as other reviewers have pointed out²¹. But a well-intentioned reader would not interpret my words in the strict ‘material’ sense as suggesting that this was the main motivation for the author to write a book a thousand pages long. There was no need to explain this. Everybody knows that the re-edition of cultic Ugaritic text forms part of more general project undertaken by the new team of the Mission Archéologique Française to revise and assess the work carried out by earlier teams. This aim was declared at the Congress of Lyon 2002, in honour of the late G. Saadé. At this congress P. himself described the epigraphical project he was going to carry out and in fact began with the edition of the hippiatric texts (1981), as I mentioned in my review article. The project was fully justified in the archaeological field given the fragmentary and deficient elaboration of such important aspects like urban and palace planning, domestic architecture, ceramics, minor objects of different sorts and so on (most attention had been paid to selected impressive artefacts and monuments). The text edition was precisely the only aspect which received a thorough presentation.

Allow me to take this opportunity to express my opinion in this regard. I think that the alphabetic texts received an adequate presentation in the work by A. Herdner: transcription with precise and simple epigraphic notes, autcopy and photographs (maybe a translation could be added). The work naturally needed to be improved, with better collations and photographs and the progressive incorporation (in the same or successive volumes) of the new texts (as was the case for instance with the Mari texts and many other collections of Sumero-Akkadian texts). But if according to P. the edition of the literary texts was carried out properly, this means that at least half of CTA (the most important part) was still valid; and accordingly it can be assumed that the edition of the cultic tablets in the AO of the Louvre was also carried out with the same accuracy. In this way the Mission would have kept control of the whole corpus of Ugaritic texts in the most rapid and effective way, as was done by the first epigraphers, albeit in an uncoordinated fashion (MSR, PRU, Ugaritica), a situation that Herdner’s edition sought to resolve at least as far as alphabetic texts were concerned. Instead, this function was entrusted to external hands, and this rival textual edition became an opponent for them to criticise. The pattern chosen mixed text edition with text study and commentary (a multiform strata that Biblical criticism and in general Assyriological text edition have succeeded in keeping separate). The result is a series of disparate volumes of personal authorship, in which the lion’s share is devoted to the commentary, which is not easy to consult – the extreme case being P.’s volumes on the ritual texts.

Moving on, P.’s assessment the “basis of my representation of the Ugaritic religious texts” (p. 786ff.) is blatantly biased, to say the least, and made from a sublime ignorance of what the hermeneutics of ancient texts implies. The assertion that I have not based my interpretation on the examination of the texts²², after more than thirty years working with them, does not need refutation (p. 786). To approach them with an idea, gained from their direct study and the comparison with similar historical systems, in search of their integration into an intelligible whole is the unavoidable stance of any religious-historical

²¹ See M. Kopel’s review of P.’s *Ritual and Cult*, an abridged version of *TR*, in *BO* 62, 2005, 182: “... but if other scholars are cited by name it is mostly to dismiss their arguments. Especially the Spanish Ugaritologist Del Olmo Lete who also wrote extensively on the ritual texts of Ugarit is repeatedly criticised severely”.

²² Unless he means the tablets in their physical entity, which would be another matter.

critic. It is a basic epistemological tenet that any theory starts from a hypothesis: put bluntly, to find something out you have to look for it. Perhaps P. has never heard of the concept of “Vorverständnis”, a basic concept in hermeneutical theory. This inevitably includes a systemic semicircular way of thinking (the hermeneutical circle), nothing to do with the psychological circularity in which P. indulges so frequently, taking as proven assertions which he initially proposed as merely possible²³. When P. says he has ‘no a priori opinion’ of the text content, in fact what he is manifesting is his absolute inability or unwillingness to make sense of the texts in their broader historic-religious context, namely, their actual historical value, without which they are simple dead or moribund ‘letter’²⁴. P. does not seem to have reflected on the method to follow in this regard. If one lacks a ‘model’ – and this seems to be P.’s case – one cannot understand the past (any ‘thesis’ is an effort in this direction). In fact, of P.’s vision of the cult and conception of the royal ideology little can be said, since he himself has renounced any sensible effort to integrate the data and models obtained from the texts of ANE. His only principle seems to be the epigraphic *sola scriptura* and his exegetical method the search for literal meaning as carried out by confessional interpreters. It is rather embarrassing to have to recall those basic epistemological principles. Finally I do not maintain anywhere that “the entirety of the cult at Ugarit of which the king was the ideological head would have been ancestral in nature” (p. 792); what I say is that the specific cultic texts that have reached us have abundant references to the ancestral cult celebrated under the ‘god-to-be’ king as their supreme liturgist²⁵.

This lack of historico-religious perspective is found in his criticism of my evaluation of the ancestral cult. He for instance assimilates the ancestral cults of kings and commoners (p. 788), ignoring the social significance of the absolute monarchy. It is improbable that “the royal cult would have been structurally similar to the cult of the commoners”. Better the other way round: texts reflecting the cult of the common people are lacking and so we know hardly anything about it. P. loses perspective of the meaning of the text even before reading it. These records were official and public, not private. He lacks a perception of what those societies were like, both tribal and monarchic; indeed, the recently recovered list of the divine kings enhances the a priori value of the cultic texts²⁶.

²³ To assert “in other words, dOL knew before he began examining the texts what they would say” can only be labelled as malicious stupidity (and on this occasion I allow myself a disqualifying adjectivation that aims to be merely descriptive), only explicable in a person who has no idea of the hermeneutics of ancient texts. At the very beginning of my involvement in the study of the Ugaritic texts I made a clear statement of the methodology to be followed (cf. *Interpretación de la mitología cananea*, 1984, pp. 11-26). As a matter of fact this was a consequence of my dedication to structural linguistics and the study of the philosophical hermeneutics (from Heidegger and Bultmann to Gadamer and Ricoeur) on which subject I taught a course at Salamanca Catholic University in 1971-1972, at a moment when the hermeneutical problem was particularly topical.

²⁴ Not “letters”, which are always respected and venerated as the voice of history, of our past. His conscious twisting of the term “letter” (n. 53) is pure sophistry.

²⁵ The *khnm* are not cultic priests; perhaps they were in charge of recording this kind of administrative texts, and also well versed in the development of the cultic rites. Nobody will deny the complexity of the Ug. theology, cosmology, liturgy (p. 792) ... but to see those ideological worlds as neutral and independent from this basic tenet is unwarranted.

²⁶ The rest of his argumentation in p. 788 is irrelevant to the point at issue and is nothing more a reassertion of his ‘minimalism’ and ‘literalism’ in dealing with the cultic texts, always having recourse to their semantic opacity. Equally irrelevant is his digression on p. 789, and n. 64; in my opinion more balanced, although hypothetical, is his appraisal of the distinction between the common/great gods cult and of the ancestor cult in p. 791. As for the new gods/kings list see as well P.’s elementary appraisal of them as historical source in my contribution to Sanmartin’s *Festschrift*, p.xxx

As for the distinction between the ‘great’ and ‘dynastic’ gods, this was posited by myself for instance when dealing with the different pantheons, but the distinction is not exclusive, as if personal Christian names and patrons were supposed to exclude the Holy Trinity. The assertion “this (royal) cult was considered to some extent private, just as every family had its private mortuary cult, and was for that reason not formally integrated into the administrative apparatus of the royal city-state” is historically unacceptable (a King is never a commoner) and the clearest consequence of the lack of an adequate hermeneutical model of the ancient Ugaritic society and the anachronistic projection into the past of our present social model. P. does not find the royal ancestor cult in the texts because he does not look for it, and he does not look for it because he is not interested in the socio-historical context the texts imply. The royal ancestor cult is prominent in those societies, as the texts from their neighbouring circles reflect: its committing to writing was an important part of royal power, not readily shared with others in ancient times.

When one has an appropriate right model, hints become serious tokens (n. 66). The historically valid model is a much more ‘objective’ model than the simple reconstruction of fragmentary and circumstantial texts at our disposal that demand a model of this kind in order to become relevant. The setting of the historical context (the model) is the basis in which the texts reveal their meaning. I do not try “to prove the model” and its validity by forcing the meaning of the texts but to allow them to reveal their meaning (p. 790). Evidently I will never convince to some one who rejects the opportune hermeneutical setting of such a meaning, but the meaning *ad pedem litterae* of the texts is very unlikely to reflect historical reality as the complexity of the other texts have revealed: any text has to pass the test of intertextuality through the hermeneutical circle.

Caught by other pressing and more important engagements, I will leave for a second instalment a detailed, more measured assessment of the specific replies P. has felt obliged to give to my criticism and of the “monumental blunders” (P. 785) he has found in my review-article. I will not, however, take into consideration every single one of P.’s punctilious remarks. A cursory reading will allow me to distinguish three groups of criticisms: 1) irrelevant, which simply quote and repeat what was said in the review-article but that do not convince the book’s author, sometimes accompanied by subjective views; b) mistaken, in my opinion, some misinterpretations of my opinions, others simply unconvincing counter-arguments; 3) more or less reasonable, which I will wholly or partially accept. Since I am not interested in saying who is and who is not a competent epigrapher, philologist, literary critic and so on, I will follow the order of the text of the book, without distinguishing between content categories (something that P. reproaches me for, p. 768). P. himself does not do this in his commentary which is the text I am examining in my review, a distinction that is not easily justified in dealing with such complex data which need to be approached from all the possible perspectives at the same time. P. is also aware of this (P. 768). Finally, his apportioning of ‘objectivity’ and ‘subjectivity’ is highly debatable and reflects, as I point out above, his particular ideas of what ‘objective’ is (certainly stones and corpses are the most reliable objective objects). Texts/tablets are the first unavoidable step, just as stones are the basic building element, but at the same time the most in need of comprehension: they must be made understandable and this is their actual ‘objective’ value. It is a question of mental attitude. Without this integration texts mean almost nothing; it is a prerequisite for their interpretation.