A Brief Reply to G. del Olmo Lete's Reply[1]

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[By the criteria of standard alphabetic orthographic practice and of the use and non-use of the word divider by the scribe of RS 24.246, it must be judged probable that the writings {yrgbbᶜl} and {ydbbᶜl} reflect the presence of a vowel between the two tokens of {b} in each name. Though less certain, it must also be judged not unlikely that there was a vowel between the consonants {bh} in the names written {yrgbhd} and {ydbhd}. A brief attempt at explaining the form and meaning of these divine names is offered.]

Prof. G. del Olmo Lete's gracious response to my criticism of his interpretation of RS 24.246:15-28 is much appreciated. I only reply, and as briefly as possible, for the purpose of stating clearly my points and because his arguments appear to me not to affect the conclusions I drew from the data as I described them.

1) We are in complete agreement that the names in question are divine names, i.e., that they refer to entities that, according to the sacrificial text RS 24.250⁺, are named as the recipients of sacrifices like other divinities. I made, however, the assumption, perhaps unwarranted but certainly not illogical, that since, according to del Olmo Lete's hypothesis, the names were borne by humans during their lifetimes, the form of the names should be that of human proper names. Even this is not of primary importance, however, for del Olmo Lete cites compound divine names that have the form of corresponding human names, i.e., with /yaqtulø/ verbal forms (p. 38).[2] However that may be, the following points will deal with the correctness of form as human proper names of the divine names in RS 24.246:15-28. On the matter of the interpretation of these names as referring to Ugaritic kings, see below, final points.

2) As regards the arguments reflecting my view that names attached to humans during their lifetimes should have the appropriate form for human proper names, del Olmo Lete's arguments in no way affect the attested data. We all know that various claims have been made for the double writing of long consonants (i.e., /cc/), but the fact remains that there are no clear examples attested to date in Ugaritic of a long consonant within a word or at the boundary of the two elements of a compound word appearing twice in the orthography. None of the examples cited by del Olmo Lete in his note 11 (p. 37) provides an

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unequivocal instance of the phenomenon.[3] To accept an interpretation that includes the appearance of the phenomenon at least twice, perhaps four times, in a text consisting of only fourteen names is to my mind simply implausible.

3) Del Olmo Lete is certainly correct in distinguishing gemination (i.e., /cc/) at word boundaries from gemination within a word; what he has not done, however, is prove that the two elements of a proper name were considered separate "words" rather than separate (lexical) morphemes. That certain morphemes could be treated as approaching words by Ugaritic scribes is proven by the fact that they occasionally place a word divider before the pronominal suffixes /nn/ and /nnn/; all the more likely, therefore, that the two elements of a proper name were seen as true lexical morphemes, as in fact is demonstrated by the rare use of the word divider between the two elements of a proper name. That being said, however, it is highly unlikely in my opinion that the pronominal morphemes developed independently from the regular rules for consonant assimilation (i.e., the forms with {n} developed from assimilations of the pronominal element, e.g. /h/, to the "energic" element /n/ and the longer forms, secondary {nn} and {nnn}, were "clipped" forms that plausibly were used in post-vocalic position);[4] it is equally unlikely that the lexical morphemes of proper names did not do the same. However that may be, del Olmo Lete has provided no examples of scribes ignoring regular rules of orthography when representing names in which the same consonant appears at the boundary between the two elements with no intervening vowel. Referring to this phenomenon as gemination is, phonetically speaking, not a confusion of two issues as del Olmo Lete avers, for the evidence from proper names is in agreement with the orthographic rule as stated in the previous point. My assertion that the hypothetical proper name /yargubba\`al-/ would not be written with two {b}s was, therefore, not based on what del Olmo Lete perceives as a "grammatical inaccuracy" but on phonetic and orthographic reality: the name /yargubba\`al-/ would have been written {yrgb\`l} because such names are written with only one token of the geminated consonant because that consonant was pronounced as a long consonant (the phonetic definition of gemination within a word), not as two consonants. In spite of an obviously careful search, del Olmo Lete has been unable to discover a single certain example of a same consonant being written twice at the boundary between /yaqtulø/ and a theophoric element outside of RS 24.246, nor for that matter in other environments (cf. below, note 8), that is, he has been unable to prove that what he calls "shared consonants" at lexical morpheme boundaries in personal names were treated like "shared consonants" at word boundaries in common speech/writing. He has, therefore, considerably overstated his case in claiming that it "is simply not true" (p. 37) that the rule for single writing of geminated consonants as I stated it holds for common words and for personal names and, at least as regards this aspect of my criticism, in stating further along that "the picture that emerges from the empirical testing of facts is very different from that provided by Pardee" (p. 39). He has demonstrated that the data are few, but has not discovered data that contradict the rule as I stated it. Simply put, he will have to find better sources than those he has cited if he wishes to convice conservative grammarians of his position, for he has adduced no evidence that is of a nature to disprove the rule as I stated it.

It occurred to me when reading the proofs for this brief reply that I had omitted to observe that at least one element of the psycholinguistic basis for the double writing of "shared consonants" at word boundaries was surely that, when the Canaanite writing traditions arose, there would have been, in a very high percentage of cases, a vowel between the two words: a case vowel, a mood vowel, or a lexically

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3. It should be obvious to any student of Semitics that examples from Late Punic, or from Akkadian, of which the writing system is based on different principles, can bear no analogical weight in claiming the existence of the phenomenon in the Ugaritic writing system.

4. See D. Pardee, *JNES* 43 (1984) 244-45, n. 14.

inherent vowel. Only when most word-final short vowels were dropped at later stages of the various languages did the proportion of consonantal clusters at word boundaries increase.

4) The phenomenon of true gemination, i.e., phonetic length of a consonant, not being represented in Ugaritic orthographic practice applies to my analysis of {yrgbbᶜl}. The situation with the element {hd} may or may not have been similar. I in any case did not provide an adequate discussion in only referring to F. Gröndahl's study of Ugaritic personal names:[5] del Olmo Lete cites examples of the divine name in what may be either post-vocalic or post-consonantal position, that is, after nominal and perfect verbal forms, written both {hd} and {d}, and two examples of what he presents explicitly as -hd in post-consonantal position: {yrmhd} and {ytrhd}.[6] One or both names may, however, have been misvocalized in the past (by myself as well as by others): instead of the old Amorite form /yarim/ the first element of {yrmhd} could be /yarmi/ (< √RMY), the form well known from Ist millennium proper names (i.e. /yarmihaddu/ instead of /yarimhaddu/); one datum apparently shows that {ytrhd} was pronounced {yatarhaddu} at Ugarit, i.e., the writing {ytršp}, which represents /yatarrašap/, from /yatar + rašap/,[7] in line with my preceding point—it is not difficult, however, to imagine a form with a vowel between the two elements of the name {ytrhd}.[8] I do not wish formally to espouse these possibilities, simply to point out that determining the actual pronunciation of the names is not without ambiguity. There are in any case clear examples of /yaqtulØ/ + the divine name being written without {h}, in particular {yᶜdrd} in RS 24.257:17', the by-form {ydrd}, and probably {ytᶜd}.[9] The writing with {hd} of the divine name in RS 24.246 is, therefore, consistent with the writing of {bᶜl} after /b/ with an additional token of {b}, though whether it be owing to the same phonetic

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5. F. Gröndahl, *Die Personennamen der Texte aus Ugarit* (Rome, 1967).

6. I fail to see how the text in which the name *ytrhd* appears "confirms my own interpretation of these as 'divine names' of the Ugaritic kings" (p. 38). The text is a letter in which one person is instructed to entrust (statues of) the deities *Gataru* and *Baᶜlu* to two human beings named *ʾiḫqmi* and *ytrhd* (see D. Pardee, *Les textes para-mythologiques* [Paris, 1988] 91-92, n. 56 ; idem, *JAOS* 115 [1995] 302-3). One may also object to the use of the data from RS 24.246 verso as statistical evidence in favor of one of the interpretations of those data: "Had these texts been taken into account [by Gröndahl], the yqtl- names with the element /-hd/ would have equalled or outnumbered the others" (p. 38). Gröndahl would, of course, only have taken them into account if she considered them to be personal names.

7. Gröndahl, *PTU*, p. 148.

8. A new study of personal names is needed, which would take into account the data from texts published since Gröndahl's work and which would utilize the new data in attempting (1) to distinguish more clearly Ugaritic names from other West Semitic names, and (2) to present more completely the data for the "grammar" and orthography of Ugaritic personal names. The questions of primary interest for the present discussion is whether the "perfect" form in Ugaritic personal names was /qatal/ or /qatala/, whether "perfect" forms of stative verbs and/or verbal adjectives were vowelless (i.e., is {ytršp} < /yatar + rašap/ representative of all such forms?), and whether common nouns in initial position could appear with and without a vowel between the noun and the theophoric element. Note with regard to the second question that while {ytršp} seems to provide proof of a base form /yatar + rašap/, "Akk. *yatar-addu*" (p. 38) does not do so, for, to my knowledge, the name is not attested in syllabically written form at Ugarit (neither Gröndahl nor del Olmo Lete cite such an attestation). The two forms cited by del Olmo Lete, p. 39, {ᵃadnᶜm} and {ᵃadnnᶜm} do not prove that the last question is to be answered in the affirmative. I suspect that it is the standard form /ᵃdōnī-/ in Hebrew anthroponymy that led del Olmo Lete not to claim that {ᵃadnnᶜm} represents simply the double writing of a geminated consonant (i.e., {ᵃadnnᶜm} would represent something like /ᵃadānī + NᶜM/, not {ᵃadān- + NᶜM}); what he does not recognize explicitly is that the first element of the two names need not be identical, for the element {ᵃad} appears in personal names at Ugarit (Gröndahl, *PTU*, pp. 88-89). Finally, in accounting for the orthography of names such as {ytrhd} and {yrmhd} it is possible to envisage that /h/ assimilated to certain consonants more readily than to others, though such a hypothesis would require a more sophisticated study than I am capable of and might also require more data than are presently available.

9. "*y(ᶜ)rd*" cited by del Olmo Lete on p. 38 appears to be an error for *y(ᶜ)drd*, but then follows an enigmatic phrase to the effect that the name only occurs in RS 1.016 (*CTA* 112), which is not the case, for {yᶜdrd} occurs in the text cited above (in reference to a deceased king of Ugarit!), while {ydrd} occurs in RS 18.030:12 (*PRU* 5 97).

process must be judged more dubious than was indicated in my presentation criticized by del Olmo Lete. It should be noted here that the claimed "Ug. allomorph add" (p. 39) does not in all probability exist: the only case that has, to my knowledge, been claimed is RS 4.474:9 (*CTA* 30), where the reading is probably {ʾnᵒdd}.[10] Only {hd} and, much less frequently, {hdd} are attested outside of proper names, therefore, and the question regarding the writings {hd} and {d} in proper names is whether the latter is a true Ugaritic alloform (del Olmo Lete's position) or whether, as appears plausible to me, it is either (1) the result of phonetic processes that reflect standard Ugaritic grammar or (2) representative of another anthroponymic tradition.

5) The cases of {yrgbbᶜl} and {ydbbᶜl} are thus unequivocal, while those of {ygbhd} and {ydbhd} certainly do not lead in the opposite direction and may in fact corroborate the first two. Taken at face value and without special pleading, these considerations lead to the conclusion that the names in RS 24.246:15-28 are not in the appropriate form for human names. If one wishes these names, which, according to del Olmo Lete's hypothesis, were given to living human beings during their lifetime and which may be expected to have been applied to these persons during their lifetimes, certainly in indirect speech and perhaps even in direct speech, to be different from the standard human form, perhaps in a form deemed more appropriate for divinities, that is another matter. Such is not, however, del Olmo Lete's position, for the burden of his argument in the "Reply" is that there is nothing in the orthography of RS 24.246 verso that precludes the analysis of these names as either human or divine names.

6) The presence of a word divider between the elements *yrgb* and *bᶜl* in *CTA* 6 vi 57 does not, of course, in and of itself definitively rule out del Olmo Lete's over-all hypothesis, but as it is consistent with the data from RS 24.246:15-28 in going against the analysis of those names as proper for living human beings, and because the two words in the colophon are part of a living king's titulature, it is certainly an element of weight in the argument.

7) Many have thought the relationship between the elements *yrgb* and *bᶜl* in *CTA* 6 vi 57 not to be those of a compound proper name, and hence to be different from that between the elements of the divine name in RS 24.246. I persist in believing that the structure of the colophon does not support its re-interpretation in the light of RS 24.246.[11] I cannot accept that the traditional interpretation is "extremely unlikely" (p. 36) and would retort that del Olmo Lete has swallowed a 'red herring'.

8) I continue to find the basic hypothesis that the kings of Ugarit would have received "divine" names during their lifetimes to be based on insufficient evidence. What evidence exists that the Ugaritic kings bore true divine names during their lifetimes, or for that matter that they bore another name in the afterlife?[12] What comparative evidence exists elsewhere in the West Semitic world for the practice of

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10. See the textual note *ad loc.* in my *Les textes rituels* (Paris, forthcoming) and here below, note 17.

11. See my translation of the Baᶜlu myth in *The Context of Scripture, vol. I Canonical Compositions, Monumental Inscriptions, and Archival Documents from the Biblical World* (Leiden, 1997) esp. p. 273.

12. I realize that the interpretation of RS 24.247 *verso* is debated, but it is at least clear that it provides a list of the (deceased) kings of Ugarit, quite plausibly for liturgical purposes (cf. D. Pardee, *Les textes para-mythologiques* [Paris, 1988], ch. 5). In this list the kings, perhaps presented as divinized (for counter-arguments to this interpretation, see, for example, those gathered by B. Schmidt, *Israel's Beneficent Dead* [Tübingen, 1994], p. 67-71), bear their "own" names. The attempt by J. C. de Moor to establish correspondences between the sacrificial lists and the king list (*The Rise of Yahwism* [Leuven, 1990] 240-41; second edition, 1997, p. 329) may only be judged arbitrary because (1) there is no evidence other than the highly dubious "*yrgbbᶜl* = Niqmaddu" for the precise equivalences; (2) it assumes that the divinities of RS 24.250⁺ are not identical to those bearing the same names in

ascribing a "divine name" to a living king? Did the kings of Ugarit even have a "throne name," a practice posited by some for the kings of Israel/Judah?—if so, what would have been the relationship between the throne name and the "divine name"? What evidence exists for the '"divine names" being given *ante mortem* when they only apply *post mortem* (p. 36)? The best comparative evidence for multiple name giving of the type envisaged by del Olmo Lete appears to be Egyptian, but how do we explain the general absence of Egyptian influence on the Ugaritic cult, but its strong influence here? Though del Olmo Lete now claims not to consider the question of identification of a specific king with a specific name to be a necessary part of the hypothesis (p. 36), he did on several occasions propose a series of identifications of the names in RS 24.246 with Ugaritic kings, and that chain of identifications was stated explicitly to stem from the identification of {yrgbb⁽l} with Niqmaddu in *CTA* 6.[13] The specific identifications were repeatedly presented as part and parcel of the hypothesis and I believe it possible that readers other than myself might have thought that the identifications did not constitute an unimportant part of del Olmo Lete's over-all hypothesis. I confess to finding that the form of the hypothesis presented in the studies cited in the previous note, even if I did not accept it, had a certain appeal, for it at least purported to be based on a specific textual datum; on the other hand, the analysis of certain divine names as being those of deceased Ugaritic kings without that textual datum is based on much more tenuous arguments having to do with structural elements of various texts[14] and on a reconstruction of the Ugaritic cult that makes a far larger part for the cult of the dead than I believe the data to authorize. I in any case can only applaud the recognition on del Olmo Lete's part that the "identification" was a "highly hypothetical 'exercise' " (p. 36), but cannot resist pointing out that it was not presented as such in the studies cited here above in note 13. The reader may be urged to consult in particular the last presentation in date, a paper presented before a group of our colleagues from other areas of Near Eastern studies, in which none of the phrases normally used to qualify highly hypothetical exercises is to be found. Those colleagues had no way of knowing, short of undertaking their own study of the Ugaritic texts, that they were hearing a highly hypothetical "exercise". The purpose of my criticism to which Prof. del Olmo Lete has replied (and of the more detailed one in *Les textes rituels*) was to show that, in addition to being highly speculative, the exercise was based on a faulty assumption regarding the form of the names in RS 24.246 and on an unlikely interpretation of the words *yrgb* and *b⁽l* in *CTA* 6 vi 57.[15]

RS 24.246; (3) it misrepresents the length and the structure of the king list (contrast his list with my edition in *Les textes para-mythologiques*—my interpretations, though certainly not infallible, were based on epigraphic considerations that cannot be ignored).

13. *UF* 18 (1986) 85-87; *AuOr* 5 (1987) 43-46; *La religión cananea* (Barcelona, 1992) 117-20; *Ritual and Sacrifice in the Ancient Near East. Proceedings of the International Conference Organized by the Katholieke Universiteit Leuven from the 17th to the 20th of April 1991 (OLA* 55; 1993) 52-58.

14. It is asking a bit much of his readers to have thought to separate the structural arguments from the author's interpretation of the colophon, for the two sets of arguments have been explicitly linked in all of del Olmo Lete's publications: though the colophon is not cited in the original study of RS 24.246 (*Salvación en la palabra, Targum – Derash –Berith. Homenaje al profesor Alejandro Diez Macho* [Madrid, 1985] 282-85), one finds there a reference to the first (Spanish-language) study in which the argument from the colophon is presented as "en prensa" (p. 283, note 46).

15. Even de Moor, who accepts and even expands on del Olmo Lete's hypothesis (see above, note 12), has trouble accepting the latter's interpretation of *CTA* 6 vi 57 as it stands or with the emendation thereof by excision of the word divider (see *Rise,* p. 240; second edition, p. 328). One cannot, however, admit with del Olmo Lete (p. 36, n. 9) that de Moor's own interpretation, according to which *yrgb* would be a hypocoristic, is any more plausible, for it does away with the very reason for wanting to connect the two texts, i.e., the presence in both of the name *yrgbb⁽l*. By its *ad hoc* nature it strains an already strained hypothesis to the breaking point and has the side effect of removing the point of comparison between the two texts!

9) As an alternative to del Olmo Lete's hypothesis for the explanation of RS 24.246 verso, which both as a feature of Ugaritic religion and on its grammatical/orthographic merits appears to me to be far-fetched, I would suggest that the names are to be explained as hypostases of the deities whose names feature in the name itself. In part because there is very little comparative data for such hypostases, I do not believe that it is any more "far-fetched" (p. 38) to describe them as bearing names that were "meaningful phrases" than it is far-fetched to accept instances of irregular writing in favor of a hypothesis that sees them as formally regular human/divine names.[16] The best parallels for my proposal come from RS 4.474:6-9 (*CTA* 30), where the names of five hypostases of *ʾIlu* show morphologies different from that of the names in RS 24.246 verso, though each does, as in RS 24.246 verso, contain the primary deity's name (in all cases *ʾIlu* in RS 4.474:6-9).[17] This hypothesis for the explanation of RS 24.246 verso requires the further hypothesis that those deities were honored in a two-part ceremony, one reflected in lines 15-20, the other in lines 21-28, for repetitions of a same name occur in those two segments of the list. Such a division is well attested in the Ugaritic ritual texts (the deities of RS 24.246:1-14 correspond, of course, to at least one, probably two, sections of RS 1.001[18]). The absence of any specific indicator in this text of this division for ritual purposes means, however, that this hypothesis may only be considered as one among others for the explanation of the origin of these names.[19] This hypothesis does at least, nevertheless, correspond to the data of the text itself, however few those data may be, which is more than can be said of del Olmo Lete's hypothesis, with or without his specific identifications.

16. That they are names is sufficient reason for the absence of the word divider between the elements; that absence can hardly be termed an "irregularity" (p. 36) with my explanation. Indeed, the scribe of RS 24.246 only uses the word divider to separate one divinity from another (line 3), not to separate the two elements of a compound divine name (in the first part of the text, compound names are present in lines 1, 11, 12, 13, 14, where the word divider is not used). According to my hypothesis or del Olmo Lete's, the scribe's use and non-use—especially non-use—of the word divider in this text may only be seen as consistent and regular. Be it noted in passing that the word divider in line 3, the only one in the entire text, is structurally important and that it belies the structural symmetry that del Olmo Lete has claimed to exist between the lists on the recto and the verso of RS 24.246 (*Diez Macho* [1985] 284; *Religión* [1992] 46; *Ritual and Sacrifice* [1993] 53-54): in spite of the two lists being arranged in fourteen lines each, the first list contains fifteen names, explicitly marked out as such by the scribe, the second fourteen. It does not seem implausible to hold that the scribe inserted this word divider, the only one on the entire tablet, to make certain that no one should interpret the line as meaning that "*ym* [is] in pair with *bʿl*" (del Olmo Lete, *Ritual and Sacrifice* [1993] 53).

17. The first three names are genitive phrases: {ḥnn ʾil}, 'Grace of *ʾIlu*' (/ḥanānu ʾili/), {nṣbt ʾil}, 'Solidity of *ʾIlu*' (/niṣbatu ʾili/), and {šlm ʾil}, 'Well-being of *ʾIlu*' (/šalāmu ʾili/); the other two are attributive adjectival phrases: {ʾil ḫš} 'the god who shows solicitude' (/ʾilu ḫāšu/) and {ʾil ⸢n⸣dd}, 'the god who arises (on behalf of his worshippers)' (/ʾilu nadādu/). For this interpretation of these lines, see my study cited above, note 10. These names are, like those of RS 24.246 verso, cited in a list of divine names, in this case preceded and followed by well-known divine entities. These names, like those of RS 24.246 verso, are highly uncommon, hence highly marked phrase names. Because of their position in the text, there is little doubt, in my opinion, that they are true divine names referring to hypostasized qualities of the deity *ʾIlu*. On the other hand, this text is certainly of a literary genre different from that of RS 24.246 (on the over-all interpretation of the text, unfortunately hampered by the damage to the all-important line 19, see my commentary cited above in note 10).

18. For the possibility of interpreting RS 1.001:12-20 as reflecting two types of sacrifices, see my forthcoming *Les textes rituels*.

19. The other hypothesis that immediately comes to mind is that the repetitions would reflect a divine genealogy, one for which no other evidence exists. This hypothesis is weakened, or complicated, by the fact that the three divine names in RS 24.250* are not in precisely the same order as their correspondents in RS 24.246 (a difficulty that de Moor [reference above, note 12] meets by placing those three names at a place in the royal genealogy different from that of the names in RS 24.246).