

Hurriter nach einer eigenen Geschichtsschreibung die Rede sein.

Zu 2. Zuzustimmen ist Verfasserin mit ihrer Gleichsetzung Arnuwandas I. und Tašmišarri (S. 158). Tašmišarri und die noch zur Zeit Šuppiluliumas regierende Taduhepa sind in den hurritischen Texten des Genres itkalzi die namentlich aufgeführten Opfermandanten. Gestützt wird diese Gleichsetzung nun durch folgende Beobachtung: Die hurritische Ritualserie itkalzi besteht aus einem Kompendium von Beschwörungen, die als die (Beschwörungs-) Worte des Wassers, der Zeder, der Tamariske, der Becher des warši-, des Öls, des Silbers usw. bezeichnet sind. In KBo XXI 22 wird eine Reihe dieser Beschwörungsworte in hethitischer Sprache in Kurzfassungen, aufgeführt. Der Text nennt als Opfermandanten „Labarna, den König“ und „Tawananna, die Königin“ (Vs. 14' u. 16'). Nach sprachlichen und paläographischen Kriterien ist dieser Text in die Regierungszeit Arnuwandas zu datieren.

Zu 3. Zu den von Verfasserin aufgeführten verschiedenen Wellen hurritischer Beeinflussung in Hattuša ist zumindest die 2. derselben unter Hattušili III., die bis zum Untergang des Hethiterreichs, also bis Tutḫaliya IV., Arnuwanda III. und Šuppiluliyama „nicht mehr unterbrochen worden sei“, dahingehend zu modifizieren, daß diese Beeinflussung nach Hattušili III. zu Ende gewesen sein muß. Es ist 1. kein einziger hurritischer Text nachzuweisen, der nach Hattušili III. datiert werden könnte. Es ist 2. zu beobachten, daß in den beschriebenen zweisprachigen Beschwörungsritualen bereits zur Zeit Šuppiluliyamas II. die hurritischen Partien durch hethitische Übersetzungen ersetzt worden sind⁴⁾. Diese Beobachtung stimmt zudem darin überein, daß nach Hattušili III. weniger hurritische, sondern vielmehr luwische Einflüsse geltend zu machen sind.

Auch kann Rezensent die Meinung, „die Übermittlung von babylonischem und älterem mesopotamischem Geistesgut erfolgte ausschließlich über die Hurriter“ (S. 158), nicht teilen. Solche Übermittlungen nach Kleinasien fanden wahrscheinlich auch unabhängig von den Hurritern bereits im 3. Jahrtausend über Syrien — Auskunft darüber werden vielleicht die Textfunde aus Ebla geben — und gewiß zur Zeit der altassyrischen Handelskolonien statt.

Sub III. 2.b ist zu verbessern: Die Gründungsurkunden des Tiš-atal ist nicht einer „Göttin PIRIGAL“ geweiht, sondern dem Nergal, zu lesen ist NĒ. <IRI₁₁> .GAL⁵⁾.

Zu korrigieren ist ferner: *ewri* ist natürlich nicht nur „[irdischer] ‘Herr, König’“, sondern auch Epitheton von Göttern⁶⁾.

Die Identifikation des Ortsnamens Nawar mit neuassyrisch KUR Namri ist keineswegs sicher und sollte erneut überprüft werden, da offenbar auf dieser Gleichsetzung die Annahme eines akkadezeitlichen hurritischen Großreichs beruht, das sich von Urkis bis Namri erstreckt haben soll (S. 164, 168, 221). Die Hauptstadt dieses Reichs

mit dem Namen Subartu soll gar Urkiš⁷⁾ gewesen sein (S. 237). Die Annahme eines solchen hurritischen Großreichs widerspricht auch den historischen Gegebenheiten: Denn nach dem Zerfall des Akkade-Reichs entstanden eine Vielzahl kleinerer, unter anderen auch hurritischer Staaten, die natürlicherweise an manchen der alten Traditionen festhielten.

Sub III 4b. Hinzuzufügen sind Bo 2373 (Forrer BoTU II 25 n. 7) und 300/u (unv.).

Sub III 5: Unbegründet ist die Annahme, daß der Kult der Šawuška von Ninive in Boğazköy auf einen Kult der Akkade-Zeit zurückgehen könnte. Natürlich bestand der Kult der Ištar in Ninive schon seit der Akkade-Zeit. Das hat aber nichts damit zu tun, daß die Hurriter in Arrapha ihre Šawuška mit der Ištar identifizieren konnten und sie — ebenso wie später in Hattuša — als Šawuška von Ninive verehrten.

Sub III 6: Es erscheint voreilig, hethitische Überlieferungen als unsinnig abzutun. Verfasserin sieht eine Verballhornung alter hurritischer Überlieferung in dem folgenden Absatz eines hethitischen Rituals, das anlässlich eines neuer Tempelbaus für die Ištar vollzogen worden ist. Damit die Göttin von ihrem neuen Tempel Besitz ergreifen möge evoziert man sie von den folgenden Städten herbei: „Dann ziehen sie die Gottheit von Akkade, Babylon, Susa, Elar (und) Hursagkalama — aus der Stadt, die du liebst — aus den Gebirgen usw. herbei“. Warum sollte nun diese Absatz nicht in den Ritualzusammenhang passen? Sind hier doch Orte genannt, in denen seit alters berühmt Ištarheiligtümer bestanden haben⁸⁾; ist es nicht geradezu selbstverständlich, daß sie die Priester von diesen, die Göttin bevorzugten Städten, herbeirufen werden.

Sub IV findet sich ein weitschweifiger Exkurs „Struktur des Elamischen“, zum Zweck eines besseren Verständnisses des altelamischen Narāmsin-Vertrags. Bemerkenswert ist hier allenfalls der Versuch, das Elamische als Ergativsprache zu erklären; hierzu jedoch sind die kritischen Bemerkungen von G. Wilhelm zu vergleichen⁹⁾.

Kapitel V befaßt sich mit Problemen der Geographie. Aber auch in dieser gewiß nützlichen Abhandlung bleibt vieles fraglich und manches zu korrigieren. So erscheint es Rezensent z.B. höchst unwahrscheinlich, daß Narāmsin und Rīmuš (V 2b u. S. 236, 246) mit dem Begriff „Ober Meer“ nicht wie auch Sargon das Mittelmeer, sondern den Urmia- oder Van-See gemeint haben sollten. Rezensent hält es zudem im Gegensatz zur Verfasserin f. durchaus wahrscheinlich, daß den romanhaften Bericht Sargons und Narāmsins ein historischer Kern zugrundeliegt¹⁰⁾.

Zu korrigieren sind die Zweifel an der Lokalisierung von Ebla mit dem Tell Mardīh (S. 221 n. 137f); auf S. 2 ist Ebla dann bereits am Balīh gelegen.

⁴⁾ Vgl. V. Haas-H. J. Thiel, op. cit., S. 60ff.

⁵⁾ Diese Lesung erwägt Verfasserin jetzt auch in *RIA*, 4. Bd., 509.

⁶⁾ 2.B.: KUB XLV 21. — 4: [p]u-d[u]-uš-du šar-ri DİŠKUR a-kab e-ḫar-ni ha-ur-ni-ia / KI.MIN SAL D[he-bat]. — 5: [n]a-aḫ-ḫa-ab al-la-ni e-[š]e-na-ša / ...

Die ungefähre Bedeutung: „Es ist wirksam der König Teššub, führender König auf der Erde. Desgleichen (= es ist wirksam) das Weib Hebat, thronende Herrin in den Himmeln“. — Vgl. V. Haas-H. J. Thiel, op. cit., S. 257.

⁷⁾ Urkiš war übrigens nicht die Stadt des Wettergottes Teššub, sondern Sitz des Kumarbi. Kultort des Teššub ist die Stadt Kummī im östlichen Nordmesopotamien (Sub I).

⁸⁾ So war z.B. der Kult der Inanna in Hursagkalamma so berühmt, daß die Inanna-hur-sag-kalam.ma^{KI} auch in Kiš verehrt worden vgl. J. Renger, „Götternamen“ (in: *HSO*), S. 144. (PRAK 2 C 37: AS 8).

⁹⁾ G. Wilhelm, „Ist das Elamische eine Ergativsprache?“, *AMI* (19

¹⁰⁾ Vgl. auch M. T. Larsen, *The Old Assyrian City-State and Colonies*. Mesopotamia 4, Copenhagen, 1976, S. 277.

Hat der Leser zu Beginn der Lektüre den Eindruck, daß Verfasserin zu einem Textverständnis gelangt ist, mit dem sie nunmehr viele der offenen Fragen nicht nur der Hurritologie, sondern auch der Hethitologie und sogar der Akkadistik, zu beantworten in der Lage ist, so verblaßt diese Hoffnung mehr und mehr, und er stellt am Ende fest, daß er so klug als wie zuvor ist.

Berlin, Oktober 1977

VOIKERT HAAS

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Johannes FRIEDRICH - Annelies KAMMENHUBER, *Hethitisches Wörterbuch*. Zweite, völlig neubearbeitete Auflage auf der Grundlage der edierten hethitischen Texte. Lieferung 1. Heidelberg, Carl Winter, Universitätsverlag, 1975 (80 S.). Preis: DM 40,-. ISBN 3 533 02357 5.

With pleasure Hittitologists greet the appearance of this revised and enlarged second edition of the *Hethitisches Wörterbuch* of J. Friedrich, the first edition of which served the needs of Hittite research for over twenty years. It is much easier to criticize a dictionary than it is to write one. We congratulate the editor for the successful appearance of this first installment of her new dictionary.

The editor of this thoroughly revised Friedrich is well-known in the discipline. Annelies Kammenhuber's¹) publications began with her doctoral dissertation, which treated the Hittite infinitive system (1954-56), and included in subsequent years books and articles on every phase of Hittite language and culture. Her viewpoints are not always representative of the Hittitological consensus, but they are always thoughtful and useful in furthering scholarly debate. Unquestionably she is well qualified to produce a second edition of Friedrich's dictionary.

If there is any objection to her two parallel projects in Hittite lexicography (the *HW*² and her *Materialien zu einem hethitischen Thesaurus*), it is that it leaves no room for another Hittite Thesaurus in the German language. It has been known for some time (certainly it was clear as of the appearance of StBoT 15 in 1971) that, assisted by funds from the Mainz Academy of Sciences, Professor H. Otten was assembling lexical files which were to be used for the most complete Hittite dictionary (or thesaurus) possible, including even unpublished texts. Surely an economical division of labor would have suggested that K. write the more compact work (*HW*²) and Otten the definitive, encyclopedic one. That plans for a Hittite Thesaurus of her own were pursued leading to the publication in 1973 of the first fascicles has not improved the prospects for the appearance in the near future of a large dictionary by Professor Otten.

On this side of the Atlantic Professor H. G. Güterbock and the reviewer have begun work on an intermediate size Hittite dictionary for English-speaking scholars, which will fill a need felt since the *Hittite Glossary* of E. H. Sturtevant (2nd ed., 1936) became hopelessly antiquated many years ago. The Chicago dictionary project has been aided by a grant from the National Endowment for the Humanities, Research Tools Division.

The existence of the two concurrent dictionary projects (K.'s and the Chicago Project) should benefit the field.

Her dictionary commences with A words; ours with L words. No overlap will exist until both are halfway complete. Each should be able to benefit from the other's publication, from the aspects of lexicographic method and exegesis of specific texts. Certainly fascicle one of *HW*² has already helped us here in Chicago.

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A proper evaluation of any book should answer two questions: (1) Is the procedure which the author has adopted for the book adequate? And (2) has the author consistently followed that procedure?

A dictionary of a dead language is text oriented. Because the author cannot elicit forms and syntactic structures from a living informant, he cannot answer many questions of interest to linguists. He must be content to organize his limited data in such a way as to provide clear answers to philological questions, both syn- and diachronic. As far as his evidence allows, he must supply at least the following information: (1) a translation or translations in the target language with degree of certainty indicated, or a statement that a translation is not yet possible; (2) the periods of attestation (both date of the copy and probable date of composition); (3) the types of texts in which the word is found (historical, mythological, legal, etc.); (4) the inflected forms attested accompanied by one or more references selected for their representative value (for high frequency forms complete listings are not to be sought); (5) grammatical information such as the gender of nouns, voice of verbs; (6) sample clauses in which it occurs, so as to facilitate determining translation and to illustrate its syntactic relationships; (7) other words in the source language of similar (synonyms) or contrary (antonyms) meaning; (8) probable cognate words within the source language; (9) logograms and abbreviations; (10) source language, if the word is of foreign origin, and meaning of the word in that foreign language; (11) bibliography of important contributions to the study of the word or its contexts including dates of each contribution; (12) information from lexical or bilingual texts; (13) occurrence of the word in personal names, divine names or geographical names.

The above I regard as the essential information which a dictionary should supply. Additional details could include: (14) speculations regarding the word's cognates in other Anatolian languages and in other branches of the Indo-European family; (15) statistics on the frequency of the word's occurrence in designated periods or text types (K. gives on -a² the information that -aš, -an and -at occur in Old Hittite in the ratio 3:4:1; in addition she often merely indicates "häufig" or "selten"); (16) cases where the word has been borrowed into a foreign language; (17) words erroneously posited in previously appearing standard dictionaries together with correct interpretation or reading. K. has included some of these additional details. At the request of several colleagues she has included (14) wherever possible. She has also supplied information occasionally on (15).

K.'s conception of a Hittite dictionary, as detailed under eighteen headings on pages 8-12, agrees in all essentials with the above delineation. Respecting (1) she has seen fit to employ not only one or more question marks but also boldface versus standard type set to distinguish degrees of

certainty as to the translation(s) of the Hittite word. The question marks alone are surely adequate. And, since it is the German translation rather than the Hittite word itself which is uncertain, should not the different typography apply to the former rather than the latter?

Periods of attestation (2) are identified as aheth (Old Hittite), aheth. ("Orig.") (Old Hittite in the old script), jheth. (Young/New Hittite), frühjheth. (early New Hittite, c. 1400 [cf. p. 35], corresponding approximately to Middle Hittite in the terminology of Güterbock, Otten and others), spätjheth. (late New Hittite, c. 1250-1200 [cf. p. 9]). K. often differs from many colleagues in the dating of particular texts. Those texts grouped by Laroche in his *Catalogue des textes hittites* (CTH) under numbers 131-147 she regards in general as late New Hittite with archaizing and lists in the dictionary with the notation spätjheth. or just jungtheth. Other texts, not assembled in a particular place in CTH, are usually to the Middle Hittite period by Otten, Güterbock and others, on the basis of either an early (OH or MH) ductus or orthography. In this reviewer's opinion the evidence assembled so far is more than sufficient to date most of the texts in CTH 131-147 to the Middle Hittite period on the basis of ductus as well as grammar and lexicon. One can safely speak now of a Middle Hittite ductus which is clearly recognized on the originals (collated) of both the Madduwatta and Mita texts, and others in this group.

Text genres in which the word is found (3) are indicated in the headings only when the word is restricted to one or two genres (cf. *aha*, *ahrušhi*). Otherwise they are noted sporadically in the body of the citations (cf. *aimpa*, *aggala*). A consistent identification of each citation with an abbreviated label (rit., myth., leg., pray., etc.) would have been preferable.

Inflected forms (4) are made the principle for arranging the context citations and discussions of meanings. This arrangement obscures both a clear presentation of the paradigm and an ordering of the selected context citations according to meaning (6). The two tasks are discrete and warrant the additional space required for separate exposition. K.'s context citations are rarely full enough to include the entire syntactic period bounded by the conjunctions and/or sentence particles. Space saved by being selective in the choice of citations can be expended for the transcription of a fuller context, which allows the user who lacks adequate library resources to observe all the more important syntactical relationships: nominal cases, adverbs, sentence particles, word order, verbal endings. It affords him a basis on which to evaluate the lexicographer's interpretation. Usually selective, K. rarely succumbs to the temptation to give long lists of text references without transcription, translation or discussion. But cf. *ali*¹, *alliya*, *alwanzatar*, *ambašši* meaning 3, and especially *NINDA a-a-an* (p. 45), wherein spite of admitting "Belege aus den Texten unvollständig", she reproduces forty-four lines of citations. Since all these citations for *NINDA a-a-an* were already printed in *Al.Heth.* (1974) 153, which K. used elsewhere in this fascicle, one could have economized here.

Other Hittite words of related meaning (7) are noted by K. (cf. *altanni*, *annalla/i*, *annari* - A). Although one may occasionally take issue with her formulation of the differentiation between two terms (e.g., *annalli* and *karuili*),

K. has not neglected to attack the problem. Antonyms are more difficult to identify than synonyms and are cited less often (cf. *alpu*, *ammiant*).

Attested cognate words in Hittite (8) are regularly adduced, as are logograms and abbreviated writings (9). Of the latter only a few have been overlooked. Since logograms are listed together at the head of each article, where they can be easily located, abbreviated writings should also be listed together and in a spot easily found. In the discussion of *ambašši*, for example, one has to comb the many lines of form citations to find out that the following abbreviated writings are known: *am-iš* [...] KUB 43.53 ii 8, *am-ši-in* Bo 3481 i 8, KBo 15.7 obv 10, *am-aš-ši-in* KBo 8.57 obv 4, rev 10. I could not find in K.'s article any citation of *am* in IBoT 3.148 iii 45, iv 1 (with *ki-ia* for *keldiya*).

The immediate source of a foreign word (10) is often all that can be ascertained, since the word in question may have been naturalized in that language from still another. *DUGaganni*, when seen together with Akkad. *agannu*, Ugar. *'agn*, Hebr. *'aggan*, Aram. *'aggana*, Egypt. *'ikn*, is almost certainly a *Wanderwort* of uncertain source. The first element in *Gišallantaru* is another case in point. In *Orientalia* 35 (1966) 390-1 I entertained the possibility that the element *allan* was that which occurs in the "Semitic word" **allan*, which occurs in Hebr. *'elōn*/*'allōn* "oak, terebinth", Ugar. *'ahn*, and Akkad. *allānu*, and that it reached Anatolia from a Syrian source which had not undergone the *ā > ō* shift of Canaanite. Goetze (*JCS* 22, 1968, 17) correctly pointed out that, while *'allān(u)* could be originally Semitic, it was more likely "Mediterranean". Thus while my term "Semitic" implied an origin, his "Mediterranean" described a distribution. K.'s wording (sub *allantaru*) unfortunately gives the impression that I thought only of Akkadian. Moreover, I nowhere posited, as she does, "Komp. mit akkad. *a*. — über das Hurr." (p. 56). Akkadian > Hittite and Akkadian > Hurrian > Hittite loan substantives (*tanani*-, *hazzizzi*-) normally show i-stems. The absence of any other known Hitt. word for the oak in view of the virtual certainty of its presence in Hittite times around Hattuša (cf. Bittel, *Hattuša: the Capital of the Hittites*, New York, 1970, p. 13), makes my identification especially likely.

The determination of the gender of substantives (5) can be approached on the basis of distinctive endings and by observation of concord. Occasionally it happens that a form, which according to its ending is thought to be neuter, is modified by a common gender adjective. r-stems (e.g., *hegur*) are especially noteworthy examples. Among K.'s corpus *aiš* shows concord with both genders. K. writes "auch c. ab Murš. II". The forms themselves are scattered through the long documentation, so that one cannot see the nature of the evidence, much less why it should be restricted to Muršili II and later.

Bibliography for special points (11) such as gender should be listed where the information on the point is given. On the gender of *aiš* Sommer, *HAB* 110, 176 n. 5 was omitted. A. Ünal's *RHA* XXXI/1973, p. 36 contribution was in the citations from augury texts of KAxU *nai*-. The translation "Schnabel/beak" for KAxU/*aiš* in these texts, not registered in *HW*¹ (though probably implicit), was noted prior to Ünal in Hoffner, *EHG*, p. 21 (sub "beak"). Other bibliographical omissions are minor:

GIŠalanzan- discussed also by M. Poetto, *Ist. Lombardo (Rend. Lett.)* 107 (1973) 29f. Under *akkant-* the vocabulary entry *akkantaš hulali* [...] was treated by Landsberger in MSL II p. 115. In Al.Heth. 88, following Goetze, it was considered as a description of an insect. On *aggatar* the vocabulary KBo 1.31 rev. 20 Akkad. *mūtu* = [Hitt. *aggatar*]; the restoration of the Hitt. is K.'s; neither AHw 691 nor MSL XIII 145.20, which she cites, restored the Hittite, which might have been [hi-in-kañ] or even [LUMUTU]! With regard to the practice, begun by Friedrich, of quoting letter and oral communications, authors should use this procedure only when it is clear precisely what was communicated. Personal communications do not enjoy explicit guarantees of a printed publication. In addition, if the communication is subsequently published by the colleague who made it, there is no longer need to cite both. Under UDULagahalanti- I am cited in EHGL (1967) 42, orally, and Al. Heth. (1974). It is not made clear what I said to K. which was not already in EHGL. I did not discuss the term in Al. Heth.

Bilingual evidence (12) includes both vocabularies, generally well covered by K., and equivalences derived from Akkadian and Hittite versions of annals and treaties and from Hitt. translations of literary texts written in Akkadian and sometimes Sumerian. The detailed comparison of such bilingual versions is less in evidence. Not all vocabulary evidence is clearly reported by K. All columns of the line should be cited: Sum., Akkad., Hittite. Like Friedrich before her, K. often omits the Sumerian, and sometimes even the Akkad. (a-4). On *aiš* "mouth" two locations are cited, von Soden "AHw 872a (heth. zerstört?)" and KBo 13.2 obv. 9. AHw 872a refers to KBo 1.38: 7-18 which has been edited in MSL XIII 245. The Weidner copy in KBo 1.38 shows clearly the remains of a broken right hand column in which the Hittite translations were recorded, as noted also in MSL XIII 245. If K. had gone beyond AHw to KBo 1.38 and also cited it for her users, there would have been no problem. Actually there was no need to cite the text in the first place, since the Hitt. is lost. The passage KBo 13.2 obv. 2'-9' is probably from the series ugu-mu, as reflected in the Akk. entries of lines 5-6 and the Hittite of lines 2, 3, 6 which have the pronominal suffix "my". The series ugu-mu consists principally of names of body parts with the possessive suffix "my". The Hittite line 9 should be translated: "When a child is born, (s)he holds the mouth away" or "When a child is born and holds the mouth away". A further new instance of *aiš* in a vocabulary is found in KBo 26.20 (+ KBo 1.44 + 13.1) II 1 (Erim-ḫuš), which reads: ka-b[a?]-x = e-peš pi-i = a-iš-ši-it (mistake for iššit?) me-mi-ia-u-wa-ar "speaking with the mouth".

Hittite words may occasionally occur as elements of proper names (13), either divine, personal or geographical names. These instances constitute important evidence, which should not be overlooked even if one must be cautious. Overlooked by K. are the PNs Ayale (Laroche, NH number 4), Akuwa (NH number 19, rel. to NA⁴aku-?), Alalimi (NH 22, rel. to alalima- "gurgling [of river, stream]"), Anna (NH, p. 337). Place names URUAlpaššiya (Ertem, 8), HUR.SAGAllina (Ertem, 174; Al.Heth., 150).

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a-1: on the related *eni* cf. now Laroche on *aši, uni, eni* in *L'annuaire du Collège de France, 76^e année, résumé des Cours de 1975-1976*, p. 445 [originally in Hittite *aši, uni* and *eni* were com. nom. sg., com. acc. sg. and neut. nom.-acc. of the same demonstrative pronoun].

-a-2: the -an in *akišš-an* is probably the particle, rather than acc. sg. of pronoun. There is no other evidence to substantiate the idea of Josephson, FSP 318, that *ak-* could be construed in the same way as the verb *ištar-k(iya)-*. And K.'s supposition that -an is a mistake for -aš is totally gratuitous. HW¹ 201 indeeds cites *tā < ta + e*, but does not posit pl. nom. c. *-a, as K. charges. In KUB 24.8 i 6' space would allow [HU]L-uš. But (1) the writing is always LÜ.HUL-... in KUB 24.8, and (2) the trace in Walther's copy does not fit [HU]L.

-a-3: since HAB (unlike StBoT 8) is not old script, the distribution there of *ū-ug-ga* may not tell us much about Old Hitt. usage. -aia: "and, also". "Datiertes ab Murš. II. (SV II. 178)" refers only to the form in the stereotyped expression *nukan kašma a-pa-a-at-ta-ia NIŠ DINGIR-LIM zik šarratti* "you shall also thereby transgress the divine oath", which contains -aia only if one assumes a form *apat* "thereby" (as does Friedrich, SV I 78, which is the page which ought to have been cited). If one assumes *apatta* "thereby" (HW¹ 25), then the conjunction is only -ia. To use this as evidence for "ab Murš. II" also assumes that the copies before us are Muršili originals, not later copies, a fact not yet established. A new example of -aia not yet cited in the discussion is *hadan harrannaia* (KUB 44 63 ii 12').

a-4: on the participle, the instr. is also attested in KUB 41.4 ii 19-20: *nuššan Ū.HI.A TI-an IM-an 4 a-a-an-te-et pār-aš-tu-uh-hi-it dai nan šallanuzzi*, "he takes with four warm p.'s and raises it". The reading NINDA-a-a-an-te-et is less likely, since then one expects *paraštuhhit-a* "and with p.". *paraštuhhama pahhurra* occurs in KBo 21.13 iv 4', which may indicate the use of this object with fire. Under NINDA-an the impression is given that a-a-an^(HI.A) is the only form attested; hence, "(undecl.)". In fact K. already cited earlier NINDA-a-a-an-ta (KBo 17.65 rev 21f.) and NINDA-a-a-an-ta/da-aš. The statement "ein Gebäck in hurr. (beeinfl.) Rit." obscures the word's occurrence in rituals and festivals which show no such influence, e.g., CTH 439, 470, 633 (EZEN *haššumaš*: no Hurrian divine names!), 727 (Textes myth. [14], Hattic moon myth & rit.), etc. The neut. gender of NINDA-a-a-an constitutes no evidence for *Fremdwort* status, since NINDAšuppi and NINDA-miumiu also occur (cf. Al.Heth. 211, which should be added to the bibliography). a-a-da-aš NINDA-aš (KUB 8.62 I 13) should also be explained before one can say that NINDA-a-a-an has nothing but folk etymology to connect it with the participle of a-.

aha: Occurs already in the MH Mešedi text IBoT 1.36. There is no reason to claim "fehlt in hatt.", since it occurs in the Hattic portion (!) of KBo 21.90 (rev 43') as a-a-ha-a. This text shows MH ductus (Otten, KBo XXI, S. VI). The word is called out by the LÜALAN.ZU_x and the LÜ.MEŠNAR (latter always plur.!).

(DUG)ahrušhi-: Is there a criterion to distinguish *Lehnwort* from *Fremdwort* other than inflection? If not, then *ahrušhi-* is clearly the former. But to what purpose is all this hair-splitting?

ak-: Statements like "Keine Ortspart., kein -za durch a. erfordert" are highly misleading. The so-called local particles (-kan, etc.) are "called for" by many factors in the clause, surely not just the verb. The same is true for -za. These facts are well-known to all Hittitologists. So, while it may be true that *ak-* in itself requires neither -za nor -kan, it is by no means out of the question that this verb would occur in a context in which either -za (cf. Hatt. III 38) or -kan (cf. Kamm. *Mat. Heth. Thes.*, *ak-* pp. 13, 17, 18!!) also occurs. I also question inelegances such as "wegsterben" for *arha ak-*. What is wrong with "aussterben"? Such wooden literal translations from Hittite into German help neither German speakers nor those of us who in such cases have to check a dictionary.

What is the purpose of the starred forms *a-ak-hi, *(a-)ag-ga-ah-hi? The attested 1 sg. forms, which are -mi forms, are indeed unexpected. Even if they appear in late texts, one cannot argue from silence that *akhi or *aggahhi prevailed until c. 1300, as K. seems to do. The form "a-ak-ta" of KUB 10 18 v 12 is clearly a-ra-an-ta in the copy! On the *akir/ekir* phenomenon K. has not treated H. Otten, *StBoT* 17 46 fairly. The correction of KUB 14 14's *ekir* to *a-kir* is based on Klengel's collation at Otten's request. Why question it? Otten's study makes it very likely that OH knew only *akir*, whereas NH both *akir* and *ekir*. The unique form *ak-kán-tu* in KUB 36 113: 5' deserves to be noted. Impersonal construction *arha akkiškanzi* is not "ab Murš. II", since it occurs in Supp. I's treaty with Hukkana (rev 27, §28). Spellings of *akkiš-* attributed to reigns of Hittite kings are worthless if not tied to the copy date (palaeography). A Muršili text recopied once or twice in the following century can give little evidence of spelling habits of Muršili's own time. *aiakki* b: all writings are Akkadian (construct + genitive).

EZEN *aiali*: Why is this not Akkad. *ayalu*? A bald denial is not enough. Similarly EZEN *aiari*.

aiš: In an article as full of citations as this it would have helped to give immediately the evidence for the use of the determinative UZU. That *tapuša pai-* in MS obv. 8f. does not indicate facial paralysis but simple malfunction is shown by KUB 44.4 rev 7f. *nahšarriyaddat weritešta nuššikan KA x U-iš tapuša pait IGI.HI.A-wa KI.MIN 9 UZUÜR.HI.A KI.MIN*. Hysteria brought on malfunction of mouth, eyes and other body parts. The literal translation "went to the side" should not obscure the generalized meaning "failed to function" required by the various parts of the body. This passage was edited by G. Beckman, *Hittite Birth Rituals* (Yale diss., 1977), p. 225ff. (text L).

I would be more cautious about assuming etymological relationship between *aiš*, *iššalli*, etc. and *iššallant-*. We do not know the significance of the latter term.

akkant-: Again the organization of citations according to the principle of inflected form rather than meaning renders evidence virtually inaccessible. The interesting translation "Marren(?)" is not accompanied by the passages where K. may believe it appropriate. I might guess Hatt. ii 1f., 52f., KUB 18.16 ii 1, 5, 7, KBo 3.3 i 16, 24. The bibliography might have included Otten, *HTR*! There at least one finds a discussion of the word's contexts.

aggatar: Some remarks about the synonym *henkan*

would have been useful. There is not even a cross reference to h.

aggati-: This is the Luwian cognate to Hitt. *ekt-* "net" (Hoffner, *Finkelstein Mem. Volume*). The Luwian word was discussed in *Al.Heth.* 125. Page reference to Laroche's edition of Gilg. in TM [123] missing.

akni-2: KBo 13.241 + KUB 44.4 rev 22-23 (cumulative line count) shows on the photo something like GAM + ? + NI. The first two wedges are aligned differently than the *Glossenkeile* in lines 22, 31, 32, 33. Otten is right; the reading *ak* is problematic.

(NA⁴)aku-: The principle should be observed in all lexicography, that one proceeds on the basis of what is clear. Broken and obscure passages should not be given equal value to clearly preserved ones. Two of the latter exist for this word: Ull. 2nd Tabl. B II 5-6, and KUB 21.19 + II 14-17. They establish that the *a.* is to be found in the sea, that it was used by women as an adornment, and that it was recovered from deep water as something of value. Why "warm sea-shells" is any more difficult to imagine than "warm stones/pebbles" totally escapes me. And as for roads described as NA⁴akuwant-, one needs to know where these roads are located. Near the coasts they could have been covered with shells! Far too much weight is given to a particular etymological view here, and far too little to careful textual analysis. Seashells (cowries) as jewelry in Anatolia are archeologically attested in Çatal Hüyük (cf. *Anat. Stud.* 16, 1966, plate L b). The initial inspiration for my proposal "sea shell" for (NA⁴)aku- came from A. L. Oppenheim, "Mesopotamian Conchology", *Or NS* 32, 1963, 407-412. The Akkad. word which started Oppenheim on his road to discovery of Mesopotamian conchology (NA⁴ayyartu) occurs as an Akkadogram at Boğazköy in KBo 11.11 iii 7: 14 UZUZI.IN.GI 14 NA⁴A-IA-AR-TUM 1 TUGku-re-eš-šar ta-lu-up-pa-an, "fourteen astragali, fourteen cowrie shells, one rolled up k. head-dress". *a.* occurs also in the Palaic ritual KBo 8.74 + 19.156 + KUB 32.117 + 35.93, ii 17-18, which shows rather old script (Otten: "ältlich"): a-ku-u-uš-š[an-?]. DUJGIR. KIS-ia išhuwawani, "we pour/scatter cowrie shells [...] into the G.-container". For cowries as jewelry in early Syro-Palestine cf. V. Maag in H. Schmökel (ed.), *Kulturgeschichte der Alten Orient* (1961), 449.

-aku: Why is there a cross reference to -ku? Why should HW² devote separate treatment to -aku and -ku?

akuka-: In KBo 21.22: 53 read: a-ku-ka[-aš] QA-TI.

akušša2: cf. above under *aku-*.

akuwakuwa-: Probably not = *akuka-*, since the latter is part of the vine.

(GIŠ)allaiani-: "v.Schu. HDA 43 Z. 16" not pertinent. Read rather: x-x-al-la-a-a[(n e-eš-du nam-ma-a)]t ištalgan ešdu KUB 31.86 ii 16 with restoration from KUB 31.89: 6.

GADalalu(ša): Add: KBo 17.103 + KUB 46.48 obv 16' GADa!-la!-lu-wa-az (with sign interchange).

GIŠallantaru: cf. above.

GIŠalanzan-: cf. M. Poetto, *Ist. Lombardo (Rend. Lett.)* 107, 1973, 29ff. Wreaths (KUB 35.142 i 8, 12) and a gate (Tunn. iii 25) are made from it, and possibly it has foliage (*lahurnuzzi*; KBo 19.142 ii 16, if so to restore).

allapah-: In Hitt.-Luw. rituals *a.* translates Luw. *tap-*.

NINDAallapuri: Probably a miswriting of NINDAlalla(m)-puri- (cf. *Al.Heth.* 170).

SALalhuešra : Read KBo 21.32 rev 6 [BE-E]L SISKUR.-SISKUR *ú-e-ha-at-t[a]*.

*ali*⁴: Important now is KUB 48.106: 12'-13' "because the field(s) of *a-li-ia-aš* are not dear to his majesty, but the field of a man is dear". The same pair *ali-/alli-* and *lagan-* may occur in KBo 19.160 obv 11'-12'. Cf. the CHD treatment sub *Lagan-*.

alili : Determining the probable identity of birds is extremely difficult. Nonetheless one should at least summarize the evidence. This bird is found near rivers (KUB 18.5 ii 1-11), sits in the *harau*-tree (poplar?) (KUB 18.5 ii 7), and is observed in association with the eagle (IBoT 1.32 obv 4-10).

GIšalkišta(na) : On inflexion cf. Neu, *StBoT* 18, 1974, 123-4 with n. 295. The logograms (Akkad.) *ARTU* and (Sum.) *PA* "foliage, leaves and needles" probably do not stand for *alkištana* "branch". On *PA-an* "frond" at Boğazköy, cf. Güterbock in *FsOtten* 85f.

alpa : Since the neut. pl. *alpa*⁴ exists, it is probable that *i.dalu alpan uizzi* in KUB 33.21 iv 15 is not acc. sg. (so K.) but a neut. sg.: "an evil cloud comes".

alpan : The principal passage KUB 7.1 + KBo 3.8 i 1-3 reads: *mān DUMU-laš al-pa-an-za našmaššikan garatieš adanteš*. The *našma* indicates that both phrases describe symptoms, not causes. Therefore *a*. cannot be "hexed". *a*. describes a condition which is probably general, affecting the entire body; the second phrase describes the effect upon the *karatiš*, a specific area. A further clue comes from the text KBo 23.85 i 1'-2' with dupl. 279/d: [(*pi-ra-an* 10)] GA.KI[N.AG *al-pa-a-an* 10 GA.]]KIN.AG (dupl. + TUR) *kap-pi-ša* [(*GIšir-hu-u-i-ia*) ... (*pé*)]e *har-kán-zi*; cf. Otten, KBo XXIII S. VI. Since the Hittite word underlying GA.KIN.AG was neut. (H. Hoffner, *JAOS* 86, 1966, 27ff., *Al.Heth.* 121), *alpan* is the neut. sg. of *alpan* and therefore pertinent here. As it describes the cheeses (which are hardly "hexed"!!), it must have to do with either their visual appearance or their odor. Either would be appropriate as a symptom of dire disease in the KUB 7.1+ passage. The same GA.KIN.AG *alpan* probably occurs in KUB 10.68: 4-5, 12. and KBo 24.40 obv. rt. cd. 8'.

alpašši : cf. *Al.Heth.* 150.

alšant : Neu (*StBoT* 18) doubts the reading *al-ša-a[n-za]* in KBo 3.22: 4 and reads: *al?-x-x-x[...]*. The other occurrences speak for a contingent of troops. New is KBo 1.44 + 13.1 + 26.20, iii 20 (lex.): (Sum.) *erín-nir-ri-a* = (Akkad.) *ni?-ru* (for *nirāru*?) = (Hitt.) [*al-š*]a-an-za ERÍN.MEŠ-za "auxiliary troop".

alšuwar : With Neu (*StBoT* 18 17) read: [*tá*]k-šu-wa-ar on basis of dupl. Bo 3234 and 617/p.

GUDalu : cf. above on (DUG)ahrušhi-.

alwanzah : Add to pres. forms Bo 3617 i 19 (Otten, *AfO* 23) *a[(l-wa-an-za-ah-ha-an)-zi]*. Since, as Otten showed in his review of L. Rost, *THeth* 2, the line count given there is often faulty, one has to exercise care in using the references in this article to lines in *Theth* 2.

alwanzatar : Cf. Neu, *StBoT* 18, 1974, 77f. Add to lex.: [*uh₄*] = [*kišpu*] = *al-wa-an-za-tar* KBo 26.11 obv? 2.

alwanzena : Add to lex.: [*uh₄*] = [*kāšipu*/*kaššāpu*] = *al-wa-an-zi-na-aš* KBo 26.11 obv 3'.

amandant : Read KUB 5.24+ ii 32]-ni-uš ha!-an-da-an-te-eš MUŠEN.HI.A.

amiyara : Evidence as cited by K. is insufficient to prove a-stem, since the forms sg. acc. *amiyaran* and *PA₅.HI.A-uš* com. pl. could derive from r-stem. The real evidence, which in fact supports K.'s claim, consists of two passages which contain *PA₅-aš* nom. sg. com.: RS 25.421 (*Ugar.* V = 169): 44, and KUB 29.7 rev 51-52. The former K. adduces as "Sg. G.?", but this is impossible. In both the Sum. and Akkad. versions one finds "she is a canal", not "she is a ... of a canal". Hitt. adds the word "like": "she is like a canal". The KUB 29.7 rev 51-52 passage runs thus: "They set them (models of curses) in the little boat" *našta GIšMA ištappešnaš PA₅-aš ištappešnaz parā ID-kán anda pé-e[-da-]i* "and the canal of the reservoir carries the boat out of the reservoir into the river". The *-kan* on *ID* does not indicate new clause. For examples of single sentences with both initial and internal sentence particles see discussions by Carruba, *Die Satzeinleitenden Partikeln* 20ff. and Josephson, *FSP* 399ff. As for LÜ.-*PA₅-a-*, one might prefer "Kanalinspektor" to "Deichgraf" (both terms in Siegelová, *StBoT* 14), but what other objection can K. raise? How does the "*a*./*PA₅*-Material" speak against Siegelová's translation? And why at the head of K.'s article does she write "Sumerogr. *PA₅* (= akkad. *palgu*)" with no bibliogr., when the explicit evidence of RS 25.421 rev 44 left uncited gives Sum. *PA₅* = Akkad. *rātu* = Hitt. *PA₅-aš* (*amiyaraš*)?

annari : A: Translating *lam-mar tar-t[a-an]* as "gesagte, genannte Stunde" implies the existence of a Hitt. verb *tart(a)* "sagen/nennen", which to my knowledge does not exist.

Chicago, Ill., August 1977

HARRY A. HOFFNER, JR.

* *

Heinrich OTTEN und Christel RÜSTER, *Keilschrifttexte aus Boghazköi*. 23. Heft (insbes. Texte aus Gebäude A). Berlin, Gebr. Mann Verlag, 1976 (4to, XII + 50 S. Autographien). Preis: DM 54,-. ISBN 3 7861 2234 X.

Volume 23 of this series contains copies by Heinrich Otten and Christel Rüster of texts and fragments mostly recovered during the seasons 1932 (/b) through 1939 (/i) with a scattering of fragments from more recent digs. Many of the fragments were found in Building A on the acropolis. Most are what Hittitologists call "ritual texts" and "festival descriptions". Of these a good number are of a Hurrian origin or background (numbers 12-54). Another category of texts represented are the oracle texts, copies of which can be found in numbers 105 and following.

Frau Rüster has again prepared indices of proper name for the volume, which naturally adds to its usefulness.

As with other volumes in this series, Professor Otten and Frau Rüster have placed us in their debt by their prompt and diligent publications. It is a joy to be served by skilled and hard-working colleagues. We offer to these friends our deep gratitude.

1 All known pieces of CTH 472A are now available in hand copies. To CTH 471-473 may now be added the fragment KUB 9 5, which in lines 2-4 especially resembles

VIII. SENTENCE STRUCTURE AND PARTS

The treatment in this chapter is very good and underlines the usefulness of this corpus for Syntactic analysis.

Par. 62. Word Order in Main Clauses. More space than that available for a review such as this would be required to study the relation between word order and logical predicate. It must suffice here to say that in some cases where the verb assumes first position, such as EA 20:11 and EA 29:29 (cited by Adler, p. 95), there is an emphasized adverbial element, *danniš-ma*, which is the logical (not the grammatical) predicate. The role of enclitic *-ma* for emphasis on a particular word has been duly noted by Adler (p. 82); for its definition as indicator of the logical predicate, cf. Rainey, *Israel Oriental Studies* 6 (1976), 51-58. Since adverbs, with or without *-ma*, are frequently the logical predicate of a clause, their role in clauses where the verb has lost its traditional final position should be studied.

By way of final observations, we will concentrate on EA 29, which Adler himself has singled out for its distinctive syntactic features (p. 94). We have already noted above its use of the genitive after *umma*. Its occasional word order with the verb in first position is discussed by Adler (p. 95). To these may be added some lexical features. Adler (p. 267) has posited a meaning, "Zwischenraum," for *binuttu* (*bīnūtu*?), which would be a *hapax* in the Akkadian lexicon. In EA 29:22 he wants to interpret *ina binutti* as a phrase meaning something like "in the meantime," and compares it with Hebrew *byn* and Ugaritic *bn*, "between." One cannot say at this point whether Adler is right or not. On the other hand, he is evidently wrong about another supposed WS cognate in EA 29. He took the expression *gu-ū-ul libbišu* (EA 29:29; similarly also in lines 81-82) as "joy of heart," and assumed a cognate with Hebrew *gyl* (p. 276). However, the same expression appears in Hattusas and *AHW*, 927b, is surely correct in reading *qū-ū-ul* (EA 29:29) and deriving the form from *qūlu*, "silence, quietness."

The one occurrence of *tarāšu* in EA 29 is paralleled by the usage in EA 26 (cf. Adler's glossary, p. 333). It is a unique construction, having "words" (*amāte/amātu*) as the d.o. (in both texts). The queen mother is to "present" the words before Pharaoh. Although this is not identical with the expression so prevalent in the Canaanite texts (*tarīš ina pāni*...), it may be the connecting link leading to the WS semantic development (which evidently corresponds to the Heb. idiom with *yāšār*).

Therefore, we may conclude by noting that EA 29 seems to have the closest links of all the Tušratta correspondence to the scribal school of Canaan.

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HETHITOLOGIE

FLORILEGIUM ANATOLICUM: Mélanges offerts à Emmanuel LAROCHE. Paris. Éditions E. de Boccard. 1979 (8vo II + 334 pp.).

This handsome volume is a fitting tribute to Emmanuel Laroche, one of the most eminent living authorities on

the ancient Anatolian languages and indeed on all things Hittite. A photograph of the honorand and a list of his publications are followed by thirty-four articles by colleagues representing nine countries.

The philology of the Anatolian languages is quite properly the subject most extensively covered, with nine articles: four on Hittite and/or Luwian, one on Palaic, one on Lycian, one on Old Phrygian, one on Armenian and one on Hurrian. C. Brixhe, using the terminology of modern Linguistics, derives the Hittite "directive" case in *-a* from a polyvalent Indo-European particle **-e*, which in most languages was replaced in the directive function by the accusative *-m* and itself survived only in certain adverbs such as *ἀνω*, *ἀμαρτή*, from thematic stems. He thus takes further a theory first advanced by Laroche. A Kammenhuber, in her increasingly telegraphic style, shows how the Hittite chain of enclitics frequently disobeys the normal rules of assimilation for the sake of clarity. F. Josephson and J. Puhvel discuss etymologies: Josephson derives Hitt./Luw. *tarpa/i-* "good (or bad) luck" from Indo-European **dherebh-*, **(s)terp-/trep-*, **sterbh-*, Hitt. *tarpalli* and *tarpassa-* "substitute" from Indo-European **trep-* "turn"; Puhvel compares Hitt. *sah-* (*sāhi*) with *sanh-* (*sanhzi*), arguing that the former means "clog (with dirt)" and derives from Indo-European **dhyēE* (cognate with Latin *finus*, *faex*), the latter "rinse, flush out", from Indo-European **snā-*. O. Szemerényi's article is devoted to refuting a claim by Calvert Watkins to have found a g-laryngeal in Palaic. R. Gusmani treats the Lycian verbal form *smmati*, deriving it from the Hitt. *ishima-* "bond". M. Lejeune refutes a theory of Otto Haas about Old Phrygian phonology. M. Salvini traces lexical correspondences between Hurrian and Urartian. G. Dumézil defends the view that **dw* became (*e*)*rk* in Armenian against an attack by F. H. H. Kortlandt.

Cuneiform Hittite as a separate discipline is represented by six articles. H. G. Güterbock publishes four "stray Boğazköy tablets" which have come to his notice at different times: a join to KUB X 37, a duplicate of KBo XXI 42, a letter of a queen to a king, perhaps an answer to the letter KBo XVIII 2, and a small piece of a ritual text (EZEN). Sedat Alp communicates an important letter found at Maşat which establishes the site as that of Tapikka and names three mountains near by which are elsewhere associated with the river Zuliya. So this river can now be identified with the Çekerek (Scylax), which flows not far from Maşat. (This article was first published in Turkish in *Belleten* XLI.) H. Otten argues that KUB XIII 9 (CTH 258) and a recently discovered duplicate must both be copies of an old text of Tudhaliya I (II) the original of which has not come to light. P. H. J. Houwink ten Cate contributes an edition of KBo XIX 76+ and KUB XXIII 36+ and shows that these two texts belong to the Comprehensive Annals of Mursili II, years 12-15 (between KBo IV 4 and KBo V 8). A. Archi discusses the "humanity" of the Hittites, adducing evidence from several texts of the Old Kingdom containing advice to a prince or governor. Fiorella Imparati discusses the controversial Middle Hittite text KUB XLV 47 describing a ritual conducted by an unnamed queen and her four sons. She concludes that the queen is Nikalmati and her son "the Priest", Kantuzzili, the author of the prayer KUB XXXI 10 and subsequently general under Tudhaliya and Suppiluliuma.

Hieroglyphic Hittite (which owes so much to Laroche's manual of signs) is represented by two articles. J. D. Hawkins publishes a stela, half at present in Hama and half in Beirut, which is said to come from Sheizar near Meharde, and compares it with the Meharde stela in Aleppo. It transpires that the author of SHEIZAR was the wife or mother of the author of MEHARDE. Mme E. Masson contributes a new edition of the well-preserved stela from Karahöyük, now in Ankara.

There are three articles on the Old Assyrian texts. P. Garelli publishes a tablet acquired by the Louvre containing a number of interesting personal names, V. Donbaz a list of female slaves and their owners from the Assur collection in Istanbul Archaeological Museum. K. Balkan publishes a tablet from Kültepe containing two words denoting parts of a wagon and a plough respectively and discusses in this connexion the construction of these two implements.

Six articles deal with religious matters. R. Lebrun presents what is known of Lawazantiya, a cult centre in Kizzuwadna. A. Caquot interprets Ugaritic *klt* as the name of the Hittite goddess Kulitta. O. Masson draws attention to two Greek inscriptions containing a female personal name Koubaba(s), suggesting that the cult of the ancient goddess Kubaba still survived, and refutes a claim to see in the toponym Gdanmaa the name of the Phrygian Earth-mother. G. Neumann discusses the gods and goddesses of the Lycians and finds that the majority of their names are of Hittite or Luwian origin, though Ertemi and Pedrita are clearly the Greek Artemis and Aphrodite. P. Demargne comments on the prominence of the goddess Athena on Lycian monuments and on the self-glorification of the Lycian dynasts through their identification with heroes like Heracles and Perseus. J. Leclant describes a number of Egyptian antiquities — inscriptions, statues and statuettes — found in Asia Minor in temples of the cult of Isis, or at least relating to that cult.

Finally there are eight articles which can be grouped under the general heading Art and Archaeology. K. Bittel and E. Uzunoglu each publish a bronze "nail-figure" of a well-known type, though Bittel's is remarkable for having a bird on its head. Both authors assign these figurines to the period of the Hittite Old Kingdom, and Uzunoglu concludes that they were used as foundation deposits in the construction of religious buildings. J.-C. Courtois discusses the terra-cotta "libation arms" which have been found in numbers at Boğazköy and also in Cyprus but only rarely elsewhere, and suggests that the Cypriot specimens owe their presence there to the numerous Hittite exiles who were banished to the island. Nimet Özgüç contributes an article on the seal designs from Kültepe and Acemhöyük, collecting and distinguishing the various divine types depicted in these designs. Tahsin Özgüç publishes a bearded lead figurine of the Kültepe period, now in the Hermitage Museum at Leningrad, and a steatite mould excavated at Kültepe itself. Ekrem Akurgal assembles an impressive amount of evidence tending to prove that the palace of Kapara at Tell Halaf should be dated to the end of the eighth century B.C. Machteld Mellink collects the evidence for contact between the West Phrygian Midas and the Luwian king of Tyana, and the cultural borrowing that resulted. Nezihi Firatli describes with numerous photographs the scant remains of the temple and theatre of Bithynion-Claudiopolis, modern Bolu.

In conclusion I would add a few random thoughts on the Hittitological articles.

Professor Güterbock (p. 144) refers to the LÚ.MEŠ Ú.HÜB and asks "Who are these men, and why are they called deaf?" I should like to draw attention once again (cf. BiOr. XXXII, 385) to Sommer's suggestion of 1938 that Ú.HÜB might be a misunderstanding of the sign ZUR = *kalû* "lamentation priest" (SAI 2404, Deimel 122d, or 213b of his 3rd ed.).

Professor Kammenhuber puts unnecessary stumbling blocks in the reader's path by writing "vor" for "nach" (p. 186 line 27) and by her use of the sign "=", unspaced, to separate enclitics in bound transcription, as distinct from its normal use, with spaces before and after, a distinction which proved too subtle for the printer at the bottom of p. 186. Further, those wishing to check the evidence for the extensive tables of enclitics on pp. 189-96 are not greatly helped by the introductory remark "Für Schreibungen und Zitate s. O."

Miss Imparati's arguments for identifying the Queen and the Priest of KUB XLV 47 with Nikalmati and Kantuzzili respectively seem well founded, though the simultaneous existence of a "house of the priest" and a "house of Kantuzzili" in the late text KUB XXVII 13 (note 26, p. 176) is certainly disturbing. But the royal genealogy proposed by Kammenhuber, which Miss Imparati seems to accept, is unduly compressed, and I have argued in a forthcoming article that the only possible schemes are those which separate Nikalmati from Suppiluliuma by three generations. This would mean that the Kantuzzili of XLV 47 would have been Suppiluliuma's great uncle, and the Kantuzzili who was acting as field-commander at the beginning of Suppiluliuma's reign must surely have been another, younger man, perhaps the king's brother. In the Offering Lists he appears among Suppiluliuma's sons.

Mme Masson is to be congratulated on her beautifully clear drawings of the Karahöyük inscription and of the Yazilikaya relief reproduced on the cover of the volume. Unfortunately the all-important name of the country mentioned in the inscription still defies decipherment.

Mr. Hawkins tentatively compares the funerary stela of SHEIZAR with the inscription of the mother of Nabonidus from Harran. The origin and history of this genre have now been fully investigated by him in a paper read at the 26th R.A.I. at Copenhagen.

Oxford, January 1980

O. R. GURNEY

* *

Johannes FRIEDRICH-Annelies KAMMENHUBER, *Hethitisches Wörterbuch*. Zweite, völlig neubearbeitete Auflage auf der Grundlage der edierten hethitischen Texte. Lieferungen 2-3. Heidelberg, Carl Winter, Universitätsverlag, 1977, 1978 (25 cm., each 80 S.).

Professor A. Kammenhuber [K.] continues to produce fascicles of her Hittite dictionary. Her persistence in this enterprise, in spite of health problems, is gratifying to all of us who are her colleagues. We owe her a debt of gratitude for the arduous labor which the transforming of file cards into coherent dictionary articles entails. The wealth of information which she incorporates in her articles is of

particular value to colleagues who do not have extensive files of their own. And we of the Chicago Hittite Dictionary [CHD] staff often find the analyses offered by HW² quite helpful.

On the other hand, it should be stated at the outset that fundamental differences between K.'s approach to the data and that of most of her colleagues (including the CHD editors) remain evident in these additional fascicles. Paramount among them is her atypical approach to text dating, which has led her to assign to many texts a date quite different from most of her colleagues. This aberrant approach has elicited much criticism in the past and will continue to do so, as she incorporates her ideas in a reference tool so basic as a dictionary. Even in those cases where she dates a text the same as her colleagues her system of indicating the date allows imprecision. We have adopted a system for the CHD for indicating probable time of composition and probable time of the copy which we believe allows as much precision as we feel is justified in each case. A colleague may not agree with our dating a particular text as OH/MS (originally composed in Old Hittite, but now existing in a copy showing the Middle Script), but at least he knows by our notation precisely what we think. HW² in such a case writes „Aheth in Abschr.", which does not indicate whether the copy is MS or NS. We also object strongly to the use of the term "aheth Orig." for what we denote as OH/OS. There is no evidence which supports the contention that texts were not recopied during the OH period. How do we know that a text in OS is the original? There are three Old Script duplicates for the first series of the Hittite laws, Friedrich's A, M and K. Should all three of these be denoted „aheth Orig."? See H. G. Güterbock's identification of a copying mistake by the scribe of KBo 6.2 (Laws, copy A), which shows that it is not an original (in *Frontiers of Human Knowledge*, Acta Universitatis Upsaliensis, Uppsala, 1978, p. 126 with n. 2).

K. also holds an unusual position among Hittitologists on the question of terminology for the noun case in -a in Old Hittite. While most prefer to employ the term "locative" for the case in -i and the stem-form locatives and devise a descriptive term for the -a case such as "directive", "terminative", or (CHD's) "allative", K. alone insists that the Hittite case in -a is a "Lokativ" answering the question "wohin"? (defended by K. in E. Neu & W. Meid, *Hethitisch und Indogermanisch*, p. 115-142) While everyone concerned seems to agree that the -a case denotes the point toward which the action moves, the use of "Lokativ" by HW² for both -a and -i cases only adds to the confusion.

The two fascicles under review contain the entries *annauli* through the first part of *arahzanda*. Including these two fascicles K. has used 200 pages for -a through *arahzanda*, for which Friedrich in HW¹ required 12. This yields a ratio of HW¹:HW² = 12:200, reduced to 1:17. We estimate that the HW¹:CHD ratio for the letter L will be about 1:11, although the CHD's page size is larger and its space is used in different ways than HW². A general comparison of the procedures and formats of HW² and CHD was given in my review of HW², fasc. 1 (BiOr XXXV, 242-246).

Comparing HW² fasc. 1 with fasc. 2 and 3, one notes that the articles in the latter are longer. This could be

viewed as either good or bad news, depending on how the expanded treatment is organized and what the prospects are for bringing the HW² to completion in the foreseeable future. Since K. also has the *Materialien zu einem hethitischen Thesaurus* [Mat. heth. Thes.] in which to develop her ideas more fully, one hoped for a more concise treatment in HW². The CHD has no publication analogous to *Mat. heth. Thes.*, but plans to publish more detailed treatments in journal articles, which can be simply cited in the dictionary articles. Dictionary editors owe it to their readers not to digress into extended presentations of evidence which are better advanced in journal articles or monographs.

We do not propose to offer here a detailed critique of HW² fascicles 2-3, in which we would analyse and seek to improve upon each entry. When the CHD reaches this part of the alphabet, it will doubtless handle many of these entries quite differently. Then there will be an opportunity for readers to see all the points on which the two dictionaries differ. The following remarks are based upon a sampling of the material covered by these two fascicles, evaluated in the light of the CHD lexical files and the experience gained from the preparation of the L-fascicle. Individual remarks which follow are introduced by a HW² page number and a letter which denotes the upper left (a), lower left (b), upper right (c), or lower right (d) column of each page.

81a: *annaz* (adv.) "früher". HW¹ added the translation "einst". Obviously it is always used with the preterite. Occasionally with an -*sk-* form: e.g., KUB 22.40 ii 34. Usually clause initial, but not always (KUB 13.35 iii 1, KBo 3.3 i 14). In view of *an-na-za-ma* (KBo 18.32:3) the entry should read either *annaz(a)* or *annaz, annaza*.

86d: *arha aniya-* (vb.) in KUB 9.15 + 39.52 iii 20ff., considering its context, *arha aniya-* must have the meaning "clean out (a house)" (reinigen).

88d: *aniyan* (n.) "Arbeit" is a good addition, not recognized by HW¹. The passage KBo 1.42 i 18 was known to Friedrich, but never led him to posit *aniyan* in HW¹. So far, so good. K.'s contention that forms of *aniyan* underlie writings with KIN is not impossible. But one should star the obliqui *aniyant-*, since one cannot prove its existence on the basis of the KIN writings and the gender argument. *aniyatt-* is also neuter in many passages. And HW²'s restoration KIN-*an[-ta]* KBo 15.25 + 24 i 25/36 is gratuitous.

88d: *aniyatt-* (n.). The excursus on KIN with the hundreds of text references seems unnecessarily long (six columns!).

94a: *annišan* (adv.). The note "mit einem -š-!" is based on Friedrich's observation in SV 1 (1926) 151f., although one would not guess this from the presentation. In fact most of the valid points of interpretation on this word derive from SV 1. "a. noch SV" is intended to alert the reader to the Kup. occurrences, although "weitere Belege bei SV" would have been clearer.

94b: *annitalwatar* (n.). K. deserves credit for determining that the word is to be read *annitalwatar* (not *anniriwatar*). But if she thinks this word is based on the noun *anna-* "mother", she should attempt to explain its suffix. In this case it is important to call attention to the existence of this suffix in the Hittite words *hannittalwa-*, *hannitalwana-* "litigant" and *hannitalwaes-* "to litigate".

95c: URUDU *ankurinu* (n.). Why the unsupported assertion "nicht Akkadogr."? Even if in its occurrences in Akkadian texts it possibly represents a loanword, one should offer more reasons for dismissing it as an Akkadogram in Hittite texts than what is written here. What about the unexplained -u ending, which looks like an Akkadian nominative?

95d: *anš-* (vb.). The iterative spellings *a-an-aš-ki-* and *a-an-ši-iš-ki-* exist, but so does *a-an-ši-ki-* KBo 23.23:77.

97b: *antaki-* (n.). It seems to me quite possible that *a-* had (at least in OH or MH) an *a*-stem. From such a stem the allative form *antaga* = *šša* "into his room" (KUB 36.44 i 12, OH/MS?) and the locative *antaki* = *šši* (KUB 11.20 i 13, KUB 45.3 iv 17) could be explained. Alongside this old *a*-stem (or perhaps a subsequent development?) one should recognize the stem *antakitt(a/i?)*- (or *antaki-* with -t- augment in oblique cases) which underlies the forms *antakitti* KUB 11.25 iii 9 (var. of *antakišši* KUB 11.20 i 13), and *antakitti* = *šši* KUB 7.5 i 7. I hardly think that *antaki* in *antaki* = *šši* is "undecl.", or that *antaga* = *šša* is "fehlerhafte Archaisierung".

97c: *andan*, *anda* (adv., prevb., postpos.) The organization of this article, in which evidence for *andan* and *anda* is presented together, makes it difficult for the reader to find what he needs to interpret passages which employ one of the two. Even if the editor believes that no clear semantic distinction can be made between the two forms, a discrete exposition of the evidence for each would have been more helpful. Equally useful would have been a detailed presentation of the evidence from alternation in duplicates to show that *andan* and *anda* were at least occasionally synonymous.

108d: ^{LU}*antiyant-* (n.). Although allusion is made to Hitt. ^{LU}*gaena* and its Akkadogram ^{LU}*HATĀNU*, ^{LU}*HATNU*, K. does not explain how these differ in meaning from ^{LU}*antiyant-*. ^{LU}*HATĀNU* in Hittite does not seem to have the same wide range, "male inlaw", which is attested for Akkadian texts, but rather the restricted meaning "son-in-law". If so, the same might be true for ^{LU}*gaena*. Why then should we settle for "son-in-law, bridegroom" as the translation for ^{LU}*antiyant-*? Furthermore I fail to see how our present understanding of Hittite marriage and family structure (so HW²) excludes Balkan's excellent suggestion that *a-* represented an "eingeheirateter Schwiegersohn". It should be quite clear that the point of the passage in the Ašmunikal decree KUB 13.8 obv. 14f. is that only an *a*-type bridegroom would ever be expected to be "given" to a family outside the É.NA₄! In all other cases the bride would enter her husband's house inside the É.NA₄. And although the word *a-* does not occur in the text, I still (cf. Hoffner in Goedicke and Roberts, *Unity and Diversity* 137, 143 n. 9) agree with Güterbock's idea (in Kramer, *Mythologies of the Ancient World* 152) that the Illuyanka myth plot can only be adequately understood against the background of this type of marriage.

109c: *antušša-* (n.) Again the organization of the article does not allow for a clear presentation of the evidence for spellings in manuscripts from OH, MH and NH. One cannot see which spellings are used in what periods without a good deal of searching in out of the way places. Somewhere in *one place* the references (with dates) should be given for the spellings with -du-, those with plene -tu-u-uh-, etc. Although on page 109d "Aheth *anduhša-*" is good, it simply doesn't convey the point which needs to be

made. According to my examination of the evidence, the writings with -du- (e.g., *an-du-uh-ša-aš*) are the rule for OH and MH. Occasional examples might turn up of *an-tu-uh-ša-* in OS or MS, but it appears that the prevailing practice for OH and MH scribes was to use -du- in the syllabic writing of this word. The one OS example known to me is KBo 17.1 i 23. This example is particularly intriguing, since it constitutes an exception to Otten's well documented observations about the 17.1's preference for the voiceless CV signs as compared to 17.3's use of the voiced ones (StBoT 8:50ff.). Almost all examples are MS: KUB 15.34 ii 30, KUB 23.77:51, 81, KBo 16.25 iv 10, KUB 30.10 rev. 23, KBo 16.31:3, KBo 23.23:58, KBo 8.35 ii 23, KUB 24.6 rev. 14, KBo 24.41 i 19, KBo 21.18:13, KUB 40.36 i 12. KBo 12.62:13 might be MS. KBo 3.60 iii 12 is OH in NS. On p. 110c-d K. is probably right to see an accusative in LÜULU^{LU}-na-az... *dai*, but surely this is acc. + -a- "and, but" + -z. There is no form -az of the reflexive particle; only -z and -za exist.

113b: I find it most annoying to see references to "Stef." and "Gü." in the line above it, which so far as I can see have no further indication of the article or book being cited. I suspect that "Stef." in connection with KBo 4.14 refers to Atti della Accademia Nazionale dei Lincei 20 (1965) 39-79 where KBo 4.14 was edited by Stefanini. But since this article is not even cited in CTH, many users would have no convenient source to locate it. "Gü." in connection with IBoT 1.36 (the Mešedi text) cannot allude to a published edition, since his edition of this text has not yet appeared.

119b: One should not write "Paradigmen für *an-tu-uh-ša-*" if one intends to include other spellings (e.g., *anduhša-*, *antuwaḥḥiš*, *an-tu-u-uh-ša-*) in the paradigm listing.

122a: *anturiya-* (adj.). Although KUB 31.86 + 1203/u (StBoT 15.45) is quoted, the full wording of the joined tablet is not given: *arahzenaššakan anturiyašša ANA AN.ZA.GAR* can hardly be cited in support of a translation "einheimisch (inländisch) — ausländisch". The text says: "Let no one affix (lit. put) wood and torch to an outer or inner tower" (i.e., a tower outside or inside of the city wall). "Foreign tower" doesn't work here at all.

125a: "*anzellu*" is surely an Akkadogram, as I explained in AOAT 22:84 with n. 16, citing also the unpublished text Bo 898 right 6. To these one should now add KUB 18.9 ii 20.

125b: -*apa* (particle): To say that this particle is attested through Šuppiluliuma II can only be true, if one counts OH texts in late copies lying about in that king's archives! KUB 24.13, cited immediately after this claim, is MH in NS. The latest firm evidence for use of -*apa* in a new composition is Šuppiluliuma I's treaty with Hukkana. It is handy, of course, to have a full listing of contributions on this particle. But one should make it clear that the fundamental study is Carruba's Or 33 (1964), which retains its worth even after the appearance of Josephson, *Particles*. Nor is Carruba's premise about the dating of the occurrences of -*apa* unacceptable to most Hittitologists. The text references given on p. 126ff. and dated after Šupp. I by Kammenhuber are mostly late copies of OH and MH originals. To prove her point she needs an original composition from the period after Šupp. I, and I see no evidence for this in her citations. What is the observation that -*apa* is not found in the OH texts in StBoT 17 (Zalpa

story) and 18 (Anitta) intended to prove? She has already admitted that it occurs in OH/OS, even if in her opinion it is less often used in OS than in OH/NS.

150d: *apa-* (demonstrative) As in the case of other high frequency words with many spellings attested, HW²'s organization is not conducive to a clear presentation of the evidence. Furthermore it is too easy to confuse OH/OS and OH/NS evidence in HW²'s arrangement. On p. 131d KBo 3.27 (OH/NS) is quoted for *a-pa-aš* in the paragraph devoted to OH/OS, introduced by "(Abschr.)", but not quoted at all under "Aheth. [in Abschr.]". Under *apun* (p. 132) "Aheth. [in Abschr.]" *a-pu-un* KBo 20.34 obv. 8 (OH/MS) has been omitted. Under *apel* (p. 132) "Aheth." should have the qualification "[in Abschr.]", since KBo 22.3 + is OH/NS. Also the exx. cited in that same paragraph following "Mit -a" are OH/NS, not "Orig." On p. 133: If the system proposed by HW² for OH does not hold even in OS (admitted in the statement), why even propose it? In the paradigms for "jheth." (p. 133) the interesting form *a-pi-il* KBo 2.13 obv. 12 (NH, cult inv.) is overlooked. Under *a-pu-un* we read "fehlt ah". One should write rather "fehlt bisher in aheth. Orig.", since *a-pu-un* is found in KBo 20.34 obv. 8 (OH/MS) and KBo 19.5:3 (OH/NS, law 99). Under *a-pu-un-na* note KUB 8.81 iii 2 (MH/MS).

137a: If *apel* in NH cannot be translated "their", one should not fault Friedrich, Heth. Elem. §112 for claiming so. He lists there only the forms *apenzan apedaš*. In his §110 one should perhaps remove the ending "(-ēl)". K. may be right in claiming that the singular NH *apel* was always kept distinct from the plural NH *apenzan*. If so, the occurrence of *apenzan* is no independent criterion for an OH or MH date of composition. But the claim that *apenzan* is "bisher nur jh." leads to no safe inference. Does K. intend to imply by this remark that OH employed a different form? We do, after all, possess two MH occurrences: KUB 23.77:60 (MH/MS), KUB 4.1 i 17 (MH/NS).

148d: *appa³*, *a-ap-pa* Why only this heading? On p. 151d (this same article) she offers "Belege für *appa*, *appan*, *appanda*". *appan* and *appanda* should therefore also be in the main heading. It is unacceptable for a dictionary editor to excuse himself from the responsibility to read and evaluate all of the evidence for an entry, even if it amounts to "Hundert von Belegen" (p. 148d). We read and evaluated more passages than that in the preparation of the CHD article on -*ma*.

161c: Kammenhuber's isolated position in matters of text dating by palaeography and orthography shows itself again in her claim: "trotz Ottens Kennzeichnung des Textes [KBo 13.175] als 'alte Schrift' jheth. wie das übrige KBo XIII!" Otten is surely right in his dating of the script of KBo 13.175. Furthermore, what is the pertinence of the dates of the other texts copied in KBo 13 to that of number 175? We all know that old originals were kept in the same archive rooms with younger copies.

162d: On the use of the verb *appai-* with the local particle -*apa* there is no good reason to claim "Altertümelnd (nicht alt!)". Of the examples cited there KBo 21.93 is OH/MS, and KUB 33.79 is OH/NS. Not cited are KBo 23.69 + 24.11 i 7 (OH/MS) and KBo 19.163 iv 19 and KBo 20.96:7 (both OH/NS).

163c: The entry for *appalai-* should have a date. It appears to occur from MH/MS (IBoT 1.36) on. In KBo

16.50 obv. 14 one should translate the promise as future: "Wir werden ihn nicht täuschen". Götze's old derivation "Falle stellen" was based on an incorrect analysis of the verb *dāi* as "puts" ("stellt"), as Otten (ZA 52 (1957) 220) demonstrated (cf. also Al. Heth. 125 n. 192). Without the combination "to lay an *appali* = to deceive" there remains no advantage to translating *appali* as "trap". Güterbock (apud Al.Heth. 125 n. 192) proposed "takes deception (?) for the king" as a translation of *našta ANA LUGAL... appali dāi*, which he thought to be idiomatic for "to deceive the king", thus virtually synonymous with *appalai-*.

164b: On *appaliyalla-* one might have taken note of the suggested translation "deceiver, traitor (?) in Al.Heth. 125 n. 193.

164d: Read ^{LU}*MEŠappanali-* in KUB 38.12 ii 25 with the determinative.

165a: There is no Hittite word **apparatar* (!) "Zukunft", despite Archi and Kammenhuber. KUB 44.16 iv 17 reads EGIR-*pa* DIB-*an-na-aš* "(the tutelary deity) of refuge" (contrast the clear RA signs in col. I, lines 6 and 7). Dupl. KUB 2.1 iv 20 has only DIB-*an-na-aš* preserved. Compare URU.BAD EGIR-*pa* *ap-pa-an-na-aš* AŠ-RU NU.GÁL KBo 5.8 ii 24 (AM 152).

165b: There is no vessel name ^{DUG}*apparma-* (or *tup-parma-*?). The lines in VBoT 89 obv. 12, 13, 22 and 23 must be interpreted in the light of other occurrences of this word. Thus: pl. nom. [g]a-*a-pār-mi-i-e-eš* KBo 10.34 i 19, *kap-pār-mi-e-eš* KBo 8.91 obv. 19, gen. [ga]-*ap-pār-ma-aš* VBoT 89 i 23, *ga!-ap-pār-* ibid. 23, *ga!-ap-pār-m[a]* ibid. 12, *ga!-pār-ma-aš* ibid. 13. It occurs always in contexts with foods, especially fruits (figs, pomegranates, raisins). In KBo 10.34 i 19ff. the g.'s are ŠA ^{GIS}HAŠ-^{GIS}HUR [ŠA ^{GIS}MA... "of (various fruits)", which could suggest that g. is an edible or potable part of these fruits. In KBo 8.91 obv. 19 g. is associated with NINDA.LÁL TUR-TIM. The VBoT 89 i 11ff. and 20ff. passages mention first raisins, figs, and pomegranates (one half UPNU of each), then in broken contexts a sequence of entries 1 ^{DUG}kappiš *ga(p)parmaš* "one k.-vessel of g.", perhaps each referring to one of the foodstuffs. In Al.Heth. 114 and 120 it was supposed that *ga(p)parma-* was "(fruit) juice".

166d: Remove the cross reference to the non-existent **apparatar* in the article on *appašiwatt-*.

168a: KUB 31.100 obv. 10 should be read: *nat ŠA EGIR.U₄-MI wetummar e-eš[-tu]* "Let it be a structure which will endure forever!" The translation "Das ist das Gebäude der Zukunft" is inappropriate.

168c: In the third line one should now read following Eichner's collation AS-KUT (not "AŠ-KUT(!)"). From a photo Eichner saw the little inscribed ZA instead of Ehelolf's copied UD, and the Akkadian verb is *sakātu* with samekh.

181b: Does one really need such a cumbersome and wordy entry as "(in die Erde gegrabene, verschliessbare) Opfergrube"? Why not just "Opfergrube"? The word has such a wide distribution that "hurr. LW" will hardly do. Better "Kulturwort" or "Wanderwort". Only the proximal source was Hurrian.

181d: The important example *nu a-a-bi ḥe-e-ša-an-zi* KUB 47.59:6 was omitted here.

193ff.: It would have been better to separate the ex-

amples where *apiya* was local from those where it was temporal.

184d: *apiyaku* is attested earliest in the MS tablet KBo 8.35 ii 7, 1 (12), (24), but in the light of the OH/NS example cited by HW² it may derive from OH.

185d: On ^{LU}*apiši-* the statement about distinguishing between "Lehnwort" and "fremdem t.t." is overly optimistic. As I said in my review of HW² Lieferung 1, these distinctions simply cannot be made on the basis of our written evidence. All that one is able to say is whether or not the foreign word shows the Hittite inflection.

185d: It is disturbing to see such careless misreadings as ^{URUDU}*a-pi-iš-ku-ri-iš*. In the light of KUB 42.39:5, KBo 3.15:4, and KUB 12.10 iv 7 this can only be ^{URUDU}*zapiškuriš*.

188a: The Hittite law 162 must be read *ta la-ar-pu-ut-ta*, not *ta-at ar-pu-ut-ta*, according to collation by Güterbock. See Güterbock in WdO Klima volume (forthcoming) and CHD *larpu-* v.

100a: One should cease writing KUŠ^{MEŠ} in the oracles and read correctly SU.MEŠ, which is the Sumerogram equivalent in meaning to the abbreviated Akkadogram TE^{MEŠ}. KUŠ "skin" has nothing whatsoever to do with extispicy.

192d: One should certainly read these writings as Akkadograms, not Hittite! (EN) É *A-BU-US-SÍ* (= [bēl] *bīt abussi*). To regard this as Hittite is absurd. There is no trace of Hittite inflection. See CAD A 92-93. The spellings of *abūsu/abussu* with doubled *s* are found at Nuzi.

193a-b: One should give credit in print at the head of the article to Rost (MIO 1:350 n. 10) for the evidence of alternation in the duplicates of Maštigga. The existing reference (193c) is too inconspicuous.

194a: ^{UZU}*appuzziyanza* is merely the ergative of ^{UZU}*ap-puzzi-* and needs no separate article. Furthermore an important further example KBo 25.107:6 is in OS.

194a: On *ar-*, if one writes "Sumerogr. GUB", why is it necessary to add "geschr. DU"? How else can one write GUB?

218: For a dictionary which is not primarily an etymological one it seems strange to see two full columns of small type devoted to etymological discussion.

219b: In the fourth line from the bottom correct the reading attributed to me (RHA XXV/80:52) to *natta a-a-ra*, not *na-at-ta a-ra*.

221c-d: KUB 21.19+ iii 28f. must be read: ... *kur-ku-ri-iš-ki-ir* "(My friends and associates) were insulting/taunting me" (not *šarriškir*). Cf. *kurkuriman* in line 31, referring back to *kurkuriškir*. The vocabulary KBo 26.20 ii 35 must be read KAxUD.Ē.A.DI (not KI).

222b: I certainly would not render *a-ra-a-aš-mi* as "o mein Freund" without explaining the *š*.

235b: It is preferable to read *a-ra-aḫ-zé-aš* in KUB 6.2 obv. 19, 21 (oracle) rather than the meaningless form *a-ra-aḫ-at-me*.

Chicago, December 20, 1979

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Johann TISCHLER, *Hethitisches etymologisches Glossar*. Mit Beiträgen von Günter Neumann. Lieferung 2 (*harzazu- — iškiššana-*). Innsbruck, Institut für Vergleichende Sprachwissenschaft, 1978 (23 cm., v + 191-402 pp.) = Innsbrucker Beiträge zur Sprachwissenschaft, Band 20. ISBN 3 85124 537 7.

The second instalment followed hot on the heels of the first (1977; reviewed above in vol. 36 [1979], pp. 56-58). The general observations made there are equally valid for the sequel now under review. To treat all of the presented material adequately would require equal space and more; and there is little point in massive further generalization. Because each discrete entry is in many ways an entity unto itself, it deserves to be scanned on those very terms. To this end I have chosen almost at random for individual comment a finite number of specific lexemes where critical discourse seemed to elucidate the author's procedures, correct what I think are shortcomings, and contribute something constructive to the advancement of Hittite etymological studies.

The entry *hašter-* 'star' is flawed in several ways. References are often wrong¹). "Toponymic thematization" into ^{URU}*Haštira-* is possible, but the record shows only acc. sg. ^{URU}*Haštiran* (KUB XIX 65 + XXXI 13, 9), ^{URU}*Hašteran* (KUB XIX 64, 24), gen. sg. ^{URU}*Haštiraš* (KBo XIX 10 + KUB VIII 75 I 22), ^{URU}MUL-(r)aš (e.g. KUB VIII 75 III 11 and 49-55), dat.-loc. sg. ^{URU}MUL-ra (cf. H. Otten and V. Souček, *StBoT* 1 [1965], 32); surely these oblique cases would also be regular from the nominative *hašterza* < **hašter-s*. Tischler recalls that Emil Forrer had contributed a *haštira-* 'star' already to the 1939 edition of Feist's Gothic etymological dictionary; for all his vagaries Forrer deserves admiration as something of a free spirit who was able to draw the correct conclusion from KUB I 1 III 49 ^{URU}MUL-ra-an = KUB XIX 65 + XXXI 13, 9 ^{URU}*Ha-aš-ti-ra-an*, despite Götze's repeated misediting (*p*)*i-ra-an* in *Hattušiliš* [1925], -u¹-*ra-an* = ^{URU}*Ha-aš-šu-ra-an* in *Neue Bruchstücke* [1930]). All the while a ghostly *aštira-* (extracted by E. Weidner from KUB VIII 75 I 22 [not II 22, as in Tischler] -a¹*š-ti-ra-aš-kán*) has been paraded through Sturtevant's glossary (1936), Friedrich's dictionary (1952), and Tischler's own first fascicle (1977), despite F. Sommer's warning (ZA 46 [1940], 46-47) that the initial may be incomplete. In the entry under review Tischler finally sits down to a helping of crow by admitting that *aštira-* "dürfte damit [i.e. in the wake of Forrer and the discovery of *hašter-*] als überholt betrachtet werden können". In fact the publication of KBo XIX in 1970 brought the join KBo XIX 10 + KUB VIII 75 which under I 22 made an honest name out of ^{URU}*Ha-aš-ti-ra-aš-kán* and thereby wiped out *aštira-* altogether; Tischler's philology is simply badly out of date. It is disquieting to see an author write an entry for *aštira-* while already aware of *hašter-* and refer loftily to "different phonetic realizations of prothesis", rather than bother to check out the inner-Hittite attestational status of the alleged variants (as had in fact already been done by C. Watkins, *Die Sprache* 20 [1974], 12).

¹) "Josephson sentence particles [1972], 6" [twice]; "Ertem 45" can be found *ad paginam* neither in the "Fauna" nor the "Flora" book.

Erica REINER, in collaboration with David PINGREE, *Babylonian Planetary Omens: Part Two. Enūma Anu Enlil, Tablets 50-51*. Malibu, Undena Publications 1981 (28 cm., viii + 100 pp.) = Bibliotheca Mesopotamica II/2. ISBN 0 89003 049 9.

"The present volume is the second in an intended series of studies of the canonical corpus of celestial omens — *Enūma Anu Enlil*. Tablet 63, the "Venus Tablet", was published in the first of this series, in *Bibliotheca Mesopotamica* 2/1. Nineteen texts, probably representing Tablets 50 and 51, form the basis of this study of the constellations or "fixed stars" and the omens associated with them. The constellations correspond to those listed in the Astrolabe B (KAV 218) and the astronomical compendium MUL.APIN. This study contains an Astronomical treatment (comprising discussions of constellations and astronomical phenomena and a star catalog) as well as a Philological one (the reconstruction of *Enūma Anu Enlil*). This volume includes transliteration and translation of and commentary on the texts, and a glossary and relevant indices".

Parpola's "Letters from Assyrian Scholars to the Kings Esarhaddon and Assurbanipal" (1970) give a vivid picture of the tremendous influence accorded to the stars by Ancient Near Eastern potentates. Astrological lore evidently was quite confusing already to the ancients (and left conveniently much latitude for interpretation); for us, the problem is compounded by the fragmentary state of the tradition. The previous edition of the monumental corpus of celestial omens, Virolleaud's "Astrologie Chaldéenne" (1908/11) is hopelessly outdated (the number of texts and unattached fragments has grown from approximately 400 to about 2000 since then). We must be very grateful to the authors — whether we are Assyriologists, historians of science, or simply interested in the history of civilization — for organizing this material and gradually making it accessible.

The authors obviously are aware that the material is of interest to a wider audience (p. 24). Unfortunately, they do little to help the non-Assyriologist. For example, it is hard to understand why they chose to include in the Appendix a new transliteration — but no translation — of Section A of Astrolabe B. If space was at a premium, an annotated translation would have been quite acceptable to an Assyriologist; Astrolabe B is well preserved in a single, beautifully written, published manuscript. But the text is so intimately related to the rest of the material covered in this fascicle, that in my opinion an integral new treatment of the text would have been justified.

The central part of the fascicle is occupied by an edition (in transliteration and translation) of 19 texts, partly constituting, partly associated with, the assumed tablets 50 and 51 of *Enūma Anu Enlil*. There is a somewhat complicated system of indices, glossaries and concordances, but it actually is convenient and simpler to use than it would first seem.

Not being a philologist, I shall concentrate my further comments on the astronomical parts.

The Astronomical Introduction (by David Pingree) is helpful to Assyriologist and non-Assyriologist alike; it would have been even more so if the author had given some attention to carefully arguing the reasons for and against his assertions. For example, (Section 2.1.2.2, page 3),

while it is quite true that the association of the constellations of the astrolabes with the months and the three "paths" is not a mathematically exact one, it might nevertheless have been good to point out that in three quarters of the cases (19 out of 26) the constellation indeed does rise heliacally in the month indicated (plus/minus 15 days), and that, with one exception each, the Ea stars are south of the equator, the Enlil stars north. In the case of the most glaring exception (EN.TE.NA.BAR.HUM) one might wonder whether this constellation had a dual identity — note that Text III 35 lists it among northern ones and equates it to ALLUL.

On p. 6, the authors state that they disagree with the traditional interpretation of "rising" (IGI) and "setting" (SÚ) as heliacal rising/setting, but they neither explain the reasons behind their disagreement, nor do they give a clear definition of the "visual analogues" they decided to rely upon instead when checking the identification of the constellations. Later on (Section 2.2), they seem to have given up their objections, and freely use the notion of heliacal rising themselves.

In Section 2.2.1.2.1 (p. 17f.) the intriguing hypothesis is advanced that the "paths" of Ea, Anu and Enlil are segments along the eastern horizon over which the heliacal rising of a particular constellation occurs, commenting that "this seems to us a conception much more in line with everything else known about Mesopotamian astronomy than is the usual interpretation" (which is that the paths are bands parallel to the equator, the path of Anu ranging from a declination of -16.6° to +16.6°, the paths of Ea and Enlil being to the south and to the north of it, the boundaries being determined by a statement in MUL.APIN, according to which the sun spends 3 consecutive months in each path). In my opinion, too little is known about the older Mesopotamian astronomy that one could justify such a sweeping assertion. The circular astrolabes (CT 33, pl. 11-12), which look like schematic maps of the sky, seem to be more in line with the traditional interpretation of the paths than with the proposed new one.

This edition does not give any cuneiform texts so far; it is planned to offer them later in the form of photographs on microfiche. I personally found microfiches with type-script material very tiring to use, and the resolution and sharpness of half-tone photographs was disappointing. Perhaps some imaginative enhancing will be needed (e.g. stereo pairs with exaggerated depth) to make this proposed new mode of publication of cuneiform tablets acceptable. In any case, it is to be hoped that this new edition of *Enūma Anu Enlil*, including that of the cuneiform texts themselves (in whatever form), will proceed speedily.

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PETER J. HUBER

REVIEWER'S NAME

Johannes FRIEDRICH Annelies KAMMENHUBER, *Hethitisches Wörterbuch*: Zweite, völlig neubearbeitete Auflage. Lieferungen 4 und 5. Heidelberg, Carl Winter Universitätsverlag, 1979, 1980 (25 cm., S. 241-400).

In the course of seven years (1975-1982) six fascicles, the last designated Lieferung 6/7, of the Friedrich/Kammenhuber *Hethitisches Wörterbuch* (hereafter abbreviated *HW*) have appeared. Although I have just received my copy of Lieferung 6/7 (1982) and have checked the length of certain articles in it, its contents will not be assessed in this review. The total number of double-columned pages in these fascicles exclusive of the front matter is 560. Allowing a year for the preparation of the first fascicle, I calculate that these 560 pages of dictionary were produced over a period of eight years, an average of seventy pages per year. This is a remarkable achievement, for which Professor Kammenhuber should be congratulated.

All Professor Kammenhuber's colleagues can appreciate the magnitude of her achievement. It is no small task which she has undertaken, and she has addressed and pursued it with energy, intelligence and fortitude. A perusal of the contents of any page in her dictionary will show how carefully she has investigated the usage of the words treated as well as words of related meaning. She has conscientiously utilized the publications of her colleagues. Undeterred by the fact that she does not have access to unpublished texts, she has made effective use of the published ones. Although hers is a dictionary of the Hittite language, she has made good use of insights gained from research into the other languages of Hittite Asia Minor (Hattic, Hurrian, Luwian, Palaic).

A dictionary serves many purposes. Above all it should be a complete and accurate collection of the words found in the text corpus. Kammenhuber has listed every word (Hittite, Luwian, Hurrian, Sumerograms and Akkadograms), including some which are broken at the end or at the beginning (cf. below on *-jarniyauwaš*). Such complete coverage is welcome to all, but especially to students.

Kammenhuber has sought not only to test the accuracy of accepted translations but to draw the semantic boundaries of synonyms. She regularly indicates known synonyms or antonyms (cf. *alpu-*, *amiyant-*, *andurza*, *apa-*, *appašiwatt-*, *ara-*, *arawa-*). Where these are not noted (e.g., *annalli-*, *annišan*, *annaz*, *apiya*, *arnu-*), it is only through oversight (but cf. below on *arnu-*). I recommended this approach to Hittite lexicography in the introduction to my *English-Hittite Glossary*, RHA 25 (1967) and we have sought to implement it in writing the *CHD*. It is gratifying to see it also in the *HW*. Obviously it is no easy task to distinguish words of related meaning, even in a living language like English or German. How much more so in a language which has not been spoken for three thousand years! Therefore, although we may not always agree with her conclusions, we applaud her procedure.

I should like to make it clear that, although my *English-Hittite Glossary* has served its purpose over the past fifteen years, helping researchers find Hittite words in particular ranges of meaning, it was never intended as an independent lexical tool, but as an index to J. Friedrich's *Hethitisches Wörterbuch*. It may have contained a few personal contri-

butions, but the English translations were largely dependent upon Friedrich's German ones. J. Tischler's recently published *Hethitisch-Deutsches Wörterverzeichnis* is similarly dependent upon the published fascicles of the *HW*, the *CHD* and Friedrich. Such indices render an important service. But they are, after all, compilations based on the existing dictionaries. If Professor Kammenhuber intends to continue to credit and criticize my 1967 glossary for its translations, I would suggest the following guidelines. Whenever its entries merely reflect Friedrich, it should be neither credited nor criticized independently. When it differs from Friedrich, it deserves whatever praise or criticism is due it! I am well aware of its flaws. I have no desire to defend its every entry. I was a student when I prepared it twenty years ago! I say this, because *HW* often criticizes its renderings, even when they merely reflect the imperfect state of knowledge which existed in Friedrich's *HW* (1952).

In earlier reviews I have indicated my differences with Professor Kammenhuber on the dating of Hittite texts. It must be stressed, however, that my views on this question, which have never been systematically set forth in print, are not adequately represented by isolated observations made years ago in my review of Houwink ten Cate's *Records of the Early Hittite Empire* (JNES 31 [1972]). It is these remarks which Kammenhuber chooses to criticize. In those days we were all groping for general distinctions between Old Hittite, Middle Hittite and New Hittite. The study of Hittite script was in its early stages. In her contribution to the Festschrift for I. M. Diakonov, Professor Kammenhuber seems to claim that, if one cannot determine the age of the script of small fragments, such as those Old Hittite festival fragments whose script was variously characterized by Otten before they were later joined, all diachronic distinctions of script other than the recognition of an Old Script and a later one are invalid. This is patently not the case. That there are features common to the ductus of all Middle Hittite tablets cannot be doubted, even after one has made allowances for the peculiarities of each scribe's hand. And even if it should turn out that Neu's (*StBoT* 25) Old Script I (what used to be called Typical Old Script) is not older than Old Script II, this would not negate the solid achievement of showing that both were in use during the Old Kingdom.

In the *CHD* the inflected forms of words are always listed in small type in the same location near the head of the article. Each form is documented by a text reference. In the *HW*, however, one cannot predict where or in what manner the inflected forms of the word will be cited. Sometimes there is no separate section devoted to this purpose, and the entire article is organized according to the various forms of the word (cf. *arahzena-*, *aramnant-*, *aranu-*, *ariyašeššar*). Sometimes there is a paradigm near the front of the article, which lists the writings and notes the number of occurrences ("12x"), but leaves most forms without text reference (cf. *arai-*², *arawa-*, *arawahh-*, *arawanni-*, *ark-*). Sometimes there is a paradigm of forms with most, if not all, forms referenced (cf. *arai-*¹). Sometimes the forms are only cited in the course of semantic determination without any meaningful order (cf. *arallai-*, *aramni-*). In *arawahh-* one is not even directed to the particular part of the *arawa-* article where they are presented and translated (they are in II 7 c).

Nor is there a regular place in the *HW* articles for the presentation of evidence from bilinguals. This makes it difficult for even experienced Hittitologists to find the information they need to evaluate the proposed translation.

Other reviewers must evaluate the etymological discussions in *HW*. Whatever their merit, etymologies should be based on translations obtained from the study of the Hittite usage and context. Sommer and Friedrich took great pains to establish this principle, but there are signs that some today are disregarding it. Fortunately, Kammenhuber is not one of these. She eschews all attempts to determine the meaning of Hittite words by etymology. If her etymological discussions help to discourage that kind of enterprise, I approve of them. To the degree that they only give a forum for controversies which cannot be resolved on the basis of Hittite cuneiform orthography, I find them of no interest.

Sometimes it is necessary in the course of discussing a word's meaning to provide background of a cultural or historical nature. But such excursions should be as brief as possible. The geographical discussion under *aruma-* "sea" and the historical-genealogical discussion under *atta-* "father" are much too long. This was a principal reason why the article *anna-* "mother" required only four columns (two pages) of text, while *atta-* "father" required at least thirty-eight columns (nineteen pages). This 9:1 ratio of coverage hardly reflects the relative importance or even frequency of occurrence of the words.

Since 1975, when Lieferung 1 appeared, several volumes of new texts have appeared. But the percentage increase in published texts which they represent is negligible. One can only understand the increased size of the average article in *HW* as an unfortunate trend in the author's lexicographic style. In the first two fascicles (1975 and 1977) only two articles (the demonstrative *apa-* 36 columns = 18 pages, *antušša-* "human being" 22 columns = 11 pages) exceeded ten pages. Even with words of extremely high frequency such as *-a/-ya* "and" (3 cols.), *-a-* (3rd pers. enclitic pron.) (3 cols.), *anna-* "mother" (4 cols.), and the adverb/preverb/postpos. *anda(n)* (19 cols.) she exercised admirable restraint. There was hope that the dictionary would be compact and finishable. But with the appearance of Lieferung 3 in 1978 the picture began to change. There now appeared *aššanu-* (24 cols.), *aška-* (28), *ar-* "to stand" (30), and *atta-* "father" (38+). Even longer were the minimonographs on *arha* (58 cols.), *aššu-* (70 cols.) and *-ašta* (130 cols. = 65 pp.). What began as a dictionary now appears to be becoming a thesaurus. We urge Professor Kammenhuber to return to the exemplary conciseness of her earliest fascicles. We do not need the many excurses devoted to subjects better handled in separate monographs.

On the whole these two fascicles (Lieferungen 4 and 5) have been very helpful to me, as were the earlier ones. They contain some excellent ideas. The detailed criticisms which follow should not be interpreted as questioning the value of *HW*. They are offered as a supplement and corrective for the aid particularly of younger scholars with no files on which to base an evaluation.

arahzena-. The interesting, even if faulty, form *arahzenun* from Puduhepa's letter to Egypt (KUB 21.38 obv. 49) should have been cited and commented upon.

arawa-: Kammenhuber (hereafter Ka.) has declared *arawa-* and *arawanni-* to be synonyms, the latter being the

rarer. And whereas Friedrich (*HW*¹) listed both as adjectives, and Güterbock (*CRRAI* 18 [1970, printed 1972] 96) considered *arawa-* to be an adj. and *arawanni-* a noun, she has claimed that both occur as adj. and noun (substantivized adj.). In fact *arawa-* is always an adj. (not yet attested as a substantivized adj.), while syllabically written *arawanni-* is usually attributive adj., but occasionally substantivized (probably so in law 194). And whereas Friedrich listed *ELLU(M)* as the logogram only under *arawanni-* (cf. also von Soden, *AHW ellu(m)* 1 3), and Güterbock assigned only the occurrence of *ELLUM* in law 51 explicitly to *arawa-*, implying that the other occurrences of the Akkadogram should be assigned to *arawanni-*, Ka. has assigned all occurrences of the Akkadogram *ELLUM* (fem. *ELLETTUM*) but the vocabulary KBo 1.45 rev. 1 4 to *arawa-*. She thereby shows that she has not recognized that *arawa-* and *arawanni-* differ in meaning and syntactic environment and are therefore not synonyms. In spite of the many occurrences of *ELLUM* (fem. *ELLETTUM*, pl. *ELLUTIM*), in no instance does it alternate in a duplicate with a syllabic writing. Yet the vocabulary entry *ELLUM* = *a-ra-wa-ni-iš* and the alternation of *ELLUM* and *arawa-* in law 51 shows that the logogram was used for either. The only attested Hittite phonetic complementation with *ELLUM* is the gen. sg. ending *-aš*. These forms could be interpreted as either *arawanniyaš* or *arawaš*. Given this situation there is no formal criterion for determining whether *arawa-* or *arawanni-* underlies a particular occurrence of *ELLUM*. In each instance one must decide on the basis of the meaning required by the context. *arawa-* is construed with the abl. and translatable as "exempt from (obligations): *šahhan* and *luzzi*, actions expressed in infinitive clauses". *arawahh-* "to free/exempt from (obligations)" is the *-ahh-* factitive built on *arawa-* (not *arawanni-*). There is no instance of *arawahh-* meaning to make a slave into a non-slave (manumission). In the only case where the objects of *arawahh-* are *IR.MEŠ* and *GEME.MEŠ* the action is specifically qualified with the instrumentals *šahhanit* and *luzzit*, so that there can be no question of a translation "manumit (slaves)" (KBo 10.2), but of freeing/exempting them from the obligations to *šahhan* and *luzzi*. KBo 13.72 obv. 8-9 and KUB 40.2 rev. 12 describe a situation in which persons have been donated to a temple which is exempt from *šahhan*, *luzzi* and from liability to the requisitioning (*appatariya-*) of work animals, and thus it can be said that the king "exempted" (*arawahh-*) the "servants" (*IR*) of the god or temple. No manumission is in view. This is the situation described also in KUB 13.8, the edict of Queen Nikalmati regarding the personnel of the Stone House (discussed by Güterbock, *CRRAI* 18 [1970, printed 1972] 93f.). *arawa-* usually describes persons who are exempted from some obligation which could also devolve on non-slaves, i.e., required services to the king (*šahhan* and *luzzi*). In the laws it once describes a "house" (i.e., a household = persons!) which is exempt from these services (law 51). In laws 50 and 51 the Akkadogram *ELLU(M)* covers *arawa-* in just this meaning. An examination of the context of [*nepi*]š *arawan ... tekan arawan* in KUB 23.68+ rev. 9f. reveals that the sky and the earth are characterized as "free" (*arawa-*) in contrast to others in the context who are under the obligation of oaths (*linkiyaš*, gen., "one of the oath"). The sky and the earth are "free" not in the sense of "non-slave", but unencum-

bered by the obligations of oath or treaty. *arawanni-*, on the other hand, means "non-slave" (perhaps also "noble", as suggested by Güterbock, *op. cit.*, although Ka. claims this meaning is "nicht nachweisbar" HW 251, but approaching "adelig" references on 257 left col.). *arawanni-* never means "exempt, freed from (obligations)" and is never construed with the ablative or infinitive clauses. The two words have quite distinct meanings and syntactic environments. Now if the preceding observations based upon syllabic writings are valid, the Akkadogram *ELLUM* (fem. *ELLUTUM*) in the laws stands for *arawanni-* "non-slave" in 25 cases, and for *arawa-* in two (laws 50 and 51). Examples of the logogram outside the laws seems always to stand for *arawanni-*. Measured by this standard *arawanni-* is far more common than *arawa-*, and both occur from OH through NH.

arma- "moon; month" Cf. below in comment on *arme-*. On 236a and 321a "Monatsanfang" and "Jahresanfang" have been reversed. The translation of *miyari* as "(Wenn ein Kind ...) wächst" is incorrect. The verb in these omens means "If a child ... is born (in such and such a month)". In the discussion of Akkad. prepositions with ITU.X.KAM, the phrase *IŠTU* ITU.X.KAM found in the lunar eclipse omens KUB 34.7 iii 10, KUB 8.1 ii 11, iii 4 is not treated. It would have been better, if, when Ka. wrote "ITU.X.KAM: fast nie heth. kompl.", she had indicated the passages which contain the rare examples of Hittite complementation. As all Hittitologists are aware, ITU.X.KAM can sometimes be translated "for x months" (not ordinal). In one such case we also observe one of the exceptional cases of Hittite complementation: "Keššiš ITU.3.KAM-aš HUR.SAG. MEŠ-aš anda wehatta "Kešši wandered about for three months in the mountains" KUB 33.121 ii 15. Cf. also naš UD.5.KAM-az našš-X[...] KBo 20.31 obv. 10.

armahhi- "to impregnate" (transitive). Nothing is said about the construction of *-za armahhi-* with the acc. of the conceived offspring (exx. under Imp. Sg. 3). Instead, the impression is given that *-za* only accompanies the intransitive construction "to become pregnant". Although Laroche's translation of *armahhiwazza* "les médicaments de grossesse/de la conception" (for the latter translation cf. Laroche, *CTH* pp. 177f.) is probably correct, one sees no reason to call this (with Ka.) the abl. of the verbal substantive. Formally there is a resemblance, but the meaning is not that of a verbal substantive. Furthermore, Laroche did not restore *waššiya[zzī]* (Ka.), but *waššiya[z]*, the abl. of *wašši-* ("ingrédients"), which is preferable in the context.

armai- "to conceive, become pregnant". Ka. has overlooked a significant variant in law 83. Instead of *armandan* the dupl. KBo 22.65 i 1 has [a]r-ma-u-wa-an[da-an].

armannai (St.?) (Gegenstand) and *armannant-* (Subst. u.B.). What KUB 42.67:8 has is not *armannai*=m[a (so Ka.)], but *armannaim*[a, a form of the Luw. part. *armannaimi-* from the vb. **armannai-*, which Hitt. part. would be *armannant-*! This vb. **armannai-* is to *armanni-* "lunula", as *haršanallai-* "to adorn with wreaths" is to *haršanalli-* "wreath". *armannantit* is instrumental, modifying one or more of the items of equipment with which the troops are to be outfitted according to KUB 40.56 iv 7 (*BEL MADGALT*). **armannai-* means "to ornament with crescents".

armanni- "lunula, crescent". The *išnaš armanni-* is also attested before the 13th century, as is clear from KBo

14.91 2 (cited 326a), which on the tablet (collation 1981) shows Middle Script. An important example of the silver lunulae in idol descriptions was omitted here: KUB 38.1 (von Brandenstein Bildbeschr., no. 2) i 14 describes the idol of the divine spring ^dIšhašhūriya as a "statue of a woman, of wood overlaid with silver ... on her breast three silver moon crescents" (3 UD.SAR KÜ.BABBAR). Cf. Bildbeschr. pp. 10ff., 47. The example from EZEN KILLAM, KBo 10.23 iv 5f. (OH/NS), is not "13. Jh.", unless perhaps the date of the copy is meant. 1 NINDA *ar-am-ma-an-ni-iš* KUB 41.36+ i 9 could be a scribal corruption of NINDA *armanni-*. Ka. expresses doubt if the suffix *-(a)nmi-* represents "ein echtes heth. Diminutivsuffix", since it can be suffixed to Hurrian DN's. Yet she sees that *armanni-* is a "little" *arma-*, for she poses the question herself, if in view of the archeological evidence *armanni-* should not in addition to "lunula" have the translation "kleiner Monat".

armawant- „pregnant". Cf. above under *armai-*.

arme- oder *arma-* c. jheth. (Teil am Ochsen gespannt) collapses as a separate word with the recognition that it is the nom. pl. of *arma-* "moon" in the same context as *armanni-* "lunula". Cf. the parallel passage KBo 10.23 iv 1-6 with *armanni-*, and cf. already Hoffner, *BiOr* 35:247.

armihi- (Hurr.) KUB 32.50:9 (*AOATS* 3:123f., Laroche, *Glossaire de la langue Hourrite* 55) has been accidentally omitted.

armizzi-. Since, as Ka. admits, *a.* is neut., *ar-mi-zi-iš-ti-iš* KBo 13.86 rev. 2, common gender and without confirming context is possibly another word. ^{GIS}*ar-mi-iz-z[i-i]a-aš=ma* (KBo 22.6 i 18) might be pl. dat. as recipient of the two sacrificed rams. But the bread and wine in KBo 20.123 iv 8-10 is offered "to the Zu[- ...] River of the bridge" (so correctly Del Monte, *RGTC* 6:361). *armizziyaš* must be a genitive in view of the syntax *armizziyaš ANA* ip Zu[-...]. On *lalaš=wa* (^{GIS}*armizzi* "The tongue is a bridge" cf. *CHD* sub *lala-* and my remarks below on *armizziya-*. The *-wa* indicates this was a saying. It is not what the oracle bird is to say (so Ka.).

armizziya- 1. to build a bridge over something, 2. (metaphorical) to divulge a secret. Meaning 2 is related to the saying "The tongue is a bridge" mentioned above. KUB 36.83 i 5-6 has been misread here. *namma-ši šu-up-pi-ia-aš* (coll.)-ni *handa šer* [a]r-mizziyanzi "They tie up a pig and [...] it down in a pit [patteššar is not 'Korb']. And they build a bridge over (šer) it (the pit) out of consideration for (handa) sanctity. (And the sacrificer steps [on the bridge] over the pig.)" Emending to *šu-up-pi-ia-an!-ni* (*šuppiyatar* denotes a feature of the moon) is unnecessary. The word is *šuppeššar/šuppiyaššar* "purity/sanctity". *šuppešni handa* also occurs in KUB 26.12 iv 34-35: (You who are eunuchs and constantly come into contact with the sacred person of the king,) *nu-šmaš šu-up-pé-eš-ni* [han]da *tišhanteš ešten* "Be circumspect (?) out of consideration for (the king's) sanctity" (cf. von Schuler, *Dienstanw.* 28f.).

armuwalai-. The method of indicating variant spellings with two sets of parentheses here fails to indicate what spelling is found where. The verb may be Luwian.

armuwalašha-. If Muwat. had connected this with the moon (so Ka.), he would have written ^dXXX-aš *iwar* (cf. ^dUTU-aš *iwar* in the parallel passage). But since he didn't, there is no proof of folk etymology. Since KUB 8.30 (with its [a]r-muwalašhaiš in obv. 2) is a duplicate of KBo 13.20 (with *armuwalašhaš* in 6), both forms are probably

nom. sg. The lemma should show both stems, *-a-* and *-ai-*. *arnaminti/arnamitti*. Now also in the birth ritual Bo 4951 rev. 12-13: *kešdiya šipanti nuza ar-na-am-mi-it-ti* [šehil] *iš-kinn=a pai*, ed. Beckman, Diss. 154 (forthcoming *StBoT* 29). The action always immediately follows *šipant-* "offer, libate".

arni-. Frequently juxtaposed to *par(i)li-*, *arni-* is a term used with bird offerings, when the birds are the object of *wahnu-* or *warnu-*.

-jarniyauwaš. What criteria determine the choice of acephalic words for inclusion? Not all of them are entered in *HW*. This particular example seems to be a gen. of a verbal substantive from a verb *-jarniya-*, quite possibly *duwarniya-* "break, smash".

arnu-. It's a pity that this verb was not compared to partially synonymous *uwate-* and *pehute-*. My examination of their contexts indicates that *uwate-* and *pehute-* are used when subject and object are both persons, *uda-* and *peda-* when either subject or object is inanimate, and *unna-*, *nanna-* and *penna-* (transitive construction only) when the objects are animals. *arnu-* can take all kinds of objects. But its appropriate translation is not "bring" but "relocate, transfer, move". Ka.'s exclusion of all translations but "bring (away or forth), move (trans.*)" seems unwarranted. The OH participle *arnuant-* describing pregnant cows and replaced in later copies by *armawant-* and *armant-* (error for *arma(u)ant-*?) "pregnant" cannot so assuredly be translated "brought (to a male animal for breeding)". There is simply too much essential information in parentheses. The special contexts of the laws, where several have proposed "bring for burial" seem to be accepted by Ka. on p. 329. But law 19a *nu ē-er=šet=pat arnuzzi* is inadequately explained. Where in the Hittite are the "ihn" and "in" of "Er bringt ihn nur in sein Haus"? In the four cases dealing with kidnapping (laws 19a, 19b, 20, 21) there is a clear gradation from the most serious case to the least. The fines reflect this. 19a is the gravest case, and its penalty should be the most severe. The offender's entire "house" (family and estate) is forfeit. Cf. on this point Güterbock, *JCS* 15:67. In KBo 3.40+ rev. 13-15, the Old Hittite song of the soldiers, Ka. is right (contra earlier studies) that *katta* + gen. is "with". Her translation (*HW* 80a) of *tiya=mu tiya* as "tritt zu mir, tritt!" is also superior to the "put on me, put on" proposed by Watkins (imp. of *dai-* would be *dai*, not *tiya*). But the "clothes of Nesa" are neither the subject of *tiya* (sg.!) nor the implied object of *arnut* ("Bringe (sie) mir mit meiner Mutter"). Like all refrains *tiya=mu tiya* need have no direct relationship to the verses which it brackets. In many songs the refrain line is sung by another singer or singers in response to the soloist who sings the verse. Or in songs involving dance (there are dances on serious, solemn occasions) one singer may divide the verses sung to the audience with a refrain line addressed to his partner ("Step to me, step to (me)"). Indeed, in this case we are told that there were two men who sang. The point is that it is far from certain that the subject of *tiya* is the "clothes". The direct object of *arnut* in the verse is *-mu*, and since for ordinary "bring, conduct, lead" with persons as subject and object the verbs *uwate-* or *pehute-* are required, I would like to see here the same meaning as is found in the laws "bring for burial". "Bring me to be buried with my mother ... bring me to be buried with my uwa-". Compare the expression

"day of your mother" (*HW* 72a) which refers to the death day. Songs anticipating death are common among soldiers. Watkins proposed that the "clothes of Nesa" were garments for burial.

Since *katta arnu-* is a legal term "to bring a case to conclusion, settle a dispute" (already in Friedrich, *HW*¹), this is certainly what lies behind the *erim-ḫuš* equation *ĒN.TAR.RIA = UZ-ZU-ZU = katta-ššan arnumar*, whatever the Akkadian may be. For *ĒN.TAR* is a Sumerogram at Boğazköy for *punuš-* "to inquire, investigate". There is no *ar-nu-wa-an-da* IR.MEŠ "die gebrachten Sklaven" in KUB 27.13 iv 14. The text reads LÚ.MEŠ É.GAL ^m*Ar-nu-wa-an-da* IR.MEŠ ^m*U-ta-SUM-ya* "The palace men of Arnuwanda and the servants of Tarhuntapiya".

arpa-. The marker probably indicates Luwian at this period (Ka. noncommittal). All examples NS (Ka. says from Hatt. III). "Misserfolg" (*HW*¹ and previous renderings) and Laroche (*DLL*) on related *arpaša-* "échouer" are to be preferred to the new suggestion "Niederlage". The evidence is three passages. In KBo 22.260 obv. 12ff. it is not clear what "handelt er auf Grund der Niederlage nicht?" should mean. *nu :arpaš=ma* (*nu ... =ma*) shows disjunctive (i.e., alternative) question (Sommer, *AU* 77f.; Friedrich, *Heth.El.* paragr. 285). The preceding context is helpful. "Since the festivals were not determined for His Majesty to perform, but were determined for a lord, shall the *ikzaš* not act? (Answer: negative). Or will some *arpaš* occur, and will it (*-aš*, i.e., the *ikzaš*) not act by/through the *arpa-*? (Answer: Yes). The *a.* which has been determined, [is it] of the head (person) [of His Majesty? ...] The evil which he will escape, [...]". KBo 23.117 is incorrectly restored and translated through failure to compare lines 2-7, 10-12, which show the same structure: *mān=ma=za* ^dUTU-ŠI LUGAL "X"-an *apez UL KAR-zi MUŠ* ^dU.TI MUŠ SAG.DU MUŠ "X" SIG₅ *šer* IR-za[...], where "X" can be MU.ḪA (2-3), UŠ (*henkan*) (4-5), GIG (6-7), or *arpa-* (8-9). These are all things which threaten the king, but *arpa-* need not be specifically "defeat". "Catastrophe" or "disaster" would be just as serious, but less (unduly) specific. In the etymological section: ^{NINDA}*arpamar* was not said by me (*AlHeth* 152f.) to belong to a verb **arpa-* and adj. *arpuwant-*. I did say *arpamar* was probably a noun of the type *ḫilammar*. We have no forms yet but nom.-acc. This claim was not "trotz seiner Bedeutung", for the word is merely a "bread" type.

arpaša-. It is highly unlikely that this verb in Hatt. I 35 can mean "es wurde mir ... zur Niederlage", since no battle occurs. Even in a figurative sense (which approaches the earlier translation "Misserfolg") it cannot indicate an outcome, for the trial only begins in the following lines, and it turns out well.

*arši-*¹ "Pflanzung". Even if the reading *ar-ši-i[š]* is accepted for the Ras Shamra passage, which would necessitate positing *arši-*², it should have been made clear that *arši-*³ as the name of a lot in oracle praxis is probably the same as *arši-*¹ or *arši-*². The name of a lot might as well be "Pflanzung" as anything else. It seems to me that *HW*'s quibbling about the precise meaning of *arši-*¹ leads to nothing very useful. In the only secure contexts (laws 103 and 105): (1) the *a.* is stolen (*taya-*), which must mean the plants themselves, not the ground, and (2) the *a.* is so-many *gipeššar*, which applies not to the plants themselves, but to the bed, plot or cultivated area where the

...the both aspects are obviously included in the ...
...that they are both OH. Indeed KBo 8.131,
...calls "jeth" shows Nen's OS Type 1 (the so-
... "Typical Old Script"). Since *aršintatli-* is not OH,
... cannot be restored as *aršintatli-*. Therefore
... is probably "branchin" after all for *arši-*.

ard(a)- Ka is quite right to prefer this stem to the
... proposed in *HW¹* Erg. 1. Although KBo 26 was
published in 1978 and *HW* Lieferung 5 in 1980, KBo
26.19.10, which is the only potential (it is broken!) bilingual
evidence for *ard(a)-*, is not cited. Although all of the Sum.
and most of the Akk. columns are broken away, on the
basis of the meanings of the Hittite words and the preserved
ends of some of the Akk. entries it is possible to reconstruct
the Sumerian column in part. I have consulted Professor
M. Civil on this fragment, and it is his opinion that the Sum.
column probably contained entries beginning with K1. He
thinks it possible that this formed part of the Boğazköy
recension of the series Diri = *watru*. According to the
most likely restoration of the Sumerian sequence, KBo
26.19.10 restored would read: (Sum.) [K1.IN.DAR?] =
(Akk.) [ni-gi₅-šú] "cleft, crack, crevice" = (Hitt.) *ar-du-mar*.
This would indicate a meaning "to split" for the verb
ard(a)-. Cf. also the vocabulary entries [IZ.ZI.DAR] =
nengiṣat igāri = *kuttaš paršeššar*, [K1.IN.DAR] = *nengiṣat*
qaqqari = K1-aš *paršeššar* "crack in a wall, crack in the
earth" KBo 13.1 iv 10-11 (erim-ḫuš Boğ.). This shows
that the verbs *ard(a)-* and *parš-/paršai-/paršiya-* shared
the meaning "to split, crack open (the earth)". The meaning
"to split (rocks)" is also attested for *paršanu-* in Ullik.
2nd Tabl. B iii 9-10 and KUB 33.120 ii 36 (cf. Hoffner in
Essays on the Ancient Near East in Memory of Jacob Joel
Finkelstein, Memoirs of the Connecticut Academy of Arts &
Sciences, Vol. XIX, 1977, p. 110). If the restoration pro-
posed above for KBo 26.19.10 is correct, there is nothing
in the equation of Akkadian [nigišš]u with *ardumar* to sug-
gest that the verb *ard(a)-* means "to saw". Probably the
verb *ard(a)-* denoted the splitting apart of rock by drilling,
chiseling and wedging techniques well attested archaeo-
logically in the ancient world including at Boğazköy (cf.
R. Naumann, *Architektur Kleinasien* 38ff.). The secondary
meaning which Ka. posits for]*a-im-pa-an arduṃeni* KUB
36.74 iii 2 would apply to a concrete "to split off" as well
as to her "absagen".

ard/ta- (a bird). How can one possibly know that these
birds were large and not very shy?! The context neither
favors nor disallows a water bird. It allows no precise
conclusion. No spelling *a-ar-ta-aš-ma-kán* is derivable from
KUB 39.35 i 7, as manuscript B makes it clear that *A-NA 5*
ar-ta-aš-ma-kán is intended. The word is always spelled
"short" (i.e., *ar-d/ta-*) to date.

^{GIS}*ar-t[a-* KBo 16.103:6 is to be read: [šeraššan ^{LU} iš š]A
^{GIS}*ar-t[a-ri]*. On other passages of this type cf. Kümmel,
StBoT 3:138ff.

artahhi- n. Much of the evidence for this word has been
confusingly presented here. The alleged evidence for the
equation with "^{GIS}PISĀN" (Ka. means ŠIDXA, which Fried-
rich (*HW¹* 289f.) listed as ^{GIS}PISĀN) is based on two pas-
sages, neither of which can stand scrutiny. The first is a
lexical entry KUB 3.94 ii 9 (with dupl. KBo 26.50:5):
(Sum.) K1.LUGAL.GUB = (Sum. pronunciation) a-[lal] =
(Akk.) *a-la-al-lu* "pipe, conduit" = (Hitt.) ^{GIS}*kal?ma?*-

a[n(-)] ...]. Laroche claimed in *RHA* XXIV/79 (1966) 165
that his collation of the original revealed the Hittite entry
to be: ^{GIS}*a[r]-da-a[h-]*, and this new reading has been accept-
ed by everyone. Recently, H. G. Güterbock and I independ-
ently collated this line. We saw traces irreconcilable with
Laroche's reading and much closer to what Weidner saw
and copied in KUB 3.94. By now part of what Weidner saw
as a complete *kal* is broken, but what remains is
clearly not part of *ar*. The following sign may be rather
la than his *ma*, but it is certainly not Laroche's *da*. The
next sign is as drawn in KUB, and not *a[h-]*. Ka.'s other
evidence is KUB 30.34 iv 14ff., which she forces to yield
an equation by an arbitrary emendation to *ar-ta-ah-ḫi-ia*
<-az>, turning a locative or allative into an ablative.
Furthermore, the complementation of the logogram
^{GIS}PISĀN (= ^{GIS}ŠIDXA) in Hittite proves that it was not
representing an i-stem noun (*artahhi-*), but an a-stem,
possibly a noun ending in *-la-*: ^{GIS}PISĀN-aš (nom. sg.)
KUB 17.10 iii 25, same form in gen. sg. KUB 30.33 i 1-2;
^{GIS}PISĀN-an (acc. sg.) KBo 6.10 ii 8; ^{GIS}PISĀN-li (loc. sg.)
KUB 20.24 iii 3, 14, 21, etc., KUB 7.41 i 30, cf. 28. No
complementations show i-stem forms, much less the con-
sonant *h*. Clearly ^{GIS}PISĀN cannot be the logogram for
(^{GIS})*artahhi-* and cannot serve to determine the latter's
meaning.

(^{GIS})*artati-* Ka. is quite right to challenge the translation
"truffle" because of the determinative ^{GIS}, but the evidence
that one "plants" these is not so unequivocal as she claims.
In KUB 29.1 iv 22-25 I would follow Sommer and Keller-
man and translate *aršai-/aršiya-* as "tend, care for" or
"tended". It appears that the *a*. plant preferred well watered
terrain like meadows (*wellu-*, *U.SAL*: KUB 9.4 iii 29ff.,
KUB 24.7 ii 50, 57ff.).

EZEN *arunitaš*. Much needless confusion is introduced
by this treatment. Puhvel in Fs Whatmough (1958) wrongly
cited both EZEN *a*. and ^d*Arunitti* as alleged derivatives of
aruna- "sea". I uncritically followed him and translated
EZEN *a*. "of the sea" (*RHA* XXV/80, 1967, 39). Puhvel
stopped short of claiming EZEN *a*. as EZEN (^d)*Arunitaš*,
although since he cited them as derivative of *aruna-* the
identification may be implied. In this regard no one has
noticed EZEN ^d*A-ru-ni[-ta-aš]* KUB 22.27 iii 38 which
proves the case for EZEN (^d)*Arunitaš*. A collation performed
by me in Istanbul (June 1981) revealed that (against KUB)
there is no word space on the tablet between AN and
a-ru-ni[-], which excludes a reading EZEN-an *a-ru-ni[-]* ...].
arruša- is not just "jeth". I would date KUB 36.75 as
OH in MS, and KUB 43.60 as OH in NS. These are the
earliest occurrences of this interesting word. It seems to
me that in these passages (OH in post OH copies) the word
denotes a dangerous place in which one could fall (cf.
muhhi in KUB 43.60 from *mauš-*) and be drawn up out
of (*šara ḫuitti* in KUB 36.75 iii 12f.). In the course of time
an idiom developed which literally meant "to go into a
pit(?) / trap(?)" but was used to describe figuratively defec-
tion from the king.

arutamannaimi- is not one word, but two: *aruta-* and
mannaimi-.

aruwašhai-(?) vb. is based on a misreading of *a-ru-wa-*
en-zi in KBo 17.28:6 (OS) (cf. Neu, *StBoT* 25:153, KBo
25.14 i 3, ii 3); cf. the form of the *en*-sign line 4.

arzana- Ka. is confused about this "house", because
much of the structure of Hittite social life is forever low

to us. But if one reads Jacobsen (*JNES* 12:184 n. 68)
and W. G. Lambert (*Babylonian Wisdom Literature* 339,
cf. CAD A² 152) on the *bit aštammi*, one sees that it had
the characteristics of an inn, a brothel and a temple. There
the *susapīnu* (attested in Hittite texts, cf. Hoffner apud
Otten, *StBoT* 15:15, 44 and now KBo 22.49 iii 1-10) and
the *anzaninu* plied their trade as pimps or matchmakers.
KBo 22.49 iii is too broken to tell if the ^{LU}*SUSAPINU*'s ritual
activities were localized in a temple. He appears to wash
someone's feet and lie down on a bed. Also active are a
priest (iii 8) and a *katra-* woman. That women (not just
priestesses but SALMEŠ K1.SIKIL "maidens" and SALMEŠ
KAR.KID "prostitutes") are several times associated with
the activities in the *a*-house cannot be ignored in spite
of Ka.'s emphatic denials.

arzanai- I have proposed to read the participle *ar-[za-n]a-*
an-za in KBo 13.1 iv 4 (Erim-ḫuš Boğ.; cf. *MSL* 17
[forthcoming]). Civil restores the missing Sum. and Akk.
entries on the basis of canonical erim-ḫuš: (Sum.) [ḫu-ur?/
ri?] = (Akk.) [a-ku-u]. For *akū* "destitute" cf. CAD A¹.

arzanatar with loc. in *-anni* is as surely deverbial (contra
Ka.) from **arzanai-* as is *lahhiyatar* (loc. *-anni*) from
lahhiyai-. Ka. here confuses deverbial nouns in *-atar* (loc.
-anni) with verbal substantives in *-atar* and infinitives in
-anna.

Enough has been observed to demonstrate the difficul-
ties facing a lexicographer of Hittite. I conclude this review,
as I opened it, with my compliments to Kammenhuber.
For all its "Schönheitsfehler" her dictionary serves as a
reliable guide to the evidence and in most cases interprets
that evidence well.

The Oriental Institute of HARRY A. HOFFNER, JR.
The University of Chicago, October 1982

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Trevor R. BRYCE, *The Major Historical Texts of Early*
Hittite History. St. Lucia, University of Queensland,
Australia, 1982 (21 cm., iv + 178 S., Karten) = Asian
Studies Monograph No. 1. Preis: \$A. 4.00.

Die Frühgeschichte der „Hethiter“ und die Heraus-
bildung des hethitischen Staates in Anatolien gehören nach
wie vor noch zu den weithin ungelösten Problemen der
Keilschriftforschung. Das verfügbare inschriftliche Material
gibt sowohl wegen seiner Lückenhaftigkeit als auch seiner
Überlieferungsgeschichte der historischen Interpretation
einen erheblichen Freiraum; selbst hinsichtlich der Rolle
der „Hethiter“ und des Hethitischen im frühen 2. Jahrht.
bestehen divergierende Meinungen¹⁾. T. R. Bryce hat sich
in seinem kleinen, durch die Art des Bindens etwas schwer
zu handhabenden Band die Aufgabe gestellt, den histo-
rischen Gehalt der vier bedeutendsten Schriftzeugnisse
hethitischer früher Geschichte herauszuarbeiten. Er wendet
sich dabei, wie im Vorwort betont wird (S. 3), vorrangig an
Studenten der hethitischen bzw. altvorderorientalischen
Geschichte. Dieser didaktischen Zielsetzung entsprechen

¹⁾ Vgl. etwa G. Steiner, *Journal of Indo-European Studies* 9 (1981)
150ff.

die Anlage des Buches mit seinen häufigen Wiederholungen
und Zusammenfassungen, die Aufnahme englischer Über-
setzungen der Texte mit einem knappen philologischen
Kommentar in den Fußnoten sowie der Stil der Darstel-
lung. Bei den Übersetzungen folgt der Verf. den gängigen
Bearbeitungen, im Kommentar wird auf einige — S. 166ff.
aufgelistete — Literatur verwiesen. Die herangezogenen
Arbeiten reichen bis zum Publikationsjahr 1979; man hätte
allerdings gerade im Hinblick auf die Zielstellung die Ein-
arbeitung aller einschlägigen Titel erwarten sollen. Gerade
hinsichtlich des Umfelds der Textaussagen, vor allem des
syrischen Raumes wären eine ganze Reihe von Arbeiten
zu nennen gewesen. Der Forschungsstand ist somit nur
unzureichend dokumentiert.

Das zentrale historische Problem ist die Durchsetzung
der monarchischen Gewalt und ihrer Erbllichkeit gegenüber
den gentilpolitischen Institutionen. Die behandelten Texte
— Anitta-Inschrift, Hattusili-Annalen, Hattusili-Testament
und Telipinu-Erlaß — bieten für eine Untersuchung dieser
Problematik die wesentliche Grundlage, allerdings nicht
sie allein. Eine gründliche Auseinandersetzung mit der
Thematik wäre einmal notwendig. Anfangs offensichtlich
noch ein gentil geprägtes Wahlkönigtum, bei dem die Nach-
folge durch die Adelsversammlung bestimmt wurde (S. 112),
entwickelte sich im Laufe des älteren Hattireiches die
Designation des Nachfolgers durch den König bei notwen-
diger Zustimmung durch die Versammlung, bis zur Zeit
des Telipinu die Erbllichkeit des Amtes durchgesetzt wurde
und sich die Monarchie als absolute Gewalt zu etablieren
began. Daß dieser Prozeß nicht ohne Rückschläge und
keineswegs gradlinig verlief, erhellt gerade aus den vier
vom Verf. untersuchten historischen Texten.

Im einleitenden Teil (S. 1-20) sind drei Karten einge-
schlossen und werden einige Bemerkungen zur anatolischen
Frühgeschichte gemacht. Gerade hier wird man dem Verf.
nicht immer folgen können, so etwa hinsichtlich der Ver-
wendung des Terminus „feudal“ (S. 1)²⁾, der Bezeichnung
von Kaneš als Vasallenstaat des Akkad-Reiches (S. 11)
oder der Einschätzung des Goldes als wesentlicher Stimu-
lus für die altassyrischen Kaufleute, die nach Aussage der
altassyrischen Texte in Anatolien vor allem Silber ein-
handelten³⁾. — Kap. 1 (S. 21-48) widmet sich dem Anitta-
Text (entsprechend der Bearbeitung durch E. Neu in *StBoT*
18, 1974), wobei es allerdings problematisch erscheint, in
ihm ein Zeugnis für „major, racially-based conflicts between
an indigenous population within the Halys basin and an
Indo-European population originally settled south of the
Halys“ zu sehen (S. 35). — Im Kap. 2 (S. 49-98) geht
es um die „Annalen“ Hattusilis I., deren Übersetzung im
wesentlichen nach F. Imparati (*Studi Classici e Orientali*
14, 1965, 45ff.) und H. C. Melchert (*JNES* 37, 1978, 1ff.)
gegeben wird. Wenn als Hauptziel der hethitischen Expan-
sion nach Nordsyrien⁴⁾ handelspolitische Erwägungen an-
gegeben werden (S. 69), so scheint die Rolle des Handels
im hethitischen Wirtschaftssystem doch überschätzt zu

²⁾ Zum Problem zuletzt A. Archi, *SMEA* 18 (1977) 7ff. und Rez.,
RHA 36 (1978) 114f. — S. 1 lies statt Bernard Hrozný: Bedrich Hrozný;
sein Vortrag über die „Lösung des hethitischen Problems“ fand 1915
in Berlin statt.

³⁾ Die Erschließung der anatolischen Goldvorkommen ist zeitlich
noch nicht genauer zu fixieren, s. P. de Jesus, *AnSt* 28 (1978) 101f.

⁴⁾ Zur hethitischen Eroberung Nordsyriens vgl. G. G. Giorgadze,
VDr 1/1964, 3ff.

werden⁵⁾. Auch der „defensive Charakter“ des I. Syrien-zuges Hattusilis (S. 70) wäre noch überzeugender zu begründen; Hattusilis Selbstdarstellung ist hier wenig verlässlich. Hinsichtlich der Identifizierung des Puruna-Flusses schließt sich der Verf. nach längerer Argumentation der Gleichsetzung mit dem Pyramos/Ceyhan (zuletzt F. Cornelius) an, anders als jetzt das *Rép. Géogr.* 6, 543 (= Euphrat). Oder wäre auch der Afrin möglich? — Kap. 3 (S. 99-131) behandelt das sog. Testament des Hattusili I. (nach Sommer-Falkenstein, *HAB*); in der historischen Auswertung wird mit Recht auf die Veränderungen in Richtung erbliche Monarchie verwiesen⁶⁾. — Kap. 4 (S. 132-161) übersetzt und kommentiert den Telipinu-Erlaß (vgl. die Diss. von W. Eisele, München 1970), wobei die Rolle des *panku* abschließend doch etwas überschätzt zu werden scheint (S. 156).

Der Verf. äußert in den Kommentaren eine Reihe von interessanten Mutmaßungen, die mit den Texten vereinbar, durch sie allerdings noch nicht zu beweisen sind. Es ist zu hoffen, daß der Verf. sich — auf einer erweiterten Quellen- und Literaturbasis — auch weiterhin mit der frühen Geschichte des hethitischen Königums auseinandersetzen wird.

Berlin, Oktober 1982

HORST KLENGEL

LINEAIR A

Louk C. MEIJER, *Eine strukturelle Analyse der Hagia Triada-Tafeln*. Ein Beitrag zur Linear A-Forschung. Amsterdam, B. R. Grüner Publishing Co., 1982 (8°, VIII + 151 S.) = Publications of the Henri Frankfort Foundation, volume VIII. Preis: hfl. 80,-. ISBN 90 6032 187 1.

Bekanntlich ist im Bereich der Schriften des „Kretischen Schriftkreises“, der voralphabetischen Schriftsysteme des Ägäisraumes also, nur für die jüngsten Sprosse, auf Kreta für die Linearschrift B und auf Kypros für die kyprische Syllabarschrift, die Entzifferung gelungen: hier durch Michael Ventris 1952, dort bereits in den 1870er Jahren. Jedoch schon — um von den letztlich zugrundeliegenden piktographischen Schriftsystemen ganz zu schweigen — für die nächstältere Linearschrift A, die nach Ausweis von Textfunden auf der ganzen Insel Kreta und darüber hinaus in der Zeitspanne von etwa 1650 bis 1450 v. Chr. (Mittelminoisch IIIb — Spätminoisch Ib) verbreitet war, sieht die Lage völlig anders aus: Die Entzifferung dieses recht komplexen Schriftsystems mit Laut- und Bildzeichen (L 1 ff.), Ligaturen (Lc 1 ff.) und einer Vielzahl metrischer Zeichen (Lm 1 ff.) und die Deutung der so geschriebenen über 300 Texte stecken noch ziemlich in den Anfängen; und von gesicherten Erkenntnissen kann in der einschlägigen Forschung bis heute noch nicht gesprochen werden.

Gegen die wiederholt vorgebrachten und so zu einer gewissen Verbreitung gelangten Hypothesen des entweder griechischen oder hethitisch-luwischen, wenn nicht gar semitischen Charakters der Sprache dieser Texte sind so viele Bedenken laut geworden, daß man sie aufgeben muß. Und historisch liegt es ja wohl auch am nächsten, in dieser Sprache der Linear A-Texte, ebenso wie jüngst Alfred Heubeck in seinem Überblick über dieses weite Feld (*Schrift*, = *Archaeologia Homerica* X, Göttingen 1979, v.a. S. 23), jene „vorgriechisch-minoische“ Sprache zu sehen, die in „vorindogermanischer“ Zeit im 3. Jahrtausend v. Chr. im Balkan-Ägäisraum gesprochen worden ist und aus der die Griechen bei und nach der Einwanderung zahlreiche Wörter und Namen übernommen haben.

So ist es denn durchaus zu begrüßen, daß hier ein völlig neuer Ansatz zu einer Lösung der Rätsel um Linear A gewagt wird. Dabei beschränkt der Autor sein Material von vorneherein auf die gut 150 Tafeln und Tafelfragmente aus Hagia Triada im Süden Kretas, da diese ein im Hinblick auf Raum und Zeit geschlossenes Corpus bilden. Als Vorarbeit zu einer Entzifferung der Linearschrift A hat Meijer eine Strukturanalyse der Texte dieses Corpus im Sinne der sogenannten „kombinatorischen Methode“ durchgeführt und dabei nur ein einziges nicht textimmanentes Faktum herangezogen, nämlich die Ähnlichkeit einzelner Zeichen der Linearschrift A mit solchen von Linear B. Er setzt damit in die Tat um, was schon 1956 der Linear B-Entzifferer Michael Ventris für die Arbeit an der Enträtselung der Linearschrift A gefordert hatte, „à essayer de déterminer, d'après les idéogrammes et la structure générale du texte, de quoi il est question en gros et quels mots ont chance d'être des noms propres, etc.“ (*Études Mycéniennes*, Paris 1956, S. 267).

Kapitel I „Textkonstitution und Transkription“ (S. 1-33) bietet eine eigenständige, aus den publizierten Photographien der Texte gewonnene Lesung des gesamten Corpus in Transkription, besser: Transnumeration nach dem von Giovanni Pugliese Carratelli 1945 eingeführten System, die Abweichungen hiervon (v.a. Nichtvorkommen oder Zufügung von Zeichen, Interpretation als Varianten oder Trennung von Varianten) werden S. 1-3 genau verzeichnet. Zu dem Corpus, das in deutlich strukturierte und daher analysierbare Texte einerseits und in fragmentarische oder relativ undurchsichtige Texte andererseits unterteilt wird, muß auch noch gesagt werden, daß die Textsammlung *GORILA*, L. Godarts und J.-P. Olivier, *Recueil des inscriptions en Linéaire A* (Paris 1976 ff.), von Meijer, dessen Untersuchung schon in den Jahren 1974-1976 entstanden ist, nicht mehr systematisch ausgewertet werden konnte.

Kapitel II „Textinhalt und Transliteration“ (S. 34-55) ist zunächst den verschiedenen Kategorien von Zeichen in diesem Schriftsystem gewidmet: Dabei interessieren den Verfasser entsprechend seinem methodischen Konzept mehr die Zeichen mit symbolischer Bedeutung (Ideogramme, Zahl- und Bruchzahlzeichen) als die für die Erschließung der Sprache natürlich dann entscheidenden Lautzeichen (L-Zeichen, soweit mit anderen L-Zeichen kombiniert auftretend). Hinsichtlich der vielbehandelten Frage des Zeichenwertes der Lautzeichen (und ebenso des Symbolwertes der Bildzeichen) geht Meijer von der Arbeitshypothese aus, daß formale Gleichheit (oder Ähnlichkeit) von Linear A- und B-Zeichen auch funktionale Vergleichbarkeit bedeuten (wobei er S. 53 Anm. 9 sogar auf einander entsprechenden

THE FIRST VOLUME OF A. KAMMENHUBER'S HITTITE DICTIONARY*

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Nine years after its first fascicle appeared, volume one of the Friedrich-Kammenhuber *Hethitisches Wörterbuch* has been completed. Very little has changed during the course of its publication in response to critical reviews of the earlier fascicles. The work is marred by faulty methodology for dating Hittite texts, a confusing and inconsistent format, a dogmatic and polemic style of presenting evidence, and a love for excursus that will not allow completion of the project in the author's lifetime.

WITH THIS FASCICLE A. Kammenhuber (hereafter abbreviated Ka.) has rounded out her first volume of the *Hethitisches Wörterbuch* (abbreviated *HW2* to distinguish it from *HW*, the first edition by J. Friedrich, unassisted). In general it is a fine achievement and solid contribution to the field of Hittitology. In reviews of previous fascicles in *Bibliotheca Orientalis* I have shared my misgivings about methodology, presuppositions and individual interpretations. Sadly I must record here that I see very little evidence that Ka. has been willing to learn from the many criticisms of her earlier fascicles in reviews. Her bibliographical references continue to be selective in a very unbalanced way. She cites works of those who are not her students or allies only to find fault with them. She misquotes my oral remarks in the Wien Rencontre on the Milawata Letter and ignores entirely the published form of that paper in *AfO* Beiheft 19 (1982). Her text dating remains erratic and subjective. Texts in OS are assumed to be "originals" even when they show copyist errors. Texts which show Hurrian influence are programatically dated late, as though there was no significant Hurrian influence from the pre-Supp. I period. This has become increasingly difficult to maintain in view of the sensational new finds from the Upper City. Linguistic features of NH "Königstexte" are routinely credited to the named king without consideration that the particular exemplar may be a copy from 100 or 200 years later. The organization of her articles betrays no clear or consistent principle. Re-

marks on the evidence from lexical texts are hidden within the body of the articles in quite unpredictable places, instead of being placed consistently either at the beginning (so *AHW*, *CAD*, *CHD*, *HW1*) or end of each article. Many quoted passages have no *KBo* or *KUB* references, but are accompanied by cross references to some other hard-to-find part of her dictionary. When one laboriously follows the trail, one is often led back in a full circle! Lists of inflected forms, instead of being accompanied by text references to enable the user to find and date them for himself, are followed by meaningless frequency counts which are inevitably incorrect by the time her articles appear in print, having been made obsolete by the appearance of another volume of published texts! This is not to deny that there is much in the *HW2* which is good and helpful. But it is distressing to see the reluctance of the editor to make improvements which have been repeatedly urged upon her.

In order to allow greater precision of reference to a double columned page, I will here refer to the four quadrants of each page as: a = upper left, b = lower left, c = upper right, d = lower right.

atta-. The fascicle contains the end of the exceptionally long article on *atta-*, about whose unnecessary length I complained in my last review. The article seeks to summarize all of the information about paternity and genealogies, even though they have nothing to do with the meaning or even the manner of use of the word. The statement in 562b about word order in the gen. construction is quite misleading. If *šiuṇaš attāš* and *attāš šiuṇaš* fell together, it was in spite of the Hittite rules of word order, which do not allow for "freie Wortstellung." Ka. has a tendency to overestimate her own knowledge of ancient Anatolia and underestimate that of the Hittites themselves! There is too much patronizing of the Hittites with

* Review article of *Hethitisches Wörterbuch*: Zweite, völlig neubearbeitete Auflage auf Grunde der edierten hethitischen Texte. Lieferung 8. By JOHANNES FRIEDRICH¹ and ANNETIES KAMMENHUBER. Heidelberg: CARL WINTER UNIVERSITÄTS-VERLAG, 1984. Pp. 561-639.

⁵⁾ Zum Problem Rez., *AoF* 6 (1979) 69 ff.

⁶⁾ Zur Befürchtung des Textes (Kol. II 70 ff.), ein „Diener“ könne auf den Thron gelangen, vgl. den Brief des ägyptischen Königinwitwe an Suppiluliuma, zitiert in *KBo* V 6 III 14 f. und IV 6 f. (s. H. G. Güterbock, *JCS* 10, 1956, 94).

statements like "die die Heth. gar nicht verstanden haben" (562c) about matters of Hurrian language or religion. Cf. also "Die Heth. verstanden offenbar nicht" (564d). Occasional examples of misunderstanding do not justify the modern scholar in assuming that he or she always knows more than the ancients. Similarly, without tape recordings we cannot today be sure of the pronunciation of Hittite or Hurrian names or words. Therefore "aids" of the type "gespr. / Tesub/" are either insultingly obvious to scholars or are not nearly precise enough. For example, is it true, as stated in 561d, that the name ^dKumarpi, which most scholars regard as a genitive ("he of Kumar"), is to be pronounced /Kumarbi/ rather than /Kumarwe/ or /Kumarfe/? (Cf. Güterbock "Kumarbi" in *RLA* 6:324-30). For the Hurr. genitive see the grammars of E. A. Speiser and F. W. Bush. Ka. is right to bring in the corresponding Hurr. expressions. I personally feel that in view of the rapidly increasing textual corpus of Hurrian from Boghazköy (cf. *KBo* 30) it is unwise to use the argument from silence to affirm that the use of the "gods of the father(s)" with ^dUTU (^{URU}Arinna)/Šimegi, with Aya, and Liluri was a "Heth. Erfindung" (563a). I fail to see how *ŠA AMA-ŠU/ABI-ŠU = ma* ^d*IŠTAR* (*KBo* 16.97 rev. 28, 30) can be translated "Der *IŠTAR* ihrer Mutter/ihrer Vaters" (564d), when there is not the slightest indication in the text that ^d*IŠTAR* is genitive! On the surface the text simply reads: "the *IŠTAR* of his/her father," "the *IŠTAR* of his/her mother." In discussing the "Angst und Furcht (des Wettergottes)" (565c) Ka. refers to "XXVIII 5 + III 3ff," which unfortunately contains no mention of the expression. The restoration *ad-da-[aš DME]Š?* proposed 566c founders on the trace, which is two verticals, which cannot be MEŠ. The translation of *maršanuwan harkanzi* with "getäuscht haben" (566d) is quite impossible (cf. *CHD* III 197f. sub *maršanu*). Important distinctions in the methodology of dating Hittite texts continue to elude Ka. "Wegen UN . . . nicht vor Murs. II. geschr." is a non sequitur. The use of UN reflects the copyist, not the original composer. At best the use of UN points to a late copy, but gives no help in dating the original. The form cited in 567d as "intaktes *at-ta-at-ti-en(= ta)*" is actually *at-ti-it-ti-en(= ta)*, much more difficult than Ka. thinks because of the vowel in the ending of the acc. *atti(n)*. The reconstructed Hittite (cited by Ka. unstarred!) **attaš=maš ši wattaz* is based on the logographic writing *IŠTU UD.KAM ABIYA*, but neither the logographic writing nor its location is given (567d). As in any good reference work one is constantly referred back and forth to other parts of the article or to other

articles. Unfortunately *HW2* is plagued by incorrect cross references: "III.5(e) Ende" should be "III(B).5(e) Ende." Ka. has chosen an extremely unlikely "solution" to *ad-di-eš(-še-eš)* in *KBo* 15.10 + i 16. Her student Szabo left this word untranslated (*THeth* 1). In their reviews, Neumann and Poetto (559d) correctly understood it as denoting body parts. But Ka. insists it means "ihre Vorfahren" (559d, 568b, 570d) in spite of the fact that such a translation makes absolutely no sense in the ritual context ("her 'ancestors' are broad, but her body parts are goodly and beautiful, and her mouth . . . , her tongue . . . !"). In 568b sub V. ^{LU}*ABU BİTI* there is no reference to the discussion and complete listing of occurrences in Pecchioli-Daddi *Mestieri*. With regard to the final vowel in the first word of the Akkadogram *ABU BİTI*, Ka. has not got the matter straight: "Meistens *ABU* statt status constr. *ABI*" referring back to II. 1A Murs. II rule f. The fact is that not "meistens" but always the vocalization is *ABU* in this term. This is even true in Akkadian texts, cf. *CAD* A/1 p. 76 where even in OB Mari the accusative singular is *abu bītim taklam* and the genitive plural following *ana* is not **ana abbī būtim aqbi=ma*. No sg. **aba*, **abi*, or pl. **abbī* is attested at all for *abu bītim*. The so-called "Palaic" word *pappa-* "father" (569b) has now shown up in the composite *pappa(-)Š[ES.MEŠ]* "brothers of the same father" parallel to *annanekeš* "sisters of the same mother" (cf. Hoffner in *Memorial Volume for Abraham Sachs*, forthcoming).

After *adatar* add an entry *attatar* "fatherhood" and list as sole syllabic writing *at-ta-tar-me-et* *KBo* 13.2 rev. 15 (Hitt col. of lexical text; lost Akkadian probably *a(b)būī* "my fatherhood").

ateš-. If *KUB* 9.31 i 35 is to be analyzed as *atišš=a* (so *HW2*), then there is no certain neut. nom.-acc. *a-ti-iš-ša* as listed in *HW2*. For *KUB* 43.60 iii 27 has *a-ti-iš-š[a(-) . . .]* without context to establish a probable case. The lemma lists an *atešša-* com. probably based on *a-te-eš-šu-uš=ma* ZABAR (without context to eliminate all doubt about the case). But *HW2* doesn't indicate whether it also considers the acc. sg. *ateššan* as belonging to the *a*-stem com. noun or an *a*-stem neut. *atešša-*.

After *adihi* insert entries: *at-ka-aš-š[a(-) . . .]* *KBo* 7.37 obv. 6; and *addu* *KUB* 14.4 iii 16, 22.70 obv. 65, 33.52 ii 2, 36.85 3', 43.22 iv 16, *KBo* 18.57 rev. 42. Ka. (versus Puhvel, *HED*) seems to want to read *addu* as an Akkadogram *ADDU*, which she does not translate in advance. This means, of course, that we do not know which Akkadian word Ka. has in mind. Some of the occurrences are broken. But those which have a

well preserved context seem to me to resist interpretation as any Akkadian candidate ("throwstick," "storm, thunderstorm," "work quota, work assignment"). In *KUB* 22.70 obv. 65, *KBo* 18.57 rev. 42', and *KUB* 14.4 iii 16' *addu* seems to be an adverb. Now there is Akk. *adu* (*CAD* A/1 131) "here then, now then," but the one attestation at Boghazköy is written *a-di*, and it is never attested with doubling in the d. Not every unfamiliar word in a Hittite text which ends in *u* is an Akkadogram! I would prefer to keep *addu* as a Hittite adverb.

adupli-. Since both *CAD* and *AHW* acknowledge the variant vocalizations *atuplu* and *utuplu* for the Akkad. garment name, and the Akkad. term exists at least as early as OB, it is hard to see why Ka. wishes to disassociate the Hittite word from the Akkad. one. I would not insist that it be an Akkadogram, as apparently J. Siegelova (*Hethitische Verwaltungspraxis im Lichte der Wirtschafts- und Inventardokumente* [Praha, 1986], hereafter abbreviated *Verw.*, 706 [glossary sub *utuplu*]) does. The syntax of *ŠA* 1 TUG *adupli* *KUB* 29.39 i 4, *KBo* 6.26 ii 49 might indicate an Akkadogram, but this might be just an Akkadographic writing of the stem form of a Hittite word. But it seems unreasonable to exclude the possibility that it was at least a loanword from Akk. Ka. misunderstands the significance of the LUGAL-*uš*=*za* TUG DINGIR-LIM TUG₂.GU₂.E₃.A *aduplita* *waššiyazi* Bo 2839 iii 25'-27' (Haas, *KN* 260ff.) by interpreting *aduplita* as instrumental *adupliti*=*a*. The form is acc. pl. neut. like NINDA *harazuta*, *kišhita*, *nathita*, *hazziwita*, *hazzizita* (cf. Friedrich, *Elem.*, 60 §92b). Puhvel (*HED* 229) is unduly skeptical about the relationship to the Akkad. word and seems unaware that the Akkad. word also has an *ad/tuplu* vocalization. It is ridiculous to claim (572b) that the borrowing of a name for a garment which has cultic use requires an identical religious outlook! No term which had a religious association would ever be borrowed from one language to another if that were the case, for no two religions have identical outlooks.

au-/u- is indeed a better description of the stem than the traditional *auš-*. *HED*'s *au(š)-* is also acceptable. The *š* only appears in forms whose endings begin with a dental (*d/t*) or an assibillated one (*z*). For *-z(a)* with the verbs of visual perception see Hoffner, *JAOS* 93 (1973): 524. The Akkadogr. *NIMUR*, which Ka. merely normalizes occurs usually as *NI-MUR* but once in *KUB* 52.75 as *NI₃*(= "*NIG*")-*MUR*. The ultra-wordy review of the treatments of *au(š)-* in secondary literature is quite unnecessary. Hittitologists are well able to read the indexes of *MVAeG* by themselves.

With regard to cuneiform Luw. *mana-* see *CHD* s.v. A table of contents which stretches over more than 2 pages (574-76) is a sign of a truly disorganized article.

[576a] What is to be deduced from the observation that (so far) in OH (but not in NH!) the subject of *au(š)-* is always humans (never gods)? If one makes no interpretation of the data, of what value is it? [576a] If one is going to emend *a-ru-me-en* in OS *KBo* 22.2 obv. 10f. (which is a carefully written text), one at least needs to warn the reader by transliterating as *a-ú!*(text *-ru*)-*me-en*. The form is omitted without note in 210c and erroneously read as *a-ú-me-en* again without "!" in 301c. The plea for the emendation is made on 209b. Such glossing over of emendations should not be permitted in a serious reference book.

[577a] The statistical information under "3. Formen" has no particular value. Furthermore not even the author has attempted to interpret the significance of how many times a given form appears. If it is to show which of two competing forms is the more common, one would have to know how many of the examples came from the very same text! Furthermore, the numbers change with each new volume of texts to be published! To know that a form occurs only once is, of course, valuable as a caution. But instead of the numbers one could wish for the references to passages where the form occurs. [577b] According to the copyist's note in *KBo* VIII, *KBo* 8.42 is not "Orig.," but "mittel- bis ziemlich alt." [577c] Add Pret. sg. *uš-ki-i[r]* *KBo* 25.123 (OS).

[578d] One can change the "wöhl **ušgaweni*" to [*u*]*š-ga-u-e-ni* Bo 1781:8' (Istanbul).

[579a] For pres. sg. 3 one must not overlook the unique *uš-git*(= BU) in *KUB* 42.100 iii 37. The "pres. pl. 2" from *KUB* 31.121 ii! 10 is surely rather an imperative! [579a] The adduced "*u!-uš-ki-id-du*" from *KUB* 36.55 ii 24 is broken and very unlikely. [579b] Since the nomen agentis *LUuškišgatala-* occurs not only in Huqq. but also in Madd., we can hardly (with Ka.) set the reign of Supp. I as the terminus ante or ad quem for the double *-šk*-derivation. [579d] Considerations such as the comparison of the pres. sg. 1 of *au(š)-*, *dai-* and *pai-/piya-* should lead to a consistent normalization of *pi-iḫ-ḫi* as *peḫḫi* rather than the supposedly neutral/unmarked *piḫḫi* used by Ka. and others. The unmarked representation might be called for in the case of *pi-di* (/pidi/ by vowel harmony?), but surely not for *peḫḫi* which *teḫḫi* assures. While on the subject I will also express my objection to the writings *te-iz-zi* and the like, which do not insure objectivity, since no one advocates a pronunciation /teizzi/. Most Assyriologists do not transliterate thus.

Hittitologists only confuse outsiders when they adopt such hyper-non-interpretive renderings.

[581a] It is not clear why only the oracle texts are allowed to have a special kind of use of *au(š)-*, nor does Ka. explain that other texts too make use of the distinction of "with and without -za" in connection with seeing something in a dream. [581d] That in 13th century texts the logogram *NIMUR* can only be read as *aumen*, not *ušgawen* is plainly refuted by *KUB* 18.5 + 49.13 which uses both forms (*aumen* 5x, *ušgawen* 4x)! It is not enough to write "Trotz *ušgawen*" before gratuitously claiming that the -šk- form is not used in the pret. The prominent exception must be convincingly explained.

[582b] For *menahhanda auš-* see *CHD* sub *menahhanda*.

[583a] I suspect that the reason why in the oracle texts the gods *uški/a-*, while the mortals *au(š)-*, is that the humans visually note the phenomena of the bird flight or exta, while the gods read the meaning ("seeing" a sin or crime which has been committed) and "foresee" the predicted outcome ("seeing" destruction, death, etc.).

[585b] I find it interesting, but not convincing, that Ka. has such a sensitivity to the strong emotional differences between the treaties and the Apology of Hattusili on the one hand and the annals of Mursili and the instructions of Tudh. IV on the other. What havoc is wrought by those who fancy they have a "Sprachgefühl" for a language dead for over 2,000 years! [585d] In the Huqq. §31f. passage, line 35, it is better to interpret *na-an-za-an* as *n=an=z(a)=an* with repeated -an "her" (cf. *HW2* 41cd) rather than (with Ka.) *n=an=z(a=š)an*, since there is no other evidence of -šan with *au(š)-*.

[592] There is an error in the listing of the middle forms: pret. pl. 3 is *ú(not u)-wa-an-ta-at* *HT* 21 + *KUB* 8.80 10 (correctly listed in *StBoT* 5 and *HED*). With Neu and *HED* I see no need to assume a dittography in the form *auštat=an* in Tel. Myth *KUB* 17.10 ii 35. Ka.'s assumption that another dittography occurs in *KUB* 17.10 iv 17 is also mistaken, since *kuit* (neut.) *andan paizzi n=ašta namma šarā UL uizzi anda-da-da-an* (i.e., *anda=at=an*) *harkzi* makes perfectly good sense with =at referring back to *kuit*, and =an as the OH particle with force similar to -kan and required here because of the local adverb *anda*.

[593a] Even if Ka. is right to doubt Gurney's (in my opinion correct) translation of *AIT* 454 ii 9f., it surely does not belong in IV.2.b (intrans.) where Ka. puts it, but under IV.3 (trans.), since the object of *au(š)-* is clearly ^dU-an. It is scarcely more than a coincidence

that no sentence requiring -za or -kan with *au(š)-* occurs in the Hurrian myths. Of what value is this observation? [593b] The use of the particle -za with *šakuwai-* was discussed by me in *JAOS* 93 (1973): 529-26. I demonstrated that the transitive use required -za, while the intransitive required its absence. I did not claim that the same principle applied to *au(š)-*. A good illustration of the principle in action is the passage quoted in 593c (top of the righthand column). [593d] Ka. retains Güterbock's old reading *menin* "face" (which he has since abandoned) for *IGI(.HI.A)-in*. The correct reading, in spite of the occasional meaningless -HI.A pl. ending, is sg. *šakuin* "eye." Cf. now *CHD* sub *mena-*, which reflects Güterbock's present opinion. [593d-594a] Ka.'s "rule" about the tense of the iter. and noniter. forms of *au(š)-* in the Hitt. myths of Hurr. origin must admit of at least the following exceptions: (1) iter. in pret. *uškinun* Ullik. III A iv 11, *uškit KUB* 33.109 + 94 + *KBo* 26.70 i 15 (= *StBoT* 14, page 38f., text 1, line 20; (2) the relationship of iter. and tense in these stories may not be a feature only of the verb *au(š)-*. Furthermore, one notes that many of the pres. iteratives in these myths seem really to be present-future in force and stress repeated action. They are not just iterative in form because of an avoidance of the noniterative forms in the present tense.

[594b] Ka. has accepted my incorrect translation of *KBo* 15.33 ii 32ff. in *AIHeth* 145f. and taken it even a step further by normalizing *kue=az* in line 32, which is impossible on two counts: (1) -za must be on the first word of the clause, and (2) -za has no allomorph =az. It seems to me rather that *kueaz* is abl. and modifies *IŠTU GAD DINGIR-LIM*. I would now translate: "They hold up before the lord of the house the cloth of the deity with which the kneading troughs were covered. He observes the (now uncovered!) kneading troughs (to see) if the *IB₂.TAK₄* (remainder?) has risen." *n=at* . . . *šara appanzi* could hardly refer to the *išnureš* (so Ka.), since this would have required *n=us* (or *n=aš*) . . . *šarā appanzi* (common gender). Admittedly one expects that the lord would examine what was held up before him, but in view of the grammatical evidence perhaps the cloth is lifted in his presence, and he looks below at the uncovered *išnureš*. [594c] The mysterious object 7 *aḫ-ḫu-wa-at[- . . .]* in the inventory text *KUB* 42.19 rev.? 7' has been correctly related by Siegelova, *Verw.* 54 n. 3, to *HFAC* 8 line 3, which I would join to *KUB* 42.81 col. ii. There we find *aḫ-ḫu-wa-ta-na-za šu-u-wa-an-za*. [594d-595a] The confused conception of the word *hali-* is reflected even in the most recent brief discussion by Otten, *Or NS* 52

(1983) 138f. Ka. has elected here to render *ḫali* . . . *auš-/uški/a-* uniformly as "die Umzäunung beobachten," a translation which may read too much into the various situations. It may be closer to the mark to render that combination as "stand watch," "keep vigil," without making a concrete visible entity out of the *ḫali*. So most recently it has been translated by Aygül Süel, *Hittit kaynaklarında tapınak görevlileri ile ilgili bir direktif metni* (1985), 56f. (nöbet tutmak). In *KBo* 13.58 iii 15-17 *ḫantezzi ḫali* and *ištarniya ḫali* surely refer to watches, not enclosures. The same is true of *appazzi ḫali* "last watch" in *KUB* 42.98 obv. i 22', cf. *EGIR-ezzi ḫali* *KUB* 8.27 left edge 3b. In *KUB* 13.4 iii 18-20 either a spatial ("precinct") or a temporal ("watch") meaning would work: "Let the watch (or precinct) be divided up. And let him in whose *ḫali* the offence occurs be put to death." Only when animals are put into a *ḫali*-, or the word is used parallel to *asawar* "sheep pen," is it clear that one has to conceive of an enclosure.

[595ab] Ka. mixes in at least three examples with pronoun object (*KUB* 13.2 ii 22, *KUB* 31.105:12, *KUB* 23.77 + 13.27 obv. 15/26f.), which should have been discussed under IV.3.b. [595d] There seems to be no reason to distinguish cases where "to see" takes a noun (IV.3.a) versus a pronoun object (IV.3.b). The suggestion on 595d ("Aber im Prt.") that the noniterative occurs in the pret. in place of the iterative is amply refuted by the examples in 596a (with *uṣḡawen*). Many portions of text cited in 596a have no text reference. Apparently Ka. doesn't care how much aggravation she causes by referring users to other parts of her articles just to find out where the text comes from! It takes as much space to write "(arrow) III. 3b" as to write out the *KUB* or *KBo* reference.

[596b] Here and elsewhere Ka. often omits the macron over temporal and conditional *mān*, which is the regular way to distinguish it from the (usually short) potential and optative *man*. The text referred to on 596b and treated in extenso on 588d (*KUB* 6.45 iii 48ff.) is a Muw. prayer and as such certainly does *not* have a temporal *mān*. Furthermore Ka. misunderstands and mistranslates it. Goetze (*ANET* 398) had it basically right. It reads: "If (*mān*) one sees the god (the benefactor) or the mortal (whom the god has helped), he will say: Sure the Stormgod . . . has favored that man and . . ." *DINGIR-LAM*(!) is clearly not a nominative. *kuit* *GIM-an* (= *kuit mahḫan*) in *KUB* 26.8 ii 5 is not "sobald . . . etwas," but "like what"; cf. *CHD* sub *mahḫan* 1 b (esp. 2). [596c] When almost every other text is cited with its edition, it is remarkable that *KUB* 19.55 (AU 202) + 48.90 is cited

without reference to the article which is the source of the join and the only edition of the joined text (Hoffner, *A/O Beiheft* 19 [1982]: 130ff.). [596bc] Under "Subj. Menschen . . ." the twofold division (1) w. pres. and imper., (2) w. pret. reveals that once again Ka.'s "rule" about *au(š)-* and *uški/a-* doesn't work.

[597b] Ka.'s oft-repeated statement that *uk* in that dat.-acc. "me, to me" occurs in NH only in Muwatalli is simply untrue. Cf. *uqqa=man=wa peḫudanzi* "they might carry me off" *KBo* 4.14 ii 80 (*CTH* 123: either Tudh. IV or Supp. II). And if, for the present, we have only found it in Muw. and Tudh. IV, this surely doesn't mean that it was not used by other NH kings! Cf. too the NH passage not yet dated to a specific king's reign *KUB* 40.1 obv.(sic) 41: *ḫUTU-ŠI=za EN-YA uk IR₃-in* [. . .] "His Majesty, my lord, [. . .] me, (his) servant," and the NH royal letter *KBo* 18.48 obv. 18-19: *nu=ita=kkan GIM-an ZAG-na* [. . .] *uk iwar LUGAL KUR Kargaḡmis* [. . .] "When the King of Kargamis [. . .] you to the right [. . .] like me." A NH copy of the Hedammu myth *KUB* 33.110 ii 3'-4' preserves *uk* as a direct object in apposition to *ḫKumarbin*: "Kumarbi [began to speak] words before his mind:" *uk-man ḫKumarbin* *GIŠx* [. . .] "[. . .] might [. . .] me, Kumarbi." As an utterance of Kumarbi (3') "But I him Kumarbi (acc.);" would make no sense whatever. Cf. also *ibid.* line 11' *uk ḫKumarbin DUMU ḫA[lalu . . .]* "me, Kumarbi (acc.), the son of A[lalu . . .]" Also not dated to a particular king is *CTH* 780.2 (Allaiturahi) *KUB* 27.29 + *VBoT* 120 + i 30-31: *tuk=at ANA ḫIŠTAR 10-ŠU ēšdu uqqa=at 1-ŠU ēšdu*, where both *tuk* and *uqqa* are datives. Now it is theoretically possible that all the undated NH occurrences belong to Muwatalli, but since we have shown its occurrence in Tudh. IV, it is just as likely that the other NH occurrences were scattered among various NH kings. So much for the Muwatalli theory. [597d] It seems strange to see discussion of passages with *-kan* and the locative under "IV.3c-e (trans. ohne Part. und 'Adv.')."

[598c] A far more plausible explanation of *nu GIŠSUKUR.ḫI.A apaš naḫṣaraz uškizzi* is found in *CHD* sub *naḫṣaratt*- 2 (end). Genitives in *-š*, instead of *-aš*, are extremely rare, if they exist at all in Hittite, and should be the last resort in explaining a difficult form.

[599c] It is convenient when arguing for a theory to give little weight to apparent exceptions such as *AIT* 454 ii 9 *ḫU=wa UL uškanzi* and emphasize supporting evidence as "signifikant" and "intakt." The trouble is *AIT* 454 is also "signifikant" and "intakt"! It is clear that *UL uškanzi* means that "as a regular practice

they do not look at the Stormgod's image." Thus the need for the iterative.

[600cd] In all of this discussion of *le* there is not one reference to the *CHD* article on this word.

[601ab] The passage from *KUB* 12.62+ is quoted to illustrate *le* with *au(š)-*. Neither Hoffner, *JCS* 29 (1977): 151f. (criticized on 600c for another point), nor the *CHD* article on *le* is even mentioned. A wooden, literal translation is presented which is sheer nonsense: "Let the blind not see! Let the deaf not hear! Let the lame not run!" It was just this nonsensicalness which led me in *JCS* 29 to argue that, in addition to its usual prohibitive meaning, *le* could be used for an emphatic negation: "The blind surely doesn't see; the deaf surely doesn't hear; the lame surely doesn't run. In the same way let not the words of sorcery see the sacrificer!" My interpretation accords with the usual pattern of the *Analogiesprüche*: as such and such happens (present tense statement of fact), in the same way let . . . (desired effect of the magic). Prohibitions in both parts is a violation of the form, and in this case makes no sense. [601d] By transcribing ^dUTU with "dNN" Ka. has obscured the real meaning of *KUB* 24.5 + rev. 8. "I want to see the sun with my eyes" is an idiom which means "I want to go on living," occurring also in Murs. II's prayer regarding the Tawanana affair (cf. Hoffner, *JAOS* 103 [1983]: 187-90). *KBo* 20.31 obv. 16ff. and dupl. is also mistransliterated and misunderstood. A rather complete discussion of this passage has been given in my forthcoming article in the *Memorial Volume for Abraham Sachs*. The whole text is an instruction to a king. It is enough to note here that LUGAL-*us* is direct address ("appositional vocative") and the verb *šallanut* (like most of the verbs in the preceding context) is imperative 2 sg. *KUB* 12.21 8'-10' reads LUGAL-*uš* ^dU-*an* ^dUTU-*un* . . . *šallanut nuttariyaš* LU[GAL-*us*] *kištati* "O king, exalt the Stormgod, the Sungod (and other deities), and you will become a swift king." *nu kuit* (neut. sg.) IGI-*it* (var. *šakuit*) *uškisi* seemingly resumed by *nu apuš* (com. pl.) *šallanut* (again imperative!) is admittedly awkward. I agree with Ka. that *apuš* refers to the gods. Could the *kuit* clause be causal instead of relative? "Because you (only) see with your eyes, hear with your ears (i.e., because you are only human), exalt them (the gods, who alone have superhuman perception)!"

[603b] The transliteration and interpretation of *KUB* 9.34 iii 33f. are seriously flawed. *iyanniyan* cannot be "Schaf," and therefore its parallel *iyandan* cannot be either. Although I would not defend all the suggestions made about *ginun* (common gender!) as "Kniegelenk,"

etc., I do agree in general that *iyandan* and *iyanniyan* are participles modifying *g/kinun* and that it is a body part; note the adverb "jetzt"! Ka.'s SIG₅-*in* in line 37 must be read IGI.HI.A!-*in* and read, not as Ka.'s *dawin* (Luwian), but, since the same word occurs in the non-Luwian Ullikummi myth, as Hittite *šakuin* "eye." [603c] There is a Luwian verb for "to see" and it is *mana-* (cf. *CHD* s.v.).

[604b] The peak of occurrences of -*za*, according to Ka.'s statistics, may be Hatt. III, but this is merely because a larger volume of text from his reign has been preserved. Ka. doesn't seem to know how to use statistics at all.

[605d] Ka. doesn't know of Beckman's edition in *StBoT* 29 of *KUB* 8.35. This is too bad, because, if she did, she might discover that it is a Hittite translation of a Mesopotamian hemerology and hardly a place to find a reference to the peculiarly Hittite concept of *parā handandatar*! In fact the text read NIG₂.SI.SA₂-*tar*, which is just the scribe's wooden translation of an Akkadian noun for "justice." Neither he nor the original Akkad. author was thinking of *parā handandatar*.

[606] Since *luluti* is once marked with a Glossenkeil, it is plausible to regard it as Luwian in spite of the fact that it is not yet attested in Luwian texts. There are other Luwian Glossenkeil words not yet found in unilingual Luwian texts. If it is Luwian, of course the *l* will not appear in final position. The problems are all Ka.'s, not the *CHD*'s.

[607ff.] No mention anywhere of Güterbock's translations and contributions to A. L. Oppenheim, *The Interpretation of Dreams in the Ancient Near East* (Philadelphia, 1956)!

[608b] Tischler is right and Ka. wrong in the dating of *KBo* 4.6. Late LI signs are irrelevant to the date of the archetype; they only affect the date of the latest copy, which could be 13th century.

[609a] Ka. here repeats her unjustified claim made in 41c that the doubled -*a*- "he, she, it" in such forms as *n=an=za= an(=kan)*, *n=at=ši=at*, etc. doesn't occur before Muwatalli. Apparently the many examples in the Deeds of Supp. (author Murs. II) do not trouble her. Perhaps she attributes them all to a late scribe. How convenient.

[610c] In *CHD mân* 1 d the special usage of postpositional *mân* "like" in dream descriptions is explained. The translation "Wie im Traum mein Vater . . ." is quite out of the question. Rather "In a dream one like my father . . ." Cf. also the mistranslation in 611c (but strangely the correct understanding in 611a!). This usage filtered down the centuries to become a commonplace in apocalyptic visions ("one

like a Son of Man" in the Book of Daniel). Most of the examples in 611a of seeing a dream without $=za=kan$ are worthless, because the position where the particles would stand has been broken away.

[611bc] The contrast intended in *KUB* 5.24 ii 12ff. + 16.31 ii 1ff. between seeing a dream with or without $=za=kan$ is not clarified by Ka.'s wooden translation ("sah . . . einen Traum an sich [selbst]"). If this is all that can be said about the significance of $=za=kan$, then we really don't know what it means.

[612b] The sentence which contains "Nach der ersten Untersuchung . . . die aus der Sicht der Hethititer . . ." reflects unbelievable pomposity and ignorance of the researches of many Hittitologists. Furthermore, "dass sein Gebrauch bei jedem Vb. anders sein kann" has been known since the first studies of the particles by Goetze in the 1930s. The alleged reference of $-za$ to the "logisches Subj." which can even be the author of the text is totally unfounded. [612c] Although there is nothing especially new in the summary of the significance of $-za$ and $-kan$ with $au(\dot{s})$, every Hittitologist (following Goetze) would certainly agree that they tend to express a mental rather than a physical seeing. What Ka. overlooks is that $-za$ and $-kan$ can be present for other reasons than to add a nuance to the verb. *KUB* 14.16 (AM 42) ii 15 is a good example: *maḥḥan=ma=an=za=an=kan EGIR-pa uḥḥun* "But when I saw him behind myself, (I turned about and fought him)." There is nothing mental about this seeing. The $-za$ and $-kan$ combined with *EGIR-pa* express the idea of "behind (my)self." And although $-za$ expresses mental seeing, there are certain constructions with noun objects such as *aššulan*, *lulu* and (*parā*) *ḥandandatar* which can quite legitimately be translated as "experience," for one mentally sees a deity's favor and power by experiencing it. [612d] Contra Ka. there is no evidence in the passages referred to for the particle $=šan$ with $au(\dot{s})$. Often she mistakes $=an=za=an$ (which also occurs as $=an=za=an=kan!$) for $=an-z(a=\dot{s})an$. The discussion of what distinction, if any, existed between $uṣki/a-$ and simple $au(\dot{s})-$ has some validity, as do Ka.'s four "rules" (613cd). But the discussion doesn't go far enough. It should have been stated at the outset that the very fact that a double iterative form $uṣkiṣki/a-$ existed already in MH shows that simple $uṣki/a-$ was not clearly perceived as an iterative. For this reason we find no example of $*uṣkiwan dai-/tiya-$, a favorite construction for iteratives, but rather $uṣkiṣkiwan dai-$. And, although when closely paired with another iterative verb such as $ištamaṣki/a-$ or $azzikki/a-$ it is clear that $uṣki/a-$ was regarded as having the same "Ak-

tionsart," there are other passages that show the speaker saw no difference at all between $uṣki/a-$ and $au(\dot{s})-$. For example, one passage uses $uṣki/a-$ in disregard of all of Ka.'s four rules: *KBo* 3.4 (AM 46) ii 17f. *nu uṣkalmišanan ammel KARAŠ.ḪI.A-YA uṣkit KUR uru Arzawa=ya=an uṣkit* "My troops (not gods, but mortals [against rule 2]) saw (in preterite [against rule 4]) the thunderbolt (a physical phenomenon which lasted but an instant! against Ka.'s durative or distributive); the land of Arzawa too saw it."

[614] too is full of exceptions to rule 2 ($au(\dot{s})-$ instead of $uṣki/a-$ in non-mythological contexts with gods as the subject). *KBo* 11.1 obv. 28 [614d] with gods in nonmythological context as subject of *uwandu* (not *uṣkandu*) violates rule 1. Of course, Ka. would list this as one of the texts not observing rule 1. But is this not just an easy way to "save" the theory?

[617d] Several cases of $=an=za=an$ are misread as $=an=z(a=\dot{s})an$. Cf. my comment on (585d). I doubt that *katta* in the passages cited in 617d means "entsprechend," making it redundant with *QĀTAMMA* in the same passages. This *katta au(\dot{s})-* is rather the "examine" suggested by Sturtevant and Otten.

[618b] Ka. is buffaloes by *tuel=an ḥandan UN-an*, because she wants *handan* to be a form of the adjective *handant-*. She overlooks the obvious solution, namely, that it is the (passive) participle of *ḥā-* "to trust": "Send your trusted man." [618cff.] deals with *menahḥanda au(\dot{s})-*, which has now also been treated in *CHD* sub *menahḥanda* 4 a and 10 a. The *menahḥanda uwa-* passages [619cd] are treated in *CHD menahḥanda* 5a.

[620cd-621ab] The subdivision of *para au(\dot{s})-* into positive and negative nuance "oversee, supervise" and "overlook, disregard" has long been acknowledged; there is nothing new here. [621d] It is agreed by all that *UD-az* in contexts such as these means "by day". Of the various analyses of the case form (nominative $*ṣiwaz$, ablative $*ṣiwattaz$) Ka.'s genitive in $-ṣ$ (instead of $-aṣ!$) is the weakest.

[622c] *KBo* 13.15:3f. is worthless as evidence for *uwan ḥarzi* "gesehen hat," since the passage is too broken to control. For the erroneous reading and interpretation of *KUB* 9.34 iii 33f. see my note on (603b).

auli-. Add to the quite incomplete bibliography on this word Haas-Wilhelm, *AOATS* 3 (1974): 46 n. 1, Puhvel, *Kratylos* 25 (1980): 137, *HED* I (1984): 229-32, Tischler, *Gass.* 42f., Kühne, *ZA* 76 (1986): 85-117. Ka.'s use of li_1 and li_2 to indicate two shapes for the regular *li* sign only invites confusion with the standard

(Thureau-Dangin) Assyriological system of transliteration, in which *li* (or *li₂*) designates the NI sign when it must be read /li/. In Hittite texts Akkadian *beli* is usually written *BE-LI₂* with NI sign. And since the choice of sign shapes only affects the date of the latest copy, not the date of the original composition, it is definitely not worth the confusion to introduce a special transliteration just to show which shape was used. Besides, consistency would then require us to use numbers in transliterating all the other signs whose shapes change significantly (perhaps as many as 100) over the centuries of Hittite writing! What havoc would then be introduced into our transliterations, and what laughter it would evoke from Assyriologists! The organization of this article is chaotic. If the word means either (1) a body part of animals and humans or (2) a sacrificial animal, then those should have been the division of the article. Instead one must look under 1, 3, 4 and 5 for meaning 2, and under 2, 3, 5 and 6 for meaning 1. Nowhere does Ka. simply list the verbs which take *auli-* as direct object when it means either an animal or a body part. Wooden, literal translations of the difficult and significant contexts characterize this treatment. No helpful explanation is offered for what is meant by *aulin karp-*, or by the *linkiyaš aulin* "a. of the oath." [630c] Ka. misunderstands *CHD* 1/1 54f.'s translation of *auliyaš inan* as "a. -ailment" (by which was clearly meant "illness of the a. [body part]") and wrongly claims that the *CHD* understood a. as "ein Leiden"! Ka.'s suggestion that the body part *auli-* denoted the stomach [631a] founders on the existence of *panduḫa-* and *šarḫu-want-* (see law 90) as designations of the stomach of animals.

auri-. A better way to express the situation with regard to Akkad. *madgaltu* would be "outside of Bog. only in a Middle Assy. place name." "M. nur in Bog. gesichert" implies doubt of the authenticity of the Akkad. word, a doubt not shared by the Akkad. dictionaries, who should know better than Ka. [631b] The only OH (but NS!) occurrence of *auri-* is neuter. Only Ka.'s silence about the context alerts the reader that in fact the entire context of *KUB* 31.110 is badly broken and therefore unhelpful. [631c] There is no need in a dictionary to reproduce the entire textual base for the *BĒL MADGALTI* Instructions. Furthermore, what is given is very idiosyncratic and hardly complete. The organization of *auri-*, unlike that of *auli-*, has nothing to do with meanings. Grouping the references by inflected forms is a perfectly acceptable method, if carried through consistently (as for example Puhvel does in *HED*). Ka. does it erratically and unpredictably, which only confuses the user of her

dictionary. [631d] Under "Gen. 1" are the occurrences of *auriyaš išḫaš* (= *BĒL MADGALTI*). I didn't check them for completeness and accuracy, but what strikes one is that in spite of the many good studies of this officer (most recently Pecchioli Daddi), there is no summary of his ranking in the civil-military hierarchy, his duties or the diachronic range of his attestation (so far not in OH). These would have been quite useful to readers. [633a] For one so insistent on precise distinctions in translation, Ka. surprises me by her confusion of *šarā pai*["ging hinauf" and *katta iyanneš* "ging hinab," especially since the second action is immediately continued by *n=aš d.U-ni kattan pait*. Although *pai-* and *iyannai-* are quite similar in meaning, the latter often shows a distinct tendency to represent inchoative action, i.e., "to set out," which fits nicely in this case where it is continued by *pai-*.

auriyala-. "Bisher nur Goe. *AM* 150ff., *KBo* V 8 I 42, III 14, 16 . . ." is surely incorrect. Other references and other brief notices of this word occurred already prior to Goetze, *AM*: Tenner, *Annalertext* 104, Goetze, *Madd.* (1928): 109f., 163 (glossary entry). And the other, newer occurrences listed here have been known for some time. [634a] As stated by Hoffner, *JNES* 28 (1969): 225-30, *Madd.* (which is MH in MS) shows the late MH and NH practice of using *-z(a)* or a dat. pronoun referring back to the subject to mark nominal sentences (i.e., those with *eš-* "to be" expressed or implied) with a 1st or 2nd person subject. Ka. tries to make the observance of this rule erratic in this text by citing "Rs. 12[f.] . . . ohne *-za*," failing to inform the reader that the beginning of the clause in question, where the particles would have been is in the lacuna and therefore probably did have *-za*! [634b] A better translation of the EZEN ITU passage is found in *CHD* sub *mayant-*. Ka. should know that *4UTU-šummi-* does not mean "meine Sonne," but "unsere Sonne" ("Our Majesty"), yet she mistranslates it twice here.

awa [634d-635a]. Since *awa* alternates with *ehu* in parallel invocations of deities (mostly mountain gods) and Archi translates it "orsù," and since the Luwian word for "come" is *awi-*, one would think there might be a connection. We attribute heavy Luwian linguistic influence on Hittite to the last century of the empire, but a sporadic word borrowed in the earlier periods is certainly not impossible.

Since Ka. is usually eager to make disclaimers of false readings, she might have noted in [635a]: [*awahḫati*]. *kulei(-)awahḫati* in *KUB* 36.101 iii 11 is written with word space, as two words. Yet in the duplicate *KUB* 36.100 + *KBo* 7.14 obv. 22 no word space appears, so that Neu (*StBoT* 5 100f.) construes

it as one verb *kuleiawahh-*. Neu doesn't comment on the appropriateness of the ending *-ati*. Factitives (verbs in *-ahh-*) almost inevitably follow the *mi*-conjugation in the middle, so that one would expect the ending *-tati*. But there is one parallel to *kuleyawahhati*, also in OS, *šuniyahhati* KUB 11.1 iv 15 (*StBoT* 5 156). So Neu's interpretation may stand.

awan. [635d] "Datiertes *a-u-wa-an* daneben bei Supp. II (Beisp. bei Ka., 1974)" shouldn't be understood to mean that this spelling doesn't antedate Supp. II: cf. KUB 21.2 + 48.95 i 8 (Muw.!). [636a] The mud really flies when Ka. calls Puhvel a "moderne(r) Schreibtischgelehrte."

auwauwa-. Why is it "schwer vorstellbar" that a rhyton should be out of gold [636c], since there are many references to them in CTH 591, CTH 649 and elsewhere (*BIBRU* GUŠKIN)? It is true that a rhyton in the "shape" of a spider, if hollowed out for the beverage, would hardly hold very much. But perhaps the spider would only be depicted in relief on the body of the rhyton. Ka. attempts to eliminate possibilities other than a rhyton in the form of an *a*. by the statement "Mit einer Tierart bemalte Gefässe . . . sind weder archäolog. noch philolog. bezeugt!" Yet large Hittite vases with human figures in relief and small silver vessels with figures in low relief most certainly have been recovered. Such an objection need not exclude the possibility of the identification *awawa-* = "spider," which can appeal to the equation with Akkad. *epūtu* "spider" in the vocabulary. Neumann's suggestion that *auwauwa-* is the Luwian equivalent of Hittite *akuwakuwa-* is phonologically possible, but cannot claim certainty. Furthermore, we must remind ourselves that we do not know for sure the animal denoted by either of these words.

awiyahha[. . .]. The long portion of KUB 36.96 transcribed and translated by Ka. is worthless as a basis for lexical conclusions because it is badly broken and all restorations are purely conjectural.

awiti-. Here we see Ka. at her quibbling best. When Güterbock, Košak and others (most recently even Siegelova, Verw.) use the term "sphynx," they have in mind what the Germans call a "Mischwesen." It is

something like the Akkadian *abūbu* monster, with which indeed it is paired in KUB 42.10 rev. 6. Ka. misses this vital detail, because she is still (HW2 192 and now 638b referring back) misreading the Akkado-gram *ABŪBI* as Hittite. Indeed, she calls this passage (with others) "unergiebig"! Siegelova (*Verw.* I 114 and II 686) is more perceptive. The passage lists 4 *awiti* 4 *ABŪBI* . . . 4 UR.MAH, an enlightening sequence! It shows the *awiti* is not the same as a simple lion, but that—like the *ABŪBU*—it is a Mischwesen. Cf. von Soden, *AHW* 8 sub *abūbu(m)* 4, where an *abūbu* is depicted on a gold bar as *mupparšu* "winged." Re *awiti* . . . *išmeriyanti* note Güterbock in *FsBittel*² 204f., who calls attention to the representation of a mixed being on a leash on Hogarth's gold ring at Oxford (*Hittite Seals*, no. 195). Ka.'s reading of KUB 12.1 iv 17 2 *awitiuš ša-ša-an-i[e-e]š* is very good, but she misses the significance by translating "schlafende"! The point is that they—like the *abūbu* . . . *šurbušu* in *AHW* 8—are "crouching." A new occurrence, which though hardly revolutionary has some interest, reads: *[a]witiyaš BI [(BRU GUŠKIN sunnai)] KBo 30.175 5' w. dupl. KBo 7.46.*

[639d] Ka. closes the fascicle (and the volume) with a small list of corrigenda, which could have been much longer. Sources of the corrections are not given except for one of mine from *BiOr* 40 415. Although several may have been proposed independently by others, I note that all but one or two were proposed in my reviews in *BiOr* 35, *BiOr* 37, and *BiOr* 40.

Thus, volume one of Ka.'s *Hethitisches Wörterbuch*, the foreword of which was dated 1974, has been completed in 1984. To my knowledge at present (summer 1987) no fascicle of volume 2 has yet appeared. According to the final words of HW2 I 639d there will be 14 volumes of HW2. At this rate that means the project will be completed in the year 2114 (1974 + 140). One can only hope that there will be Hittitologists and students in those days still interested in reading the 1974 observations by Kammenhuber about our generation of scholars, theorizing on the basis of our limited published text corpus.