* * *

E. NEU und W. MEID (Ed.), *Hethitisch und Indogermanisch. Vergleichende Studien zur historischen Grammatik und zur dialektgeographischen Stellung der indogermanischen Sprachgruppe Altkleinasiens.* Innsbruck, Prof. Dr. Wolfgang Meid, 1979 (25 cm., 310 pp.) = Innsbr. Beiträge zur Sprachwissenschaft, Bd. 25. ISBN 3 85124 542 3. öS. 860.–.

Though I do not like books of this kind (which are always uneven), we may welcome this volume now it is there, as it does contain many important articles. A solution to problems, here the position of Hittite as an IE language, however, cannot be forced in this way. My own impression is that, of the alternative that either our picture of Proto-Indo-European (PIE) must be changed or that Hittite left the group earlier than the other languages, the first is certainly true (not only because of the Hittite evidence) and the second may well be true too. The impression one gets is that on too many points Hittite *seems* to represent an older phase as not to be *really* an older branch.

I arranged the articles in this order: writing, phonology, morphology (noun, pronoun, verb), lexicon, position/archaism of Hittite.

Puhvel discusses the sign *pít/pát*. He finds *pát* only in *pattar* 'dish'. Uncertain is *pi/attar* 'wing' (PIE *ŏ*, *ĕ*). Five forms have *pít*; for *pittalwa(nt)*- 'light, thin' this rests upon equation with Lat. *petilus* (*-um*, tenue et exile) and Gr. *petalon* 'leaf', about which one might be hesitant. For *padda*- 'dig' P. prefers *pè-da-* because of Lith. *bedù*. Also for *-pát* he would consider *-be* (Av. *bā* 'truly', Lith. *bà* 'surely') because of difficulties of syllabification and gemination.

F. Josephson discusses assibilation in an article which I did not understand in some places because of its extreme brevity; the argumentation is often too lapidary to be clear.

He finds some ten instances with *k̂a*- > Hitt. *sa*-, three or four with *k̂e*- > *se*-. I find none of them convincing (*sasanna*- 'lamp' < **kas*$_e$*no*-ï ξανθός is the worst). Following observations of Foley that *y* > *dy* precedes assibilation proper, he finds *y* > *dy* > *l* in Hitt. *lesi* 'liver' (Arm. *leard*, with the same development) but also *y* > *dy* > *z*. The forms *suwais* 'bird', *sankui*, 'nail' would have a palatalized laryngeal.

Gusmani ("Ittito, teoria laringalistica e riconstruzione") thinks that laryngeal theory and Hittite are a good test case for the method of reconstruction. He therefore gives some general considerations and then gives his own view on this problem.

He thinks that the idea one has about the value of our reconstructions determines the importance one gives to the Hittite evidence. He confronts the 'realistic' attitude (the reconstructions must be as close as possible to the linguistic reality) and the 'algebristic' one ('non attribuisce significato alcuno alla verisimiglianza fonetica ... solo alla coerenza del sistema funzionale soggiacente alle realizzazioni storiche ...').

I never understood this dichotomy and I do not think it does justice to anybody (For example, I am called an example of the first attitude, but I have in all my writings about laryngeals never discussed their fonetic nature, but always in fact treated them as algebristic entities).

I think we can be short about this question. Everybody will agree that our aim is to reconstruct Proto-Indo-European exactly and in all detail and all phases of its development leading to the known IE languages. The problem is that we never shall realize that. Also I think we will all agree that our insights become ever better and more precise: if somebody would not think that, he would probably look for another job. Then the problem remains, how reliable certain reconstructions are. Everybody again will agree that new evidence or new insights might alter reconstructions that seemed beyond doubt; that is not a problem specific for linguistic reconstruction. I cannot imagine that somebody is not interested in the exact reconstruction of earlier language phases: this is exactly the task of historical linguistics. What remains is that one may be overoptimistic as to the everlasting value of certain reconstructions, while another may be too sceptic about the results (while they agree on the best possible reconstruction at a given moment). Everybody too adheres to fonetic probability as a criterium, and everybody is aware of the necessity of structural considerations. It should be said then, that there is no principal or practical disagreement on the methods of linguistic reconstruction. It is not useful to describe extremes that do not exist.

Not unimportant is G.'s statement (in discussing the monovocalic theory) that fonetic or fonematic probability is not important, only the fact whether the material with which we work supports a given view. Here the extreme view (fonetic probability is not relevant, because absolute exact reconstruction is impossible) has dangerous consequences. It should be clear that the evidence of the languages of the world — whether positive or negative — is always extremely important. They can bring us to unexpected solutions (e.g. the ergative construction) or impell us to look for other, better interpretations. It would be unwise to neglect it, and so is every theory that leads to such a view.

Also I don't see the relevance for the laryngeal theory. On the one hand it is often called algebristic, on the other it is stated that "the spirit of the neogrammarians survives with many laryngealists" (n. 4). While phonetic probability is of little or no importance, phonetic improbability is a reproach to the laryngeal theory.

Such inconsistencies are found more often. The laryngeal theory may not be of greater simplicity (and is therefore to be rejected), though in the next sentence it is recognized that simplicity is no argument.

G. suggests that laryngealists neglect Hittite (e.g. 'sarebbe assurdo non valorizzarla (la documentazione ittita)'). This is simply not true. The first large study of laryngeals in one of the IE languages was H. Hendriksen's on Hittite. The fact is that the interpretation of Hittite is much more uncertain than that of Greek and Sanskrit. And the evidence of these two languages leads to the now mostly accepted form of the laryngeal theory. It is good method to start with what can be better evaluated. On the other hand nobody has ever denied the difficulty presented by Hittite. Everybody is prepared to reconsider the theory on the basis of Hittite data. But it is hard to conceive that the picture of three vowel colouring laryngeals would prove incorrect. Then it is most probable that Hittite started from that same basis. It remains possible that there were more, but no evidence for that has been found elsewhere. And if Hittite would prove that there were less, then there remain a few problems to be solved, especially in Greek.

Neu reviews the case endings with regard to the position of Hittite. He pays special attention to the use of the bare stem in naming constructions. He rejects the explanation through 'Genuskongruenz' or '-attraktion' (*a*-stems have *-a*, not ntr. *-an*, agreeing with *laman* 'name') and follows Neumann, who speaks of a vocative-commemorative, because the voc. would also have the function of mentioning (Nennen, Erwähnen). I do not understand Neu's line of thought. He first stresses that *u*-stems do not have the voc. form (in *-ui*) in this second function. Therefore one would conclude that we should *not* speak of a *vocative*, but really of *two* cases (the voc. and the case under consideration). He also stresses that the term casus absolutus or indefinitus only means that it has a zero-morfeme. This is exactly what we have in Hittite, but he rejects this term and prefers (Neumann's) vocativus. Neu then supposes that the wider function of the vocative may be inherited, and for this PIE case he chooses casus indefinitus. I suppose that he means that a casus indefinitus in PIE may have had a far wider use (e.g. subject of intransitive verbs?), which was restricted in Hittite but not to the vocative function only, as in the other languages. Such a development is not at all improbable, but I doubt whether the Hittite naming construction is evidence for this theory.

For abl. *-az* Neu accepts the explanation as **-o-ti* (for which Melchert compares Arm. *-ē<*-e-ti*), though the loss of the *-i* is hard to explain; the particle *-z* is no exact parallel, as it is enclitic: an ablative ending is not in a comparable position (it will have been a fixed ending in Anatolian because of Luw. *-ati*).

Neu thinks that the datives in *-ai* do not point to **-ōi*. They are loanwords, where *-i* was added to the fixed form *-a* (*labarna* + *i*). This seems quite probable. (See already Pedersen, *Hitt.* p. 28.)

For the dat.-loc. pl. *-as* Neu accepts the theory that it originates from loc. *-osu*, *-āsu*, with loss of *-u* as abl.sg.-*ti>-z*. This explanation is not convincing. The loss of *-u* is a problem (see above on *-z*); I don't see why it would be 'funktionslos'. (Note that Pedersen, *Hitt.* p. 33, considered the possibility that *-u* was a separate particle, so that *-ās* might have existed beside *-āsu*.) The *o*-stems had *-oisu* as far as we know; is it suggested that *-oi-* was taken from the pronouns? From *-āsu* = *-eh$_2$su* we would expect *ahsu* (but *-asu* could be analogic). I doubt whether this form was strong enough to conquer all stems and the dative. Given the datives pointing to **-bhos* (and Skt.-*bhyas*) and **-mos*, one might be tempted to separate an element **-os*, but this is a mere speculation.

Neu's conclusion is that the 'concrete' cases, and especially those of the plural, did not yet have a fixed form in PIE, and that Hittite made other choises than most languages. (If Arm. abl. *-ē>-eti* would be correct, this would be most important: it would mean that Hittite was not as isolated as it looks.)

Kammenhuber criticizes F. Starke, *Die Funktionen der dimensionalen Kasus und Adverbien im Althethitischen* (Wiesbaden 1977). She objects to the limited material, to a number of interpretations and then presents the material.

That the material is limited cannot be an objection. It is good method to consider only the oldest phase, here Old Hittite. And 200 sentences seem not too small a corpus. Some of the interpretations K. criticizes indeed seem not convincing ("Wenn jemand einen Mann *im* Feuer *ver*wirft" instead of "ins Feuer wirft"). It is not indicated why the material is presented again, and above all there is no conclusion. I looked several times if a page was missing. More than 90% of her material confirms that forms in *-a* indicate a direction (That *dai-* 'to put' has a locative in *-i*, like *pai-* 'to give', is no problem; cf. the Latin verba ponendi). So I do not know what the objection really is. It seems that the author holds that the *-a* spread from the *a*-stems. But the fact that they are more frequent there (6 or 8 against no more than 3 in any other class) does not prove that. — It is regrettable that Starke proposed a new term (Terminativ) for directivus, which was generally adopted; it should be forgotten.

Weitenberg presents the evidence for the hysterodynamic *i-* and *u*-stems. He assumes that the accusative singular got *-an* from early *-ōim>-ōm*. This was the starting point for *a*-stem forms. In this inflection fits *tanau-* 'Tanne?' together with Germ. **danwō-* with *-ōu/-u̯-*. The root forms *mahrai-/muhrai-* are supposed to preserve old ablaut **mo/eHr-/*mHr-* (with anaptyctic *u* as in *ishunau-<*sHn-*). One would expect **mHr->mar-* (with reintroduction of the *h* giving *mahr-* again). Non -IE origin should not be excluded.

Laroche discusses the anaphoric pronoun *a-*. He stresses that the lexicographic data cannot be trusted (only the textual evidence), and that 'different' forms constitute one paradigm ("De cela ... H. Pedersen est le seul à avoir pris conscience". A remark I find more often). This paradigm is as follows (slightly different from p. 151); I add the enclitic personal pronoun:

	anaphoric		*enclit. pers.*	
	sing.	plur.	sing.	plur.
nom.c.	*as-i*	(*uni-us*)	*-as*	*-e* (*-at*)
acc.c	*un-i(n)*	(**uni-us*)	*-un?*, *-an*	*-us* (*-as*)
n.-a.n.	*e-n-i*	*e*[1]	*-at*	*-e* (*-at*)
dat.	*e-da-ni*	*e-da-s*	[*-se*]	[*-smas*]
loc.	*e-di*	*e-di*		
abl.	*e-de-z*	*e-de-z*		

() Younger forms [] not relevant here

([1]Laroche has *ea — once without asterisk — for which he refers to HW^2 *a-*[1], but there I find only *e*. This is confirmed by the enclitic pronoun. Apparently the neuter did not have a separate plural ending. Would sg. *-at* replace an older *-e*?)

-i is the deictic particle. Rather than a separate stem *en-* for the neuter *en-i*, I would prefer the alternative (Laroche n8): *e with secondary *-n* as in *newa-n*. I am not convinced of a Hittite distribution "pronominal *ed, adjectival *e*, sur le modèle de *mekkis*: *mekki*". I find *e beside *-at< *-od.

L. supposes that from *eni*, reanalysed *e-ni* (cf. *uni*), resulted *ni* in *edani* and *apenissan*. For *uni* he assumes a stem *u-*.

L. stresses the necessity of "une théorie de l'indo-européen antérieure à celle des langues classiques".

Strunk in a lengthy article rejects the connection of Hitt. *hunikzi* (verletzen) with Lat. *vincere* (überlegen sein, übertreffen) on semantic grounds. On the other hand he compares the relation *hunikzi* 'stechen' : *huekzi* 'abstechen' as terminative: punctual with that between some Greek and Indo-Iranian nasal presents to their root aorists and with the terminative value assumed for the nasal presents. This is an important reason for him to give up his doubts about the IE origin of the Indian 7th class (doubts which I do not share). He thinks the other Hittite verbs can have been created after this one verb. This seems not probable to me, as there is no agreement in function or resemblance in the sound pattern (except that all end in a guttural). Etymological connection with OP *avajam* 'stach (ein Auge) aus' is considered. But I am not convinced that the Hittite verbs mean 'stechen', a translation which is only in one place possible or probable; it seems suggested by the supposed connection with the Old Persian word.

Jasanoff advances a new theory on the origin of the Hittite *hi*-conjugation. Like his recent book on stative and middle it is lucid, original and most stimulating. He rejects the explanations given, also the one that it originated from the perfect, in which I tend to agree with the author.

He posits a series of active present endings starting with *-h_2e*, *-th_2e*, *-e*, parallel to (and with no identifiable functional difference from) the series *-mi*, *-si*, *-ti*. Both could be athematic as well as thematic. The Hittite *hi*-conjugation continues the first series directly, elsewhere it would have disappeared.

This is a simple solution to the problem, but difficult to prove. Basic, of course, is the place of this conjugation in Indo-European. J. only remarks that there is no functional difference between *hi* and *mi* in Hittite either, but this, I think, is an abnormal situation, and it must derive from a system where it had a logic place. No explanation can be considered definite without a solution to this problem. Of course it might definitely lie beyond reconstruction, and the author may have done well to concentrate on finding categories that had this conjugation (and postponing the functional problem to later) (Compare now the farreaching suggestions by Kortlandt, *Lingua* 49 1979, 66-8).

One side of the problem is the relation to the other sets of endings. A priori I find a set of endings identical to that of the perfect improbable, unless both were originally one and the same. This is indeed suggested at the end. I think it is a necessary consequence of the theory. The perfect would then be only one branch of the *$\underline{h}_2e$*-conjugation. This is also Cowgill's view, but the difficulty that most languages have clear representatives of the perfect and none of the (other) *$\underline{h}_2e$*-presents brings him to adopt the Indo-Hittite theory.

The author started from the view that there existed a series *-oh_2*, *-th_2e*, *-e*. It is not quite clear to me whether he means that this are the original thematic endings. This is suggested by the remark that *-o* + *h_2e* resulted in *-oh_2*. But the thematic endings cannot be explained in this way. The old ending for 2.sg. was *-ei* (Kortlandt, ibid. 51-70, reconstructs *-eh_1i*, and demonstrates a 3. pl. *-o*). These cannot be explained from *e/o* plus Jasanoff's endings. A separate set of thematic endings must therefore be allowed.

The verbs with ablaut *o/e* (e.g. *molH-/melH-* 'grind', Lith. *málti*, OCS *mlěti*) are considered to have had a *h_2e*-present. The argument is that the ablaut shows them to have been athematic, while these verbs are never *mi*-verbs. This would require a full demonstration. Verbs with *a/e* in Hittite (*sakki*, *sekkanzi*) are indeed *$\underline{h}$i*-verbs. The type *kānki*, *kankanzi* could have *kank-*<*$\hat{k}$enk-*. I discussed these verbs (KZ 87,86ff; 88,181ff) in a different context, concluding to an old *ō/e* ablaut. This seems to me a more probable interpretation than that *ka-a-an-* indicates the accent (we never find **ka-an-ka-a-an-zi* = *kankánzi* in the plural).

Verbs with *u* and *i* extensions that have ablaut and were therefore athematic, and are *hi*-verbs are also candidates for the new present. Here *tehhi* is explained as 1sg. *$dheh_1$-i-$h_2$ei*>*tehhi*, 3sg. *$dheh_1$-i̯-ei*>*dēi̯i*>*dāi*, 3pl. *dhh_1-i̯-enti*>*tiyanzi*. I would suggest *o*-grade in the singular: *$dhoh_1$-i-$h_2$ei*>*daihai*>*tehhe*, *$dhoh_1$-i̯-ei*>*dāi̯i*>*dāi*; in the plural *e*-grade might also be considered, *$dheh_1$-i-*>*dei-* and *$dheh_1$-i̯-*>*dhēi̯-*>*tiv-*.

Kuryłowicz too gives an explanation of the *hi*-conjugation. He too rejects the idea that it arose from the perfect, that had become a preterite. "Aber eine Umformung der Präsensflexion unter dem Druck des Präteritums ist unwahrscheinlich. Eine semantische oder morphologische Motivierung der Spaltung des Präsensparadigmas bleibt hier aus". I think this is a decisive objection.

K. thinks that the *hi*-conjugation is the active created to Middle deponents, which had the — older — 3.sg. (Hitt.) -a. This would have happened through simple addition of *-i* to the endings. However, this gives *-*hai*, *-*tai*, *-*ai*, while the 3.sg. was *-*ei*. This, I fear, is a fatal objection to the idea. The Middle certainly had (only) *-*o*, the *hi*-verbs 3sg. *-i*<*-*ei*. I don't see that the remark "Was das *-i* der 3.Pers. Sing. anbelangt, kann es ebensogut aus *-*ei* entstanden sein (vgl. idg. *-(t)o* in der 3.Pers. Sing. des Mediopassivs gegenüber dem *-e* des Perfekts)" solves this problem. Also it is not clear that *-i* was sufficient to indicate that the form was active; there is no model for that.

The *i*-verbs are explained from a relation **sakkanzi*: *sak* = *pii̯anzi*: x, x = *pāi*. This is the type of relation which I do not understand.

Cowgill too discusses the *hi*-conjugation, elaborating his view that it is necessary to adopt the Indo-Hittite theory to explain it. He objects to Eichner's view on the same grounds as Kuryłowicz: "the motivation for creating new present-tense forms on the basis of preterital perfects seems absent... new forms are not created just because the formal mechanism is there... there was neither need nor room for them". (p. 13).

C.'s idea is that both the perfect and the *hi*-conjugation derived "from some earlier formation which was significantly different from both". He assumes a nominal verb based on a 3.sg. derived from a thematic adjective or noun. He then gives a sketch how the development in PIE and Anatolian can have been. He claims to be able to explain more than his predecessors.

Such an attempt must be speculative. It is difficult to discuss them because, when it would be shown that part of it is improbable, the author might admit that it might have to be modified. On the other hand it is so general that it can hardly be proven: if it is correct, it would be beyond reconstruction. I think it is premature to conclude that Indo-Hittite is the only possibility left. For example, the generally admitted fact that the *mi*-verbs and the medio-passive agree very well makes one hesitate to conclude to an entirely different relationship between Hittite and the other languages.

C. points out that the 3.sg. preterite *-s* presents a problem, when comparing the perfect endings. (He compares the *-s* of Av. 3.pl. *cikōitərəš*.) I think this point is of fundamental importance: it is such unexplained forms that point the way to older phases.

Two minor points may be mentioned. C. accepts Sommer's view that, as the optative suffix *-i̯eh₁-/-ih₁-* shows quantitative ablaut, and as Hittite went through this ablaut phase (*dāi/tiyanzi*), Hittite must have had the optative. The fact that Hittite shows no trace of the optative should warn us. (Incidentally, one might ask whether the same argument proves that *dāi/tiyanzi* and with it the *hi*-conjugation, must have belonged to both Anatolian and the other languages).

C. (n. 8) does not believe in Eichner's explanation of *mehur*. He assumes that *h* had no etymological value here, as in *ehu* 'come!' < *ei* + *u*.

Lindeman suggests that the type *tehhi* had *o*-vocalism in the singular, **noiH-h₂ei* > *nehhi*. On the basis of a 3.sg. **s(e)h₂i̯-o-* 'binds' an old *i̯e/o*-present **s(e)h₁-i̯o-* 'sows' would have been reinterpreted as **s(e)h₁i̯-o-* (and **sh₂oi-* would have induced **sh₁oi-*). I doubt whether it is necessary to assume this rather improbable reanalysis as the origin of the *-i-*.

In n. 30 L. rejects my argumentation that *o* was not coloured by h_2. He accepts the view that a number of the relevant words had $h_3 = h^u$ (not h_2) and that this was sometimes dissimilated to h_2, e.g. not **h₂eus-* > Lat. *auris*, **h₂ous-* > Gr. *ous-*, but **hᵘeus-* > Gr. *ous-*, **h⁽ᵘ⁾eus-* > *auris*. It seems to me to be a weak point that for several words such an ad hoc solution is necessary, and that Greek is supposed in some cases to have had this dissimilation but not in others; this seems arbitrary. See also *MSS* 34 (1976) 17f. On *-oh₂* I would now be less confident: Kortlandt posits h_1 in *Lingua* 49,67.

L. too doubts (n. 6) Eichner's explanation of *mehur* as **mēh₂-u̯r̥*, because one would expect an interchange *mē-* < **mēh₂-/mā-* < **meh₂-*, which is not found. Though the *ē* could have been preserved only in Hittite, I remain sceptic, as is L.

Tischler gives a list of the Hittite words that have an IE etymology. This list may prove very useful. He uses it to show that Hittite is not a 'Mischsprache', but that the number of inherited elements may be not less than in other languages. However, the only comparison he gives is with Armenian, a language reputed to have preserved only a very small part of the IE vocabulary. Also I am not certain on some essential points. In n. 42 he writes: 'Es werden nur Bestandteile des Grundwortschatzes in Betracht gezogen; das die 'Kulturwörter'... fremder Herkunft sind, ist bekannt'. Does it mean that loanwords are not counted? Then it is stated that 420 words are of IE origin, while only 240 'sicher fremder Herkunft sind'. But it is not made clear how many words are simply of uncertain origin. Note 78, giving literature on these loanwords, only mentions Semitic (and Indian) loanwords, and I find no mention of loans from other, non-IE and non-Semitic languages. The number of 'certain loans' may therefore be very subjective. Even in a language so long studied as Greek the ideas on what are loanwords differ widely.

R. Lebrun discusses Luw. *huwarti-* 'décoction' and *hurtalli-*, which cannot be 'mélange', but would be 'un objet cultuel ou attribut' but also 'malédiction'. The latter is connected with Hitt. *hurtai-* 'malédiction'. It is supposed that *hurt-* continues **huwart-*. *G. Jucquois* then tries to find the IE etymon. He connects Av. *urvāta-*, Sl. *rota*. These, however, are best analysed **u̯r-eto-*, **u̯r-otā*. If they are cognate with Gr. ῥῆσις with the enlarged root **u̯erh₁-*, it cannot be identical with Anat. **huwart-*, because Greek shows that there was no initial laryngeal. J. further tries to connect Balto-Slavic words like Lith. *versmė̃* 'source', which seems to me quite improbable.

Poetto (Some parts of the Body and Secretions) connects *anassa-* 'orifice'? with Lat. *ānus* from **āno-* with the suffix *-(s)sa* found in *genussa-*. (But what was *āno-*, **eh₂no-* or **h₂ēno-*; does Hittite represent **h₂no-*?) *gakkartani-* prob. 'shoulder-blade' is compared with OIc. *herðar* pl. 'shoulders'. *u̯alula-* 'bladder' is derived from the root **su̯el-*, OE *swellan* (outside Germanic unknown up to now). *muwa-* 'sperm', which Laroche derived from the root **meu(H)-* Pok. 741, he would rather connect with Gr. *muelós* 'marrow' (as **muu̯-elo-*; i.e. **muH-elo-*?). Semantically I do not find this convincing. *sipa-* 'Eiter'? would be **sepa-*, cognate with OIc. *safi* 'sap'. *Oettinger* shows that *sau̯itra* 'horn' is Hittite (not Luwian) and plural, now that OHitt. *sau̯itran* has been found. He derives it from the root **seuH-* in *su-ú-iz-zi* 'stösst', Skt. *suváti* 'impels' as **souH-e-tro-*. Thus the suffix *-tro-* is demonstrated for Hittite. He points out that it is one of the suffixes that were frequent in late Indo-European but rare in Hittite (like *-ti-*, *-te/or-*, *-tero-*, *-e/os-*). From which I would conclude that Hittite did not partake in this development.

Eichner explains the forms *gē̆/inussus*, *-ssi(n)* as a *ssa*-derivation of *genu-* 'knee', meaning 'Kniekehle, -gelenk', and explains the origin of *ginussarii̯a-*. I think this is perfectly acceptable. He concludes that, as there is no indication for Luwian influence, this *-ssa-* must be old in Hittite, and recognizes it also in (*hassa*) *hanzassa*. He thinks that Luw. *-(a)ssa-*, *-assi-* too was inherited, and accepts the idea that a PIE Zugehörigkeitsuffix *-so-* was used for forms like **teso* and developped in Anatolian to a productive suffix, while it died out elsewhere. (Cf. Oettinger, above, for the reverse.) A parallel is *-sor-*. This seems to me perfectly possible. (I might compare *-iH*, which forms the Latin and Celtic gen.sg. of *o*-stems, and is found in the suffix *-iHo-*.)

E. assumes that **ns* developped into *ss* when between unaccented vowels, otherwise to *nz*. I am not convinced that this is the correct solution. The idea that *genu-* has its single *n* from the zero grade *gənu-* (the inflected forms would have the accent before or after the *n*, thus supposedly requiring *nn*) seems improbable to me. It is desirable that the ideas about the accent are soon put forth as a whole.

Watkins compares NAM.RA GUD UDU 'deportees cows sheep' with formula's like LAv. *pasu vīra*. Essential for them is that a category is expressed by two elements combined asyndetically. He maintained that there is a complete taxonomy of wealth, as follows (my captions):

wealth = moving = men
animals = large = cows
horses
small = sheep
goats
non-moving = ?plants etc.
?minerals etc.

While in Akkadian texts NAM.RA means all kinds of booty, in Hittite it is only people. This use would be the result of an inherited IE way to organize the field. Hittite does not have the division men + animals, but men + (large + small animals), for which W. compares Rigvedic *paśvó gā́* and, for a sequence of three elements, *gám áśvaṃ vīrávat* 'cow, horse (and) abundance of men', or LAv. *pasvasca staorāca mašyāca* (*bizəṇgra*) 'small and large cattle and two-footed men'. In Hittite too the large animals may have been subdivided in cows and horses (together with mules and asses). (Only in India a splitting up of the small animals is found: *ajāváyaḥ* 'goats and sheep'). For the grouping of animals as large and small we find in Latin *pecus maius : minus*, or *bubus et ovibus*, in Greek το μεν μεζον προβατον ... το δε μειον.

In the Merchant Epic he finds a full list. Thus *iyata tamēta* (traditionally 'plenty and abundance') would correspond to moving + non-moving (I use moving rather than moveable); *iyatar* can be literally 'that which is going' (ON *gangandi fé*). The plants (?) are indicated by *halkiyass-a* GEŠTIN.ḪI.A-*as* ('grain and ?grape'). In the mineral sector, however, we find a list of nine items without subgrouping. This and the foregoing have no parallel in other languages. I think here we are beyond the old formula's. W. thinks this full list elucidates some places in the Iliad (ψ 259ff, 549f, H 467-75), but here I do not find evidence for old formula's: there are other elements, it is not a normal situation (army camp). That we find slaves, horses, cows, gold, bronze as valuables is no argument.

It is a pity that we do not know the Hittite terms. W. suggests that UDU with *u*-stem enlargement stands for **pekkus*. If that were true, it would prove beyond doubt that Hittite continues the IE tradition on this point. But we cannot be sure. Therefore, I think the comparison, though probable, cannot yet be considered proven: after all slaves, cows and sheep are the most important elements of booty.

Meid discusses the question whether Hittite left PIE before the other languages or not. He rejects this alternative and believes in an early separation with at first continuing communication. I fear that this is a paper solution only, which does not solve anything. Also I don't see why the author needs it, because after this he argues in all instances for a very early departure of Hittite. He goes on to deny a 'einheitliches Indogermanisch', an idea which to my mind does not contribute to a better understanding, but rather invites to an arbitrary use of dialectal differences.

In general Meid argues for the antiquity of Hittite on the basis that it represents the stage of development which must be assumed for an early phase of PIE. It is then more probable that Hittite retained this situation than that it reached this phase through extensive losses. He stresses that, even if the existence of some *forms* could be demonstrated (e.g. of the optative characteristic), it must not yet have had its later *function*.

As regards the feminine, he posits that from a system with masculine, feminine and neuter, always the latter disappears first. This is not correct. To give just one example, in Dutch the opposition masculine : feminine has virtually disappeared, while the opposition of these (this) to the neuter is in full force.

Meid gives a sketch of the origin of the *e/o*-subjunctive. It should be observed that things are at least in this respect more complicated that the thematic forms had separate endings.

In note 37 the author shows a misunderstanding of the laryngeal theory, which seems coloured by emotional expressions ('das Griechische zum Hauptzeugen ... hochzustilisieren, durch und durch laryngalistisch'). Let me repeat only that the theory (without use of the Hittite evidence) is based on comparative evidence, not on internal reconstruction (within PIE). That Greek is of major importance because it kept the three laryngeals separate, is a simple fact. The laryngeals are mentioned because they would prove that Hittite is archaic in this respect and therefore probably also in its morphology. But a language can be archaic in its phonology but not in other parts of the language system and vice versa.

V. V. Ivanov, "Syntactical Archaisms of Old Hittite" (73-8), gives a very short survey of archaic and perhaps Indo-European phenomena regarding adverbs and enclitics. He repeats his view that the construction adverb + possessive pronoun (type *katti-mi* 'to me'), also found in Old Irish, could be ancient given the subjective character of the IE verb as appears from verb endings like *-mi*. He discusses *ta* and *nu*, *-kan* and *-san*, *-(i̯)a* and *-ku* and comments on the relative sentence (but without comparative relevance). He mentions the use of the genitive in *n-as u̯astulas* 'and he is a sinner < of the sin' (cf. OIr. *is cuil* 'it is (of the) sin'), and points to the sequence negation *-kuiski* -verb parallel to OLat. *nequis violatod.* It would have been better if the author had been less succinct.

Rosenkranz has an article 'Archaismen im Hethitischen' in which he mentions his theories, many of which are very doubtful (e.g. that on an interchange media/tenuis in IE). Some space is given to questions of hypo- and parataxis. The idea that the type *u̯astulas* 'sinner' is not a genitive ('man of sin') but a nominative seems refuted by the Akkadian construction with ŠA. In general the notes are too short to demonstrate, even to illustrate, the author's ideas.

W. P. Schmid presents a new 'Verwandtschaftsmodell'. It is a geographic one. It has Baltic in the centre with too concentric circles. The first has Slavic, Italic and Germanic; Indo-Iranian in the first and the second. In the second are put languages that are not autochthonous in the countries where we find them. But are Germanic and Italic autochthonous? The languages are simply put in their present position, only in a strongly schematized picture. The *conclusion* is that Hittite can only have preserved archaisms, never shared innovations. I fear that this is no sure basis for conclusions. It is assumed that Hittite left the first circle before the satemization on the basis of two loanwords (Lith. *geležìs*, Proto-Hattic *hapalki* and *kuu̯anna-*, Lith. *švìnas*): they must have reached the satem group before the satemization, but the Hittites found them in their homeland. (The first etymology is far from certain).

Čop finds relations between Anatolian and Uralic. He thinks Anatolian was a 'Randsprache', and because of dat. pl. **-mos* in *-smas* 'them' a Northern language, so it would have been the most northerly dialect of Indo-European and thus close to Uralic. The article is full of suggestions, which are often even hard to understand, seldom elaborated so as to give something like an argumentation. Such publications are not fruitful to my mind.

The argument concerning *-smas* is of course not a reliable basis for so far-reaching conclusions. (An analysis *-sm-* with ending *-as* is more obvious.) As agreement with Uralic are mentioned *pp*, *tt*, *kk* against simple tenues elsewhere in IE. The author assumes that Hittite *pp* etc. were real double consonants. A second point is that both have postpositions. It is stated that PIE too had postpositions but lost them. The evident conclusion that there is no reason to connect them with Uralic is not discussed. Thirdly there are lexical agreements: Hitt. *neka-* 'wife'; *u̯enzi* 'beschlafen' with Ur. words for 'sich recken, sich ausstrecken'; and *lāki* 'neigen, beugen' with forms for 'to fall'. In this way I think we can find 'agreements' between all languages. The article provides no basis for serious discussion.

Leiden, January 1980 R. S. P. Beekes