Lindeman, Fredrik Otto: Einführung in die Laryngaltheorie. Berlin, de Gruyter, 1970, kl.-8°, 115 S. (Sammlung Göschen 1247/1247a). 5,80 DM.

This introduction might be welcomed for several reasons: it is neither too long nor too short, it is very well informed and gives much literature, it is lucid and it is critical towards many wild ideas. But I must also say that there is much in it with which I disagree. This will be evident if one knows that the author announces (p. 11) that he does

not accept the conclusions of my Development of the PIE Laryngeals in Greek (henceforth Dev.). As, then, most of my arguments can be found in Dev., and as the informed reader will hardly find any new argument, the reviewer is at a loss what to do. I shall note the points where I disagree.

An introduction, giving the basis and the frame of the theory, is followed by a chapter on the necessity of the assumption of laryngeals. Here Cuny's argument of the zero grade of the disyllabic roots is used (*stīrṇá-* not from **strəno-*, which could only give **striṇá-*), also when before vowel (*ἔκαμον* not from **é-kmə-om*), together with Kuryłowicz's that 3 pl. **pu-n-ə-enti* would not give *punánti* but **punānti* if *ə* were a vowel. The Hittite *h* is another argument.

Follows a systematical treatment of the different developments according to the position of the laryngeal.

L. starts with three laryngeals (to end with six), H_3 being indicated structurally by the cases with *o*-vocalism instead of expected *e* (*ὄψομαι* < **H_3ek^u-*, *δίδωμι* < **-deH_3-*).

It is assumed that *o* was coloured to *a* by H_2 (§ 26 and 39), which is certainly wrong (Dev. 128, 166–8). I fail to see why *βωμός* is not acceptable as evidence (as *θωμός* is for *-oH_1-*, § 43). It is true that (absolutely) conclusive evidence is very limited, but I have never seen a conclusive counter-argument; L. gives no evidence for it. I hope to come back to the problem elsewhere.

It is denied that PIE had roots beginning with a vowel (§ 29), but there are (Dev. 90; cf. Rix, MüSS 27, 102). I admit that for 'to be' there are arguments for a laryngeal in Sanskrit (§ 35 *svastí-* trisyllabic from **su-H_1esti-*; § 47 *ásat* < **ḷ-H_1s-*) but the Greek evidence makes me doubt (see however Ruijgh's review of Dev. in Lingua 26, 184, 187ff.). But one case does not decide the matter.

For Sanskrit the laryngeal umlaut giving *-im-* < *-ṃH-* and *-$_e$mH-* (*śíma-* < **ḱ$_{(e)}$mH_2o-*) established by Kuiper is denied. I don't see that Renou's "influence possible de l'échange *ir/r*" can be presented as an alternative, to explain e.g. *śímiā* as instrumental of *śámī*. L.'s argument that *sanóti* derives from **sṇH-* (p. 44, 103f. n. 56) is contradicted by the fact that from the same root *sína-* occurs, which must be **s$_{(e)}$nHo-*. Skt. *timirá-* is a case that cannot be explained without assuming a reduced vowel (**t$_e$mHro-*; **tṃHro-* would give **tā(m)ra-*). This assumption is also necessary to explain the Greek *aRa* forms (not mentioned by L.), type *κάματος*; the existence of the reduced vowel is denied p. 72.

Kuryłowicz has rejected many of his brilliant laryngeal interpretations (he now seems to explain everything through analogy) and L. follows him. A case (p. 46 n. 23) is 1 sg. *cakára* against 3 sg. *cakára*,

explained according to Brugmann's Law (short *o* in open syllable is lengthened in Indo-Iranian) from **k^{u}e-k^{u}or-H_2e* : *k^{u}e-k^{u}or-e*. His argument for abandoning it is not convincing: the disyllabic roots (**ĝe-ĝonH$_1$-H$_2$e* : **ĝe-ĝonH$_1$-e*) should have short *a* in both forms. However, it is easy to assume that the group of disyllabic roots followed the general pattern 1 sg. *-ă-* : 3 sg. *-ā-*. More important is that we have several categories where the same explanation works: 1. 1 sg. pf. Act., type *cakára* < **k^{u}e-k^{u}or-H_2e*; 2. causatives, type *janáyati* < **ĝonH$_1$-éi̯e-ti*; 3. aorist Pass., type *ájani* < **é-ĝonH$_1$-i*; 4. second member of compounds, type *aja-gará-* < *-golHo-*[1] (*o* indicated by Skt. *g*, laryngeal by *gīrṇá-*); 5. isolated forms: *jána-* < **ĝonH$_1$o-* (*γόνος*); *samá-* < **somHo-* (*ὁμός*; laryngeal indicated by *simá-* < **s(e)mHo-*).

There can hardly be any doubt about the interpretation.

In § 52–55 a development *Hy* > *yy* (*Hw* > *ww*) is assumed: „vielleicht ... noch vor dem Aufkommen des quantitativen Ablauts entstanden". Often dubious developments are placed far back in the prehistory of PIE, perhaps because we know as little of such phases as of the supposed developments. The idea is wholly unfounded. I doubt the process. Optative forms as Skt. *deyām*, *δοίην* are concerned, which seem to derive from **deH$_3$-i̯eH$_1$-m*. Here I am inclined to believe that analogy was at work (in Sanskrit *e* introduced from the plural, **deH$_3$-iH$_1$-me* > **da-ī-ma* > **dema*, Hoffmann, Pratidānam 3; in Greek *-ι-* from the plural **deH$_3$-iH$_1$-me* > *δοιμε-*).

The problem **pō(i)-/pī-* is not brought nearer the solution (§ 56–61). L.'s suggestion of root forms **pei̯-*, **peiH-/pi̯eH-* with dissimilation **pi̯eH-* > **peH-* is unacceptable: **pei̯-* is not attested, and the dissimilation—there is not even a possibility of dissimilation.

Aspiration in Indo-Iranian is accepted, for tenues and mediae. It would have been useful to point out which laryngeals and which consonants are affected, e.g. in the following way:

laryngeal	H_1	H_2	H_3	H	
aspirates	*k*				*khánati*
	g ?				*hánu-* ?
	t ?				*pánthāḥ*
	d				*sadhástha-*
		g			*duhitár-*, *mahá-*
		t			*sthā-*, *rátha-*, *-stha* etc.
				g	*ahám*

As far as I know *p* is never aspirated, and H_3 never aspirates.

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[1] Burrow, BSOAS 20, 1957, 131–44 has shown in an important article that the word cannot have had *g^{u}-*, since then we would have had **gŭr-*. It cannot, then, be cognate with *βιβρώσκω* etc.; Burrow connects it with OIr. *gelid*, the *l* being confirmed by *giláti*.

§ 74, dubitatively formulated, should not have been written: Germ. **hauzjan* from**-e*]H_2 H_3*ous-* (the supposed root of 'ear') with $HH > k$, while *ἀκούω* would even have taken over the (an) *-a* of a preceding word. Of course, *ἀκούω* and **hauzjan* simply represent **H_2kous-*.

L. is very critical towards the hypothesis of laryngeals becoming *k* or *w*, *y*, that are all based on the interpretation of a few scattered facts. "Die vorgebrachte Evidenz ist aber meistens recht zweifelhaft und z.T. unklar" is the conclusion (§ 77) with which I fully agree.

There is an important part of the Indo-Iranian evidence which is swept away (p. 104 Anm.), and some of it is not even mentioned. Not mentioned is the fact that often Avestan zero corresponds to Indian *i*, e.g. *vr̥ṇīté* : *vərənte*, *brávīti* : *mraoiti*, *gr̥bhītá-* : *gərəpta-*, *gabhīrá-* : *ǰafra-*, *párīṇas-* : *parənah-(vant-)*. There can be only one conclusion: Avestan lost the consonantal laryngeal, and, more important, Indian, and Indian alone, vocalized this laryngeal (Kuiper, Notes on Vedic Noun-inflexion, p. 24ff.). This allows of two general conclusions: 1) the laryngeals were preserved down to Indian and Iranian (separately; and, therefore, to *all* IE languages); 2) a consonantal laryngeal could be vocalized. On the last point L. is not clear: the vowel (e.g. of Lat. *datus*) would not be a reflex of a vocalized laryngeal; "man dürfte in solchen Fällen häufig (i.e. not always?) mit ursprünglichen Gruppen von Laryngal und einem anaptyktischen Vokal rechnen" (p. 88), but also: "Vielmehr möchten wir uns hier an einer phonematischen Notation halten: . . . Schwundstufe */*CH-tó-*/" (p. 89). It is to be regretted that the badly needed clarity is lacking here. The vowel is called anaptyctic: but one that arose in PIE (and then to be expected elsewhere, not only near laryngeal; note that L. denies reduced vowels) or later? If it is later, it is of little importance, if it is in PIE, did there not exist forms **CH-to-*? They are evidently to be expected (the groups are not more difficult than many others), which is why L. at the end posits them. But most important is this: theoretical considerations (as: vocalization of a laryngeal is difficult) are necessary, but facts are primary. Sanskrit (for one) shows that consonantal laryngeals (not $_eH$ or H_e, for then Avestan would also have had a vowel) could be vocalized: why then obscure the discussion with anaptyctic vowels, that only create difficulties unnecessarily?

Indo-Iranian presents more interesting material. There are also cases where both Avestan and Indian have *i*: did Avestan also vocalize, but less often, or must we assume vocalic laryngeals for PIE (as Kuiper did, l.c.)? Then there are the Sanskrit paradigms, where *i* appears to belong to the strong cases, zero to the others: *jánima jánmanas*, *vánitā vantrá*. Here the same two possibilities suggest themselves: did Indian vocalize in some cases and not in others, or do we have PIE vocalic laryngeals beside the consonantal ones?

The interpretation of Hittite may be summarized. Vocalization to *a* is denied. H_2 and H_3 appear as *h*, H_1 as zero. The difficulties that remain are solved by introducing voiced laryngeals. Thus for *a* instead of expected *ah* or *ha* a $\underset{\cdot}{H}_2$ (§ 28) or $\underset{\cdot}{H}_3$ (§ 42) is assumed, for unexpected *eh* a $\underset{\cdot}{H}_1$ (§ 44). That this $\underset{\cdot}{H}_1$ is voiced is concluded (§ 85) from the fact that here *h* is mostly written single, while *h* near *a* is written double (*hh*). However, this gives the picture that H_2 and H_3 disappeared when voiced, while voiced H_1 remained a separate phoneme, and voiceless H_2 and H_3 remained separate phonemes while voiceless H_1 disappeared: this is improbable and shows that these are ad hoc assumptions. On the evidence for (voiced) $\underset{\cdot}{H}_2$ L. remarks: "Die meisten der vorgeschlagenen Zusammenstellungen sind aber recht unsicher" (p. 37); the better cases he gives are: *appa* and *pai-* 'give'. That is not a very safe basis to postulate a PIE phoneme. For (voiced) $\underset{\cdot}{H}_3$ there are *da-* and *pa-a-as-zi*, for $\underset{\cdot}{H}_1$ *mehur* etc. I think that one started from the cases with initial *h-* and concluded that H_1 disappeared, while H_2 and H_3 remained. But the development in anlaut may be different from that in inlaut. (That the position was a decisive factor seems to me evident from the 2 sg. ending *-ti*, not mentioned by L. Pedersen's connection of it with Skt. *-tha* $<$ *-t*H_2*e*, and his suggestion that the *t* in Hittite was by the laryngeal protected from assibilation, seems to me convincing and gives *-t*H_2*i*. We must then conclude that H_2 disappeared in this position.) We could as well start from inlaut and conclude from *pa-a-as-zi* that H_3 there disappeared and from *mehur* (the connection with **me*H_1- $>$ *mē-* 'measure' seems to me very probable) that H_1 is retained in that position. The graphic difference *h* : *hh*—if a reality at all (L. is very careful)—might as well simply continue $H_1 : H_2$ while H_3 disappeared. I would like to add that e.g. *hau̯is* 'sheep' *might* be **H*$_2$*ou̯is*, not **H*$_3$*eu̯is*. But let's wait for Mittelberger's book on the subject.

I might consider in this connection the probability of a second group of laryngeals. L. follows Andreev in considering $H_1 : H_2 : H_3$ as palatal : neutral : labio-velar ($\hat{x} : x : x^u$). I think this is right[2], or at

[2] I would like to propose to replace the inadequate symbols for the laryngeals by $\hat{x}$ (x'), x and x^u (x^o). The use of capitals (H, *A E O*) is awful, *A* etc. are specially inadequate to denote consonants. *ə* also is inappropriate for the phonetic reality, and unnecessarily has a diacritical mark; also the sign *ə* would then be free to be used for what it is intended to represent, a reduced vowel (though for clarity's sake we should at present retain the clumsy symbol *e*). I followed Kuiper in using *ħ*, but the sign is typographically difficult. The *x* is extremely simple and adequate. Instead of three signs to distinguish the laryngeals ($_1$, $_2$, $_3$) we could do with two (ˆ or ' and ^{u} or ^{o}), the same as used for the gutturals; also the simple x (= H_2) is the most frequent. For the vocalic form, if it existed, x makes no difficulty. A difficulty gives the laryngeal of unknown colour; one could use x^x.

least a probable working hypothesis. (I am convinced that the attempts to explain away one of the series of gutturals have failed. The existence of the three laryngeals strongly confirm this, and vice versa.) However, he then adds his voiced set ($\hat{\gamma} : \gamma : \gamma^u$), comparing the voiced set of gutturals ($\hat{g} : g : g^u$ beside $\hat{k} : k : k^u$). I think this is wrong, for then we would immediately expect aspirated laryngeals. Look at the structure of the PIE phonemic system we would get:

p	(b)	bh
t	d	dh
$\hat{k}$	$\hat{g}$	$\hat{g}h$
k	g	gh
k^u	g^u	g^uh
$\hat{x}$	$\hat{\gamma}$	
x	γ	
x^u	γ^u	

For aspirated laryngeals, however, there is no evidence. I would like in this connection to object to the words: "Es hat wenig Zweck, wie Martinet . . . mit Recht betont, ein für allemal eine bestimmte Anzahl von idg. Laryngalen ansetzen zu wollen" (p. 100). To my mind this is very useful. It has become a pastime to set up laryngeals on theoretical considerations. Of course, we must work with an eye on the structure we (re)build, but the facts, derived from comparison, are decisive. Theories have no value in themselves. We should stop confusing theory and facts. If somebody builds up a theory for which he cannot adduce facts, he should not publish it.

But let us return to our problem. L.'s attempt to place the six laryngeals he assumed in a structure is a legal one, only his solution is not probable. On the basis of the indications that the laryngeals in many respects (notably in root formation) behave as s I would suggest:

$\hat{k}$	$\hat{g}$	$\hat{g}h$	H_1 (or $\hat{x}$)
k	g	gh	H_2 (or x)
k^u	g^u	g^uh	H_3 (or x^u)
t	d	dh	s
p	(b)	bh	

That there is no corresponding labial is no serious drawback; the labials are often less well represented ('Labialscheu'), as there is hardly evidence for PIE b. As regards the assumption of voiced laryngeals, they could be added here, but I think they are improbable, because there is no opposition $s : z$. Thus the structure confirms the facts and vice versa, because I see not enough evidence to set up a new series of laryngeals—at present.

I will be short on Greek. L. does not accept that the opposition between the three laryngeals was preserved in Greek. I tried to demonstrate this in Dev., but I have not convinced L. Let me remark that his explanation of the *ε* of *ἄνεμος* as due to analogy of a verb **ἄνετι* or **ἀνεῖ*, "eine Umbildung eines athematischen **ἀνατι* = Skt. *ániti*" (p. 91) is an eloquent testimony to the improbability of L.'s view: from **ἀνα-τι* we might as well expect **ἀνα-ω* (cf. *ἀϱό-ω* from *ἀϱο-* < **H_2erH_3-*), but most important is that there is no trace of such a verb—and in general influence from forms supposed to be lost seldom solves a problem. I note that L. considered prothetic vowels for *ἐνεγκεῖν*, *ἔασι* and *ἐμέ* (p. 70f.); if so, he must accept $H_1 > \varepsilon$. I should also like to point out that he supposes that his six laryngeals were to a large extent preserved in Hittite ($H_2 + H_3 : H_1$, $\dot{H}_2 + \dot{H}_3 : \dot{H}_1$). If there, why not in Greek? We come up here to a more important question, i.e. that the author says he rejects the Indo-Hittite hypothesis, but in fact uses it. He even calls all IE languages except Hittite 'indogermanische' Sprachen, which should not be allowed even for brevity's sake. Compare p. 91: "Ziemlich früh fand in der Form der Grundsprache, die den 'idg.' Sprachen zugrunde liegt, ein Zusammenfall verschiedener 'Laryngale' statt"; this is the Indo-Hittite hypothesis.

I have demonstrated in my book that we find as much *ε* and *o* as *α* as prothetic vowels and that prothetic vowels mostly occur in the same words as in Armenian, that $\underset{\circ}{R}H_1$, $\underset{\circ}{R}H_2$, $\underset{\circ}{R}H_3$ give *Rη*, *Ra*, *Rω* before consonant, *εR*, *aR*, *oR* before vowel, that these two arguments are confirmed by the negative adjectives with *νη-*, *νᾱ-*, *νω-* (from $\underset{\circ}{n}$-H_1-, $\underset{\circ}{n}$-H_2-, $\underset{\circ}{n}$-H_3-), and that between consonants too we find *ε*, *α*, *o* where we expect them.

I would like to add one new piece of argument. The form *μῶνυξ* can only be explained from an element **sm̥-* and a second element beginning with a laryngeal, which is indicated by the *ὀ-* of *ὄνυξ*, i.e. **sm̥-H_3-nogh-* (see my article in Orbis 20, 1971, 138-42). This confirms $\underset{\circ}{m}H_3 > \mu\omega$, i.e. the existence of the three original laryngeals in Greek. Of course one could suppose an original **μᾱνυξ* with the *ᾱ* replaced by *ω* after the *ὀ-* of *ὄνυξ*; but then the prothetic vowel must have been *ἀ-*, i.e. **ἀνυξ*. The *ὀ-* must then be secondary, after **ongh-* < **(H)ongh-* or **H_3engh-* (Lat. *unguis*; assimilation is excluded: *ἀνύ(τ)ω*, *ἀμύγδαλον*, *ἄμυδις*, *ἀμύνω*, *ἀμύσσω*). This form however is not found in Greek. Of course it might have existed . . ., but are such chains of possibilities probable?

To my mind it is hard to understand how one can prefer Kuryłowicz's second explanation—by analogy—of the Attic reduplication above his first, laryngealistic one (p. 71f.). In Dev. 122f. I pointed out that this second attempt is extremely improbable.

Finally another important point where L. takes a step backwards. He holds that $\underset{\circ}{r}H$ etc. before consonant first became $\bar{\underset{\circ}{r}}$ etc. There is no necessity to assume such an intermediate phase, and therefore we should not assume it. We cannot abandon the principle of looking for the simplest explanation. If we can explain A directly from Y, we are not allowed to introduce an intermediate X only because it is possible. Methodically we should therefore do without $\bar{\underset{\circ}{r}}$. Of course, Greek $R\eta : R\bar{a} : R\omega$ show that $\underset{\circ}{R}H_1 : \underset{\circ}{R}H_2 : \underset{\circ}{R}H_3$ were preserved (unless we should assume $\bar{\underset{\circ}{r}}_1$, $\bar{\underset{\circ}{r}}_2$, $\bar{\underset{\circ}{r}}_3$).

Conclusion: There are many essential points on which I disagree with L. These points are: 1) (re)introduction of $\bar{\underset{\circ}{r}}$ etc.; 2) his hesitation to accept vocalization of the laryngeal; 3) introduction of a set of voiced laryngeals; 4) colouring of o by H_2; 5) the development $Hy > yy$; and his *rejection* of: 6) the survival of the opposition between the three laryngeals in Greek, 7) the explanation of Attic reduplication, 8) the explanation of the exceptions to Brugmann's Law, 9) the vocalization of laryngeals in Sanskrit only, 10) laryngeal umlaut in Sanskrit, 11) the assumption of reduced vowels for PIE, 12) the existence of roots beginning with a vowel; and 13) his dangerous inclination to the Indo-Hittite hypothesis.

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