

mrht "oil" from *wrh* "anoint." On page 29, pl. 88, 1, the *ti*-sign needs to be inserted in the blank. On page 30, pl. 88, 2, some speech introduction does seem necessary, perhaps [*p3*] *hn* "the utterance of the birds and fish [is this]: 'These are the momentous things which have transpired for us.'" On page 32, pl. 20, 3 (end), *mn iw't n ph tw n nh* is not corrupt but rather comparable to *qd3wt im.f nt pr-hd*, quoted by A. Gardiner, *Egyptian Grammar*, 3rd ed. (London, 1957), p. 66, n. 12. On page 34, pl. 20, 8, read *n3 iww nhr(nhr?) n hr Imn* "The islands are delightful to Amun, more than the *ksbt*-tree of Opet," and compare K. Sethe, *Die altaegyptischen Pyramidentexte* (Leipzig, 1908-22), §§ 799, 1720, and especially 1693: "They let you live and enjoy (*nhr*) the seasons of Harakhty." On page 36, pl. 86, 2, *nms* probably derives from *nmi* + *s*; compare *q3(i)s* "dispute" from *q3i* "oppose." Thus the whole passage would read "Hail to you who appear in the Head Wrap! The whole land wraps around you (saying): 'Praise!' Praise!" On page 36, pl. 86, 3, *h*" is surely emphatic: "Hail to you who appear in the Appearance Crown! That the premier (crown) of Re appears is (solely) for the purpose of bequeathing his office to his beloved son."

My only real objections to Ms. Condon's work concern the format of her translation. The text is obviously poetry, and most of it is supplied with verse points to mark the ends of lines as well as pause signs to mark the ends of stanzas. Some of these signs are lost, and a few appear to have been misplaced or omitted. Nonetheless, one can readily ascertain where most of the divisions belonged, and Ms. Condon really should have had her translation printed in verse form. This would have helped the reader to follow her comments on poetic style, and it might also have helped Ms. Condon. Her division of the text into seven hymns is never actually explained but appears to derive from an assumption on her part that each side of the three fragments ought to contain one and only one hymn, except where the mention of two different kings on a single fragment requires recognition of one more hymn. I find this most unconvincing. Certainly the Egyptians did sometimes write down their poetry in verse format with one poem to a page, but that practice was exceptional. Usually they ran the verses together in order to avoid wasting valuable papyrus, and that is just how the Turin fragments were inscribed with the sole exception of one partially blank line on pl. 86. Even Ms. Condon seems to have had second thoughts about her divisions, as shown by her comments on pages 38, 40-41, 43, and 45-46. Different kinds of evidence can be brought to bear on the problem: A king's name generally appears only once per hymn, most often in the last stanza but sometimes in the first and nearly always toward the end of the stanza. There appear to be eleven (one in pl. 20 is a restoration) instances of a royal name in the Turin fragments. A change of refrain generally marks the beginning of a new hymn, or hymn section if the king is not named again. There are at least

eight such changes in the Turin fragments. This evidence leads me to conclude that the Turin fragments contain at least eleven hymns (six of them complete) and probably fourteen:

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pl. 87, 1, 1-8		pl. 89, 2 - pl. 20, 1, 3
pl. 87, 1, 8 - ----		pl. 20, 1, 3 - II, 4
---	pl. 21, 10	pl. 20, II, 4 - ----
pl. 22, 1-6		---- - pl. 86, 1, 1
pl. 22, 6-8		pl. 86, 1, 1-5
pl. 22, 8 - pl. 88, 10		pl. 86, 1, 6 - ----

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NEW HAVEN

Probleme der Textdatierung in der Hethitologie. Beiträge zu umstrittenen Datierungskriterien für Texte des 15. bis 13. Jahrhunderts v. Chr. Edited by S. HEINHOLD-KRAHMER, I. HOFFMANN, A. KAMMENHUBER, and G. MAUER. [*Texte der Hethiter*, Heft 9.] Pp. viii + 315. Heidelberg: CARL WINTER UNIVERSITÄTSVERLAG. 1979. DM 130.

The importance of the topic treated here can hardly be overestimated. The entire conception of Hittite history, society, and language depends crucially on views about the relative chronology of the texts. The present work (henceforth *TH 9*) opens with a lengthy survey of the problem (pp. 1-62). The debate over dating Hittite texts has been long and heated, and argumentation has often turned polemical. This fact should be borne in mind when reading the present survey, written by a participant in the controversy, which ends with a critical evaluation of the most recent works of the opposing 'Marburg School' (pp. 55-62). The chief criticism offered is that sweeping generalizations have been made, about both the chronology of texts and 'dating criteria', on the basis of a much too narrow and haphazardly chosen set of data.

The authors of *TH 9* propose to remedy this situation by comparing the body of texts whose dating is 'disputed' with two sets of texts whose assignment to different specific periods (one earlier, one later) is 'undisputed'. In addition to establishing the dates of the 'disputed' texts, this procedure promises a basis for judging the validity of various proposed 'dating criteria' by checking their distribution in the earlier and later sets of 'undisputed' texts.

The desirability of such a large-scale, rigorous comparison is obvious, and so is the crucial importance of properly selecting the sets of 'undisputed' texts (pp. 63-74). Unfortunately, the authors of *TH 9* fail utterly to distinguish between a text (a given composition) and a manuscript (the tablet or

tablets on which a text is written). Thus of twenty-one 'texts' listed as 'by Tuthaliya IV' (pp. 69-71), nine are manuscripts of ritual, festival or other texts whose date of composition either cannot be determined or is definitely Old Hittite. Out of twenty-two additional 'texts' listed as 'of the time of Tuthaliya IV' (pp. 72-73), fully seventeen are manuscripts of either undatable texts or of Old Hittite compositions. It is manifestly absurd to determine the features of the Neo-Hittite language on the basis of such a corpus. Nor can any judgments about Neo-Hittite orthography be based on manuscripts which are copies, since the copyists may have taken over spellings from older models. The set of 'texts' ascribed to Arnuwanda I (pp. 63-65) likewise contains much ambiguous material.

This inclusion of masses of undatable (and even Old Hittite) compositions in the corpora of 'undisputed' texts naturally invalidates the subsequent wholesale rejection of 'dating criteria' (pp. 150-243). Thus, the spelling *pi-e-ra-an* is rejected as a dating criterion because it occurs in late Neo-Hittite (p. 180). However, a check of all texts whose composition can assuredly be dated to Neo-Hittite shows that this spelling does not occur there. Its presence is thus valid evidence that a given text (NB: not *manuscript!*) is Old Hittite (see my dissertation "Ablative and Instrumental in Hittite," Harvard University, 1977). Many more such examples could be cited.

The failure of *TH 9* to carry out its promise (due to the fundamental error in selecting the text corpora) is highly regrettable, because it is likely to obscure several legitimate issues raised concerning the use of 'ductus' in dating Hittite manuscripts (pp. 86-111). For instance, one may justifiably share misgivings about the relative dating of several Neo-Hittite manuscripts on the basis of the proportion of newer sign variants (p. 88).

Also highly significant is the claim (p. 99) that the Old Hittite ms. of the Anitta text, *KBo* III 22, contains the newer form of the sign URU, and that the Old Hittite ms. *KBo* XVII 1 has the newer form of AL. The difference between the older and newer forms of URU and AL is one of the criteria by which the Marburg School dates manuscripts, and the absence of the newer forms in the Old Hittite columns of *StBoT* 20 implies that these variants do not occur in Old Hittite. Verification of the claim of *TH 9* that the newer forms do already occur in Old Hittite manuscripts would destroy the usefulness of these two sign variants in dating manuscripts, thus reducing the already small number of relevant signs. The published autograph of *KBo* III 22 Vs 17-19 does indeed show the newer URU, and that of *KBo* XVII 1 II 19 the newer AL. What are we to conclude from this? Is the claim of the Marburg School, that the newer forms do not occur in these manuscripts, based on an autopsy of the tablets, while the autographs are in error? If so, one misses any explicit statement of this fact in the relevant portions of *StBoT* 18 and

StBoT 20. On the other hand, what is the basis for the opposing claim in *TH 9*?

This case highlights a problem in the presentations of both sides in the controversy: most scholars have access only to the published autographs. Because far-reaching conclusions are drawn depending on the occurrence or non-occurrence of a few sign variants, it does not seem unreasonable to ask that in the future both sides take the following steps: (1) state clearly whether claims about sign shapes are based on inspection of either photographs or the tablets themselves; (2) explicitly account for discrepancies between the results of such an inspection and the published autographs; (3) cite the occurrence of crucial sign variants by line, rather than merely by text, number.

One must certainly agree with the statement of *TH 9* (p. 62) that much remains to be done in the area of dating Hittite texts, and a sequel is promised (p. vi). Unfortunately, unless major improvements are made in the fundamental methodology employed, such a sequel is not likely to contribute significantly to this most important sphere of Hittite studies.

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Hethitisches Wörterbuch. Zweite völlig neubearbeitete Auflage auf der Grundlage der edierten hethitischen Texte. By JOHANNES FRIEDRICH and ANNELIES KAMMENHUBER. Lieferung 4. Pp. 241-320. Heidelberg: CARL WINTER. 1979.

Fascicle four runs from the end of *arāzanda* to *arma*- 'moon, month' (seven pages, incomplete), just past the half-way mark of the letter A. Of the eighty pages, thirty are spent on an accounting of the thousand-odd attestations of the preverb-postposition-adverb *arā*, a vast classification job mainly by verbs which *arā* qualifies; here again common sense would have argued for perhaps a five-page entry and consignment of the oppressive mass to the storage-bins of the *Materialien zu einem hethitischen Thesaurus*. But such litanies of complaint are becoming increasingly repetitious with each passing issue; I shall instead try to fasten on concrete and specific points where critical reaction may add something usable to the discipline.¹

¹ Exactitude seems to be improving; here are some mistakes I noticed on a first reading: page 247b line 36: for *memini* read *memiaš*; page 290a line 41: for *a-ri-e-iz-zi* read *a-ri-i-e-iz-zi*; page 298b line 45: for *KAo* read *KBo*; page 303a line