

BESPRECHUNGEN

Die Laryngealtheorie und die Rekonstruktion des indogermanischen Laut- und Formensystems. Ed. A. Bammesberger. - Carl Winter. Universitätsverlag, Heidelberg 1988. 585 pp.

We can distinguish general discussions and articles on specific problems.

GENERAL *Surveys*. There are a few surveys: IE general (Schmitt, Beekes), Anatolian (Eichner), Greek (Ruijgh).

R. Schmitt gives a short and very clear survey of the most important aspects of the laryngeal theory. He stresses that, if laryngeals are accepted in some forms, they must be considered everywhere. You recognise phonemes or you don't; *tertium non datur*. - I do not agree with his statement that all laryngeals were vocalised to *a* in Armenian. We find *e-*, *a-*, *o-* in anlaut; Beekes 1987.

Beekes gives examples of all laryngeal developments in all branches of IE.

General considerations. Meid displays his doubts about the laryngeal theory with all the well known objections: it is too abstract, schematic, non-realistic, unpronounceable, only relevant to PIE, perhaps one laryngeal is enough, every language has an *a*, etc. He frankly confesses that his hesitation is partly a psychological matter (his title is "Einige persönliche und sachliche Bemerkungen zur Laryngaltheorie", a title which is changed in the table of contents!): it is difficult to accept a change of paradigm. I think I can understand this - giving up earlier views is both necessary and painful - and respect it. Only it makes a discussion difficult. I see no new arguments. His invective against the Greek prothetic vowel (*eruthrós* < **h₁rud^hróś*) gives suggestions which have all long since been worked out and proved inadequate: the following sounds can neither explain the presence versus absence of these vowels, nor their timbre (*e-*, *a-*, *o-*). The laryngeal theory explains these vowels and the negative adjectives in *ne-*, *na-*, *no-* (from **gh₁-* etc.), and the Attic reduplication at one stroke, problems for which no reasonable alternatives are available. I do not see what evidence could be more convincing. Maybe the fact that the laryngeal theory is less important for Germanic and Celtic is a factor. This does not mean, however, that Germanists can do without, as is shown by Lehmann's etymological dictionary of Gothic: neglect of laryngeals hampers the correct interpretation of the forms at every step. - Meid objects (n. 15) to the attempt to reduce all forms to a single structure. This is a serious problem. I fully agree with his view that the nominatives in *-ter* must not be derived from **-ter-s*, "während es sich in Wirklichkeit doch um ein grundsätzlich andere - und ältere - Art der Markierung handelt." I think that for heuristic purposes we must try to find a single basic structure. But in itself it is not a decisive argument. As regards the laryngeal theory, where it reduces forms to one structure, this is an unavoidable consequence or there are other reasons; and of course there are some points of uncertainty.

Seebold's article is incomprehensible to me. For example he doubts Cuny's argument about **plə-no-* by suggesting that both *l* and *ə* might have been vocalic. Apart from the utter improbability of this assumption, this would have given *-ala-* in Greek instead of *-la-* (*tlātós*). Discussing the *ē-* of Skt. *āste*, he says it is "Dehnstufe statt Schwundstufe." I understand neither his starting points, nor his conclusions. It is too superficial and too vague. He constructs theories that are both quite new and improbable. Thus he starts from a root shape *eR-eK-* which was "im Normfall, d.h. in der Hochstufe, vereinfacht zu (*e*)*ReK-*, d.h. mit 'prothetischem Vokal' ... im Griechischen, sonst mit Schwund des Vokals der ersten Silbe." The Schwundstufe was *oRK-*, "(offensichtlich durch Schwächung von *eReK-* im

Nebenton zu *oReK*- und dann Schwundstufe des *e*), und in dieser Form blieb das *o* erhalten." The initial *o*- "scheint im Lateinischen in offener Silbe gedehnt worden zu sein (*ōrāre*) ..." Such a series of phantastic, improbable ad hoc assumptions is beyond discussion. - The discussion considers only Hittite material, a well-known misunderstanding. The laryngeal theory cannot be judged in this way, as it is built primarily on other evidence. - At the end S. states the reason why he does not believe in laryngeals: PIE had phonemic long vowels, "Und wo soll diese Opposition vorhanden gewesen sein, wenn man alle wesentlichen Längen beseitigt, indem man sie als Laryngalkontraktionen erklärt?" I know of no laryngealists who deny the existence of the long vowels *e* and *o* beside the sequences *eH*, *oH*.

Mayrhofer gives a brief survey of the main arguments for three laryngeals.

Eichner gives a short but clear and severe criticism of monolaryngealism. "Der Monolaryngalismus verfügt ... über keine eigene Theorie." "Der Laryngal wird ... addiert und nicht in das System integriert. Allfällige Konsequenzen ... werden nicht gezogen."

E. gives nine rules for PIE, which are not all beyond discussion. He objects to my view that PIE had no **a* (n. 37). I admit that there are a few difficult forms, but even the development of the labio-velars in Greek still presents difficulties. Decisive is: 1) **a* is not found in any ending or suffix, only in unmotivated roots (where there seems to be some evidence); 2) already Meillet, Introduction 99, wondered about the limited distribution of PIE **a*, and the majority of his instances are explained by *h*₂; 3) you would also expect long **a*, for which there is hardly any evidence. See lastly Lubotsky 1989. I consider it as one of the most important insights provided by the laryngeal theory that PIE had no phoneme **a*.

E. writes (132 n. 29) that we should note the colouring, e.g. **stah*₂-, not *steh*₂-, because it is theoretically possible that the *a* was restored to *e*. Here we must be clear on the theoretical level. Who, like E., admits a phoneme **a* (and assumes that *e* next to *h*₂ merged with it), must write **stah*₂- as then *eh*₂ > *ah*₂ was a phonemic change. If one refutes PIE **a*, we cannot write *ah*₂ as it is a phonetic variant of phonemic */eh*₂ /, and we should use a phonemic notation as much as possible. (This is why I write *i*, *r* throughout, not *i̇*, *ṙ*, as these notation lead to false conclusions.) In this case, restoration of [*ah*₂] is impossible, as only phonemic changes can be analogically restored. As regards *e* next to *h*₃, there was a phoneme **o*, but there is evidence that the laryngeal *o* did not merge with it (Kortlandt 1983a, 1984; *h*₃ *e* and *h*₃ *o* have different reflexes in Armenian and perhaps Hittite; Lubotsky forthc.: *h*₃ *eCV* > Ilr. *aCV* without lengthening according to Brugmann's law: *oCV* > *āCV*). Thus here too we must write *eh*₃.

SPECIFIC PROBLEMS General. *Penney* uses the root structure of PIE to determine what sounds the laryngeals were. He finds indications that they were not velar fricatives, that they did not belong to one set, and that *h*₃ was not voiced. I agree with all this (cf. Beekes 1989), but I am not sure that the evidence of the root structure is decisive. - I think that there is evidence for roots in *-TH* (like **plth*₂-) as well as *TH*- (like **kh*₂*eid*-). For *H_xRH_x* add **h₁reh₁*- 'to row', *h₃neh₃*- 'name', for *HeH* add *h₁eh₁*- in Skt. *ātman*-, and *HoH* (or *e* if there was at least one *h*₃) in Gr. *ōkús*.

Peters refutes the theory that *Rh₁* between consonants could give *oRi* in Greek (instead of *Re*); and that *uh*₂/*h*₃ would have given *uā/ō* instead of *ū*. Cf. Beekes p. 73 n.4. I also agree with Peters that separate developments for *h*₁ and *h*₂, *h*₃ are improbable for other languages than Greek. And if there were separate developments,

which is a priori possible, these should be phonetically understandable, and the developments mentioned are not. I also agree that a separate development for h_1 in Tocharian is not to be expected. In this language the developments $uh_1 > \bar{u}$ and $uh_2 > w\bar{a}$ are both phonetically quite probable, but I think uH gave $w\bar{a}$.

Hoenigswald tries to use the theory of automatic vowels for the explanation of laryngeal developments, but this does not appear to be easy. Cf. Beekes 1988. Cf. also Ruijgh (below). - The forms Gr. *knātós* and *kámatos* are not phonetic variants, but different formations (*kámatos* < **kmh₂-etos* ; Waanders 1974, Beekes 1975; cf. Ruijgh p. 452 n. 25).

Oettinger discusses the nominal ending of the nominative dual. The endings $-\bar{o}$, $-\bar{i}$, $-\bar{u}$ point to $-H$, Gr. *ósse* < **h₃ek^w-h₁* shows that it was h_1 . Beside Lith. $-e$ O. points to Toch. * $-\bar{a}$ < * $-e$ and Ven. $-e$. These can only be combined into h_1/h_{1e} . My problem is that nominal endings have the structure $-(e)C$, whereas pronouns and verbs show (also) $-C(e)$. But the facts have priority.

Dunkel presents yet another study on particles. The connection with the subject of the present volume would be that a PIE * a and words beginning with a vowel must be admitted. He speaks of "unnatural constraints". This is a misunderstanding. For * a see above. The absence of vowel-initial in languages with laryngeals and pharyngeals is quite normal. And it is, of course, easier to make reconstructions when certain constraints are not observed. The facts, presented too sketchily, are far from sufficient to demonstrate the point (e.g. Gr. *hēsperos*, *etós* with **we-* beside *aúsios*, *aútós* with **au*, a connection rejected by Frisk).

Germanic. *Fulk* reconsiders the evidence for a vocalic reflex, mostly u , of a laryngeal in non-first syllable. He correctly rejects most proposals: there is no certain etymology, no laryngeal, no u , or an original u , or anaptyxis. That Angl. $-ur-$ in the s -stems has anything to do with a laryngeal is impossible (as the nominative had $-es$, we would have an ablaut $-Hes/-Hs-$, which is out of the question). The idea should be buried once for ever. For the third weak class verbs F. admits the possibility of $H_i > a_i$. As this is the only evidence for the development, it must be rejected. (Note that, in contradistinction to F.'s statement, this development is now generally rejected for Indo-Iranian.) For this class see now Kortlandt 1990, who assumes $-\bar{e}je/o-$.

F. retains one instance, **anuđi-* < **h₂enh₂ti-*. He opposes this development to that of **b^herHg-* 'birch'. Following highly dubious speculations he assumes that the laryngeal was non-syllabic in 'birch' but syllabic in 'duck'. This must decidedly be rejected: PIE had only consonantic laryngeals. As it would be the only instance of it, there can be no doubt that the 'duck' does not show a development $H > u$.

There is evidence for $H > a$ in final syllable: Kortlandt 1981, 132; Beekes 1990. - As to the 'duck', the paradigm apparently was not an i -stem as F. assumes, but **h₂enh₂-t-s*, acc. **h₂(e)nh₂-ét-m*, gen. **h₂(e)nh₂-t-ós* (Beekes 1985, 63f.). The last two forms (assuming that full grade was generalised) resulted in **anab-*, **anb-* resp.; the first must have been **anab-* (otherwise **anb-*). The u will be due to raising in acc. *anađ-un* (van Helten, PBB 1891, 460-3).

Markey discusses "the new biology of cognitive evolution and punctuated equilibria". I must confess that I do not understand it, and that I don't see the relevance to the laryngeal theory. As to the *Verschärfung* he points out that "it can and did occur dialect-independently at any time", and concludes that support for laryngeals is too weak. In fact this is not a conclusion but a statement. The article is too short and too theoretical to be convincing.

Polomé has a rich article "Are there traces of laryngeals in Germanic?" The title may be misleading; the normal effects of laryngeals are not discussed, but P. concentrates on the more debated developments: *Verschärfung*, *k* from *h*₁ and *ē*₂.

P. refutes the idea that laryngeals played a part in the origin of *ē*₂ or of the OHG *r*-preterites. Connolly's theories are rejected (cf. my remarks p. 99). This is no doubt correct; the evidence is simply negative. P. gives a survey of the *ē*₂-problem. His conclusions are that there are several sources (my ordering): 1. borrowing from Latin; 2. compensatory lengthening for the loss of -z- before dental (I think: voiced) stop; 3. lowering of the *i* in **ei* under specific conditions (in nouns and adjectives); 4. vowel contraction after the reduction of the reduplication in the strong verbs; 5. unexplained pronominal forms. I would add: 6. lowering of *i* before *r* (Ringe; with lengthening in **hē*₂*r*, as Kortlandt suggests to me); this point is in part the same as point (5); 7. the Gothic gen. pl. ending -e < **-eian* (Kortlandt 1978; 1990 sect. 7; he compares the lowering of **ē*₁ in *saian*). As to 4. I consider the contractions as very doubtful (the type **hehait-* > **hehet-* > **heet-*). On the other hand the ablaut *ai/ē*₂, *au/eu*, *aR/eR*, *ē*₁/*ē*₂ compels us to reconstruct *ē*₂ < *ei*. (With *ē*₁-verbs the *ē*₂ must be analogical.) I would suggest that this *ei* originated after the development of PIE **ei* to *i*. Thus, *ē*₂ developed from an *ei*₂. The advantage would be that no specific condition for the development *ei*₂ > *ē*₂ is necessary: a new diphthong *ei* may have developed differently. As to the nouns (3.), it seems improbable that only a very few were subject to lowering. Perhaps incidental developments are responsible in each case (a.o. point 6.).

If one does not accept Van Coetsem's inversed ablaut (which seems indeed too daring), the *e* remains to be explained. It is tempting to connect it with the *e* of the reduplication. I think that the form **rerd-* < **re-rh₁d-* (plural with zero grade of **reh₁d-*) was the starting point, as it is monosyllabic and has *e*-vocalism. From forms like this through analogical replacements the *ei* = *ē*₂-forms may have arisen rather than from the dubious sound laws suggested until now. I think that the zero grade plural forms must have been essential as the full grade forms, with -ō- in the *ē*-verbs and *a* (or *ō* ?) in the *aR*-forms, offer little promise for the kind of development we are looking for. The zero grade of the root with *CRēC*-verbs was in reduplicated forms identical with that of the *CaRC*-verbs (whether these originated from *CoRC-*, *Ch₂eRC* or *Ceh₂RC*). - Forms like **le-lh₁t-* > *lelt-* may have been dissimilated to *lei₂t-* (cf. Gr. **e-ue-uk^w-om* > *eweipon*). Perhaps also **sle-slh₁p-* was reduced to *sle-lh₁p-* (cf. Lat. *steti*) > *slelp-* etc. The strange form *slelp-* may have been changed into *slep-*; or dissimilated into *sleip-*. If in a form like **h₂e-h₂ik-* (root *aik-*) > *aīk-* (> *aik-*) the *e* of the reduplicating syllable is restored, we have **eik-*.

As to *kk* < *Hw* P. points to forms with velar (Skt. *sphig⁺* - ON *spik*, W. *llaith* < **lekto-* - ON *leka*, Latv. *dzīga* - ON *kuikr*, RuCS *stog⁺* - ON *stakkr*; but **stog-* would have given Slavic **stag-* < **stōg-*), and to the difficulty of the sequence -*Hiw*-. Goth. *sauil* < **seh₂uel-* seems enough to refute the theory. Still P. is prepared to accept a development *w* > *k*, under yet unknown circumstances, but does not think that laryngeals were a factor. - P. did not yet know Kortlandt's 1988 article. K. solves a number of problems by assuming a metathesis *Hiw* > *iHw* (the laryngeals moving to the position after the last anteconsonantal resonant). This is a phonetically probable development, and it solves **deh₂iuēr* > PGm. **daHiuer* > **daiHuēr* > *taik-* nicely. (Note that the colouring of the *e* to *a* would have been retained upon the metathesis.)

I would like to add the following objections. For *stakkr* it is not at all certain that it contains the root **steh₂-*, nor is there any evidence for a *-w-*; the etymology is a mere guess. The connection of ON *skeika* 'swerve' with *scaevus* 'left' is completely arbitrary. ON *leka* requires a root with *-e-*, which makes **leh₂u-* impossible. Also the connection of *leka* 'leak, trickle' with Lat. *lāvit* 'wash' and Hitt. *lahhu-* 'pour' is semantically not very convincing. As to 'spit', OE *spádl* etc. < **spaipla-* shows that there is no reason to derive OHG *speichaltra* etc. from **speHw-*. Thus very little remains, and I remain very sceptical.

On the Verschärfung P. notes only that it can probably better explained without laryngeals. There is a (negative) note on Rosemarie Lühr's evidence for *RH > RR* (*R = r, l, m, n*).

In conclusion P. states that he is a strong adherent of the laryngeal theory but that "it is time to abandon certain views formulated in the enthusiasm of discovery in the days of the rapid expansion of the theory."

Jasanoff connects PGm. **knē-* 'to know' with Hitt. *ganes-* and Toch. A *kñas-*, which would point to **gnē-s-*. He explains this form as a proterodynamic (= static) *s*-present **ǵnēh₃-s-/ǵneh₃-s*. The long *ē* was not coloured by *h₃* according to Eichner's rule. (Perhaps Arm. *caneay* has **cani-* < **ǵnē-*, a Lindeman form.) In PGm. **knē-jan* the *-s-* was removed because the preterite **keknō(w)-* did not have one. - An objection is that it requires the existence of an *s*-present beside a nasal-present and a *sk*-present. Rix (Kratylos 14 (1969) 184f.) suggested that the Germanic present was created "nach dem das germanische Verbum dominierende Schema Präsens *e, ē* : Präteritum *a, o*." Hitt. *gan-* and Arm. *can-* rather derive from **ǵnh₃-*.

Indo-Iranian. Strunk defends his explanation of *ījate* from **h₂i-h₂g-e-*, quite convincingly. He further defends the interpretation of Gr. *enenke/o-* from **h₁neh₁nk̑-*. It is mainly based on Av. *nāsa-*, but this form cannot be the reflex of *h₁neh₁nk̑-*, as Indo-Iranian vocalises the *n*, as is shown by *mās* 'month' < **meh₁ns* and *vāta* < **h₂ueh₁nto-*. (Pirart's suggestion to separate the latter word from Lat. *ventus* etc. and to derive it from **h₂ueh₁-to-* is quite improbable, and refuted by disyllabic *vāta-* in the Rigveda.) - The question whether the PIE form was **h₁neh₁nk-* rather than *h₁neh₁nk̑-* is wrongly put: PIE had no opposition between *n* and *n̑*, so PIE had */h₁neh₁nk̑-/*, the phonetics of which are unknown (and irrelevant); cf. Beekes 1985, 134f. However, of the later languages some vocalised *n* while others - like Greek - did not. Strunk (n. 40) objects to me that it is unclear how *nasa-* could be analogical. But is it impossible that a (reduplicated) aorist **naas-* from a root *nas-*, of which the structure had become unclear, was reshaped into **na-ns-*? The point is that it *must* be analogical. If this is what happened, the fact remains that Avestan points to a reduplicated thematic aorist.

Stephanie Jamison studies the length of Skt. *i/ī < H* in an interesting article. She concludes that *H* became *i* in final syllable before consonant. This is not convincing for two reasons. First, a long reflex in final syllables is phonetically not very probable, and certainly not in a closed syllable. Second, the only evidence is 2, 3 sg. *-īs, -īt* (notably in the *s*-aorist, i.e. from **-is-t*), and there is strong counterevidence. The type *kravīṣ* is explained as having short *i* after the neuters in *-as*, which does not convince me. Forms like *jāniṣ, -im* from **ǵ^wenh₂-* are rejected because they are inflected like *i*-stems and could be analogical. But if these forms had *-ī-*, they would hardly have become *i*-stems.

Other forms with *i* are explained analogically. Thus *brāvīti* would have *-īti* after *-īt*. Even if this is accepted, why then *āniti, vāmiti*? She adds "and because it creates a metrically attractive ... form." But language does not change to please poets. Ninth

class forms *-n-ī-* are explained as lengthened "to avoid confusion with *i-liaison*" in the perfect (*-i-ma*), which is not convincing either. More probable seems the formulation that it serves to mark the morpheme boundary. - I don't think we are on the right track. (It is a pity that she did not publish the collection of the material.)

Anatolian. Eichner summarizes his views of the developments. I only mention his rules of the origin of *h* : *hh* (my formulation): we find *hh* when the stress immediately precedes or follows (except when the preceding stressed vowel is long). The exception seems to imply that a long vowel was stressed on the first mora. So the difference does not point to different laryngeals. Lyc. \ddot{U} : *g* agree with *hh* : *h*.

Françoise Bader analyses (esp. Anatolian) kinship terms in elements like *h₂e-*, -*h₂o-*, *ne-* etc. I see no confirmation for these highly hypothetical suggestions.

Armenian. Greppin reviews the Armenian evidence. Central is Kortlandt's view that *h₂-* and *h₃-* resulted in *h-* before PIE **e*, but in zero before PIE **o*. Greppin accepts that *h-* can represent a laryngeal, but doubts Kortlandt's rule, as *h₃e-* would coincide with *Ho*. But this is exactly what Kortlandt denies. His interpretation is strongly confirmed by the fact that in Indo-Iranian the reflex of *h₃e-* is not subject to Brugmann's law, as is PIE **o* (Lubotsky 1990). - In general there is more material than Greppin gives, and sometimes he rejects reliable evidence. Thus *erek* is rejected as it could have (real) prothesis. But if the etymology with Gr. *érebos* is accepted, there was *h₁-*, which developed into *e-*, and prothesis is impossible. (Moreover every *r-* had a preceding laryngeal.) - Greppin explains *ayc* 'goat' from *h₂iǵ-*. This explanation is impossible for Gr. *aíks*, which requires **h₂eiǵ-* (which would give Arm. **hayc*). Also *ayc* 'inspection', which Kortlandt explains as **h₂oiskā*, would have zero grade. One might a priori expect from *HiC-* either *iC-* or *hiC-* or *ayC-* (and so for *HuC*). If *unkn* goes back on **h₂us-*, this would prove the first development.

Greppin suggests that some of the problems with *h-/ø* can be solved by assuming that some words with *h-* are loans from Anatolian. Now these loans are uncertain in general, and it is said that Hitt. *h-* would normally give Arm. *x-*. It also implies that *ø-* was the normal Armenian reflex, which would mean that all instances of *h-* are loans, which is impossible as these words show typical Armenian sound developments.

Balto-Slavic. Kortlandt discusses the relevance of laryngeals for Balto-Slavic accentuation. While the Baltic developments are easier, the Slavic ones are very difficult. One must have a thorough knowledge of the development of the Slavic languages and of their later accentual developments, which are phonological, morphological and analogical. This makes the field very difficult for the average Indo-Europeanist. This article may help, because it is very condensed, but that is at the same time its drawback. I may also refer to the very clear exposition of Vermeer in the 'Akten' of the VIIIth Fachtagung (to appear in the Innsbrucker Beiträge, ed. Beekes). Laryngeals are essential for the history of Balto-Slavic accentuation, and its history has now in principle been retraced. (Laryngeals and the glottalised stops (the former voiced stops) are the only source of acute intonation, aside from metatony and analogical developments.

Greek. Ruijgh gives a summary of the Greek developments and their consequences, with numerous observations and suggestions. - He assumes that a vowel developed after a laryngeal which stood between consonants (word-end counting as consonant). There are some problems with this rule. Thus R. assumes that *uHC-* gave *uC*. However, *u* is a consonant in his above rule, so in *wHC* we expect *wH_eC-* > *waC-* (*weC-*, *woC*). This is what I proposed in 1988 (e.g. *wástu* < **wH₂stu-*). From *HHC-* R. expects a long vowel, as in *ókús* (if one laryngeal was

H_3). I think that R. assumes H_eHC- > $VC-$. But if we start with HH_eC- (which is also in agreement with this rule), then we would get a short vowel, $(H)VC-$. I think that this was in fact the development, i.e. vocalisation of the second laryngeal resulting in a short vowel (as in *wástu* above). (If the rule is repeated, in HH_eC- a second vowel would arise, i.e. H_eH_eC- .) As for *okús*, it can be explained from *HoHkús*. I think, then, that the developments are more complicated. Also, in $CRHC$ a development CR_eHC- is postulated (by a separate rule), which is undoubtedly correct, but there is no explanation for this enigmatic rule and its relation to the vocalisation of the resonants.

Sihler gives a useful survey of the "Greek question" (the triple reflex). There is a discussion of the last piece of supposed counterevidence ($h_3 > a$), *dános*, supposed to belong to $*deh_3-$ 'give'. - S.'s analysis of the kinship terms as containing an element $-h_3ter-$, with e.g. $*bhra-h_3ter-$, seems most improbable to me.

Barton reconsiders *iaúō* - *áesa*. For a terminative root he expects an original root aorist with a characterised present. This leads him to posit an aor. $*h_2euh_1-t > *awe(t)$, reshaped into an *s*-aorist (like *epérasa* for $*perh_2-t$). This form also explains Arm. *aga-y*. Gr. *iaúō* is derived from $*h_2i-h_2euh_1-ti$. The (characterised, non terminative) *s*-present $*h_2uh_1-es-$ gives the presents Skt. *vásati* etc. - This surprising analysis is completely convincing. One could add that the usual reconstructions with Schwebeablaut $*h_2i-h_2eus-/h_2ues-$, or an aorist from an extended root ($*h_2i-h_2eu-/h_2ues-$), and perhaps an *s*-aorist from a root in *-s*, are all improbable. (It requires that Gr. *aulē* is a recent formation. One might posit $*h_2ouh_1-leh_2-$, with colouring of the *o* and loss of the laryngeal after *o*, but in my opinion the first is incorrect and the second doubtful.

Sara Kimball accepts that *o* was not coloured by h_3 and discusses the origin of the analogical \bar{a} 's in perfects like $*pépāga$.

Klein compares Gr. *zōós* with Lat. *vīvus* etc. and derives it from $*g^{wih_3}uos$. Also *prīato* 'bought' would represent $*prih_2to$, the difference of short and long vowel does not bother him. Forms with \bar{i} - are due to "chronological differences" or "simply unexplained variant developments". Why not try Gr. *theós* - Lat. *deus* again? Cf. also Peters' article (above).

Latin. Ringe considers isoglosses of laryngeal developments in Italic and Celtic. His conclusion is mainly based on the difference in HR_2 - ($h_3ŋ-$ > Lat. $*on-$, Cl. $*an-$; $h_2ŋ-$ > Lat. $*or-$, Cl. $ar-$). He therefore denies an Italo-Celtic unity, and rightly so, I think, at least as regards sound developments. I add that, as the (first of the) developments mentioned imply that the difference between the laryngeals was retained, this development will be older than $RH > R\bar{a}$, in which the two branches agree. - There is much in the article that is uncertain or inconclusive. Thus, the reconstruction $*daiHuēr$ is certainly wrong (for $*deh_2iuēr$). An original $*h_2usōs$ is improbable, as this type had $\bar{e}R$, not $\bar{o}R$. Therefore the conclusion of Latin (and Greek) *au-* < h_2u- must be rejected.

Hamp explains the long vowel of *ācri-* from a (de)compound $-o + h_2okri-$ > *ākri-*. On this problem see Schrijver's dissertation on laryngeals in Latin (forthc.).

Tocharian. K.T. Schmidt suggests a development RH to Toch. $\bar{a}Rk-$. I think that such a development, which is found in no other IE language, is very improbable, the more so as Tocharian vocalises laryngeals more readily than most languages. This development, only after vocalic resonant (before vowel, consonant, or both?) is an improbable phonetic conditioning. The assumption of two different vocalisations, $tṛnH-$ and $tṛṇH-$, also seems to me very unlikely. - I don't see what *tārḱ-* 'loslassen'

has to do with **terh₂*- 'durchdringen'. - We should not make all the mistakes of the beginning of the laryngeal theory again for Tocharian.

Non-Indo-European languages. Koivulehto summarises the evidence for the reflexes of laryngeals in Uralic loanwords. We find Ural. **x*, Fi-Ugr. **k* and **ʃ*. In anlaut *k*- and zero are found, the latter perhaps a development of Ural. **x*. What strikes me is that most of the words mentioned are not typical loanwords, e.g. 'kochen (in a primitive way!), bescheiden, wehen, sich (nicht) schämen, lieb, wachsen'. On the whole, then, I would rather suspect genetic relationship. Thus Kortlandt (forthc.) recently argued that **teki*- 'to do' (*dheh₁*-) and **toxi*- 'sell, bring' (*deh₃*-) are evidence for a common origin. Needless to say that it is very hard to judge the correctness of the comparisons.

The root **k^wel*- probably had no laryngeal; cf. Mayrhofer Et. Wb. 534; so nr. 8 **kulki*- should be abandoned. - Fi. *mies* cannot derive from **ghmē(n)* as IE only had *-o(n)* in this word. - Interesting is **kesi*- if the connection with **h₁jesu*- is correct, but semantically this is not the most convincing comparison.

Shevoroshkin states that the classical reconstruction of the PIE laryngeals is *h̥*, *h*, *h^w*; and that these must have been velar fricatives *ɣ*, *x*, *x^w*, as other laryngeal-like phonemes do not have a palatalised variant. As velars do not colour vowels, he rejects vowel-colouring laryngeals. As labialised phonemes leave a *w*-reflex in several languages, a labialised laryngeal should have done the same; therefore he rejects such a laryngeal. On the basis of Hittite he then reconstructs two laryngeals, an unstable one (glottal stop?), giving zero in Hittite, and a stable one, giving Hitt. *h*. (This is the 'classical' mistake of building only on Hittite.) On this basis he goes on to connect IE with the other Nostratic languages, Kartvelian, Afro-Asiatic, Dravidian, Uralic and Altaic. It should be stressed that in this way the comparison starts from wrong assumptions about PIE. The article goes on to discuss details of exceptions to doubtful sound laws, so that it becomes unreadable except for believers. (On p. 537 Nostratic "evidence" for laryngeals causing Verschärfung is given.)

The volume has a few surveys and further articles on relatively minor issues. It presents little that is really new. I think that this is typical for the situation: the laryngeal theory has found its definitive form. The most important points are that there were three laryngeals (monolaryngealism can hardly be defended any longer), and that Greek knew the "triple reflex". Only details must be worked out, among them such difficult questions as the development of sequences like *HH*-, *-HRHR*-. Compared with *Evidence* of 1965 progress has been spectacular. If in *Evidence* it was the first time that laryngeals were brought to general attention, the present volume shows the establishment of a mature theory. The importance of the laryngeal theory can hardly be overestimated. It can be compared to the discovery of the primacy of *e* and *o*, or of the vocalic resonants. It has led to a complete restructuring of the sound system of PIE, which in its turn led to the glottalic theory (which in itself is by far not as important as the laryngeal theory). The consequences for the morphology were equally far-reaching. We must be grateful to the enthusiasm of Alfred Bammesberger who brought out this volume. It is sad that Cowgill, who contributed one of the most important articles to *Evidence*, did not live to see this book, which is dedicated to his memory.

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