Bammesberger, Alfred: Studien zur Laryngaltheorie. Göttingen, Vandenhoeck & Ruprecht, 1984, gr.-8°, 159 S. (Ergänzungshefte zur Zeitschrift für Vergleichende Sprachforschung, 33.) Brosch. 45 DM.

The book contains 38 small sections about individual problems in which laryngeals are concerned, mainly in Greek, Germanic and Baltic, for which the author gives a non-laryngeal alternative. I cannot discuss all of them, and I shall concentrate on Greek. B.'s main points are that he rejects the triple representation of Greek ($h_1 > \varepsilon$ etc.), and the existence of three laryngeals in (the latest phase of) PIE.

I disagree with almost everything that B. proposes (except for a few minor points which are not representative of the laryngeal theory). The book shows clearly, to my mind, that his position forces him into – often very – improbable alternatives. I must say that I cannot understand that such a distinguished scholar does not recognize that the explanations provided by the laryngeal theory are much simpler and more consistent. The book also continues a number of misconceptions about the theory which by now should have died out.

I object to the following remark (11): Jonsson distinguished between a) Anhänger, b) Gegner, c) Agnostiker. B. then remarks that you might have a fourth group, of those who plead for "eine sorgfältige Anwendung der Theorie". I think this is rather unjust against a large group of "Anhänger". – The basis for the laryngeal theory would be (10): a) theories about the root structure; b) Hittite material; c) reinterpretation of known material. In my view the starting point was formed by ablaut considerations; and the material is in the first instance provided by Greek and Indo-Iranian, and in the last by Hittite because of the many uncertainties in the historical interpretation of this language. "Auf keinen Fall soll man die Laryngaltheorie dann anwenden, wenn man mit traditionellen Methoden nicht mehr weiterkommt" (12). This is quite ununderstandable to me. I venture to say that it is unscientific: a new theory should be tried there where the old system of explanation does not work. In fact that is the best proof for the correctness of a new theory, if it explains what was unexplainable within the old system. – The existence of three laryngeals is based on Greek. "Man sollte klar feststellen, daß eine derartige Argumentation zirkulär ist" (12). It is not. Suppose all IE languages had only a phoneme *a*, and only Greek had *e, a, o*. Then the decision is made by the question whether the single or the triple vowel system is an innovation. Also, the problem is not as simple as this. Hittite *h*/*hh* might point to two laryngeals. "... empirisch faßbares Sprachmaterial steht bei solchen Rekonstruktionen [of a third laryngeal] nicht zur Verfügung" (14). But if one claims that Greek *o* in several cases directly continues h_3, this is empirical material.

The question only is how probable that statement is. – A starting point is (17 f.) that laryngeals are only admitted, "wenn unmittelbares Sprachmaterial des Hethitischen den Ansatz eines Laryngals rechtfertigt". This is the same point as the preceding. It is a methodical mistake. Suppose the IE languages had only one vowel, *a*, but Sanskrit showed palatalization before certain *a*'s, that would make a strong case for a PIE **e*; if then one language turned up that had an *e* exactly in the cases where we predicted it, we would consider that as proof for a PIE **e*. But then, in principle (there will have been analogical changes), every instance of palatalization in Sanskrit would have to be considered as evidence for an **e*, not only those cases where an *e* was preserved in that new language. (In fact B. does not stick to that principle, e. g. when he admits a laryngeal in *pánthāḥ*. In fact only one Hittite form with a *h* is mentioned.)

There is a tendency to oversimplify matters. Thus in the conclusion it is said that the Greek triple reflex "kann jedenfalls bei einigen zentralen Beispielen als Neuerung erklärt werden" (142); and immediately following: "Der lautgesetzliche Reflex von *ə* (...) ist *α* im Griechischen". I cannot understand such a conclusion. Add to this p. 59: "Das Hauptbeispiel [for h_3 > *o*] bietet das Paradigma für ‚pflügen'" (*ἀρόω*). I wrote a book on the subject, but I am quite surprised to read this.

The author is also unaware of a number of minor problems: 28 *sth$_2$ent* > *stent*; 44 *ǵn̥Hō-* > *ǵnō-*; 126 *ryHos* > *r̥yos*; 132 *dheHm̥* > *dhem̥* > *dhem*; 113 *dH-wōs* > Lith. **dawōs* (a laryngeal was never vocalized in Balto-Slavic), all of which are wrong to my mind.

I shall now discuss some sections.

2. Greek nasal presents. B. remarks that there is no certain evidence for *-nēmi* or *-nōmi*. The latter type may have switched to *-nūmi* through 1 pl. *om-n-H-men* > *omnamen*, which was changed to *-nomen* (after 3 pl. *-nonti*), which again became *-numen* in labial environment. Now B. draws two conclusions, both of which are wrong to my mind. First, the absence of *-nēmi* pleads against h_1 > *e*. "Es ist nicht einzusehen, warum etwa **βαλλεμεν* [from **g^{w}l̥-n-h$_1$-men*] ..., im Griechischen fehlt" (24 f.). I think it is quite easy to see why: **βαλλεμεν* would be a type on its own and was therefore eliminated. And the fact that the roots in h_1 and h_3 changed to *-nūmi* rather suggests that these rare types were eliminated. (As to *omnūmi, om-n-h$_3$-men* > *omnomen* would give B.'s form directly, as he admits.) Second, I don't think B.'s explanation is "ebensogut" (as what exactly?). For from **omnamen* I would rather expect reshaping into *-nāmi*. Or, if *-nomen* was analogically created, reshaping into a thematic present.

3. For $h_1 > e$, $h_3 > o$ in Greek B. discusses *τίθημι* and *δίδωμι* and gives an analogical explanation. Thus "dürfte diese Hauptstütze für den Ansatz von drei phonologisch distinkten Laryngalen, ..., entfallen" (31). Not at all. Even if the analogical explanation would be fully acceptable, it remains that we posited for reasons of ablaut **deh₃*-, rather than **doH*-; then we expect **dh₃* > *do* (at least a reflex different from h_2); and in fact we find only *do*-. It is to opponents of this view to show that their system as a whole is better. (A basic mistake is, of course, to consider this question in isolation.)

5. B. thinks that **es*-, **ed*-, **ei*- did not have initial laryngeal, for *ὑγιής* cannot be derived from *h_1*su*-; *h_1*dont*- would have given **edont*-; you would expect **ειμεν* from *h_1*y*-. *ὑγιής* is a problem, but B. does not mention that *ἐυ*- "good" beside Skt. *su*- must represent *h_1*su*- as it is almost impossible that Greek introduced *ἐ*- (from "to be" or from whereever else). Thus there is a problem, but things are not as simple as B. suggests. *ὀδοντ*- derives from *h_3*dont*-, as is shown by Arm. *atamn.* *h_1*i-men* gives regularly *ἴμεν* (also according to M. Peters, Untersuchungen).

9. B.'s explanation of *ὄσσε* is improbable (assume **ὄσσον*, du. **ὄσσω*, which influenced **οτε* = **ok*ʷ + *e*) and inferior to derivation from **ok*ʷ*ih*$_1$. B. defends a PIE dual ending *-e* on the basis of OLith. *-e*. I have no explanation for these forms, but I am very hesitant to admit this ending (beside -h_1; perhaps -h_1*e*?), because the *o*-stems, which derive from the consonant stems (see my Origins), had -*oh*$_1$, and because the *i*- and *u*-stems, which are consonant-stems, had -h_1 too. The Old Irish forms, type *carait,* could be the nom. pl. (for -*nt*-h_1 > -*nta* would have given **carat*, which was identical to the gen. pl.) rather than have the neuter ending -*ī*, as Rix assumed. B. considers *-*ie* as a possible source of -*ī*, but such a development has no support anywhere. It disregards Kuiper's demonstration that it contained a laryngeal (see the references given in my Development 145); for Balto-Slavic see Kortlandt, Slavic Accentuation 44. Skt. *janasī* would not prove a neuter ending -*ī*; it looks like an ending of the *i*-stems. That may be true, but it is a neuter ending in Sanskrit and may have been so in PIE. He does not explain the *ā*-stem ending Skt. -*e*, which may contain the same -*ī*. B. reproaches Forssman for assuming **ok*ʷ*yh*$_1$ for Greek beside -*ih*$_1$ for Balto-Slavic. The same mistake was made by Lindeman (Triple Representation 47). It is the phoneme sequence /-*ih*$_1$/ (which one may as well write /-*yh*$_1$/) which developed differently in different languages. – The essential thing in the comparison of Greek with Balto-Slavic is that both groups have an unexpected -*i*- in the dual; unexpected unless we accept that it had the dual (which we expect in the case of "eye") end-

ing -ih_1 which is also found in Skt. *janasī*. The fact that these forms are isolated in Greek and Balto-Slavic proves that they are archaic forms.

10. B. explains *ϑεός* from **dheh₁-o-*, and *ϑεσ-* from **ϑασ-* < **dhh₁s-* with *-e-* from *ϑεός*. But **dheh₁-o-* would have contracted early; it disconnects *ϑεσ-* from *ϑεός*; it has no semantic basis; it disconnects *ϑεός* from the Armenian and Italic words, while still a form **dhHs-* is posited for *ϑεσ-*. It is clear that this is no serious alternative and that Rix's explanation proves h_1 > Gr. *e* without a doubt.

13. For *N̥H-C* B. assumes *ā̆* (sic); to distinguish between the two nasals, *m* or *n* could be introduced either before *ā̆* or in the middle of it, giving *μᾰ* and *αμα*; as *αμα* was identical with full grade *αμα* (*h_2emh_2), *εμε, μη, ομο, μω* were created to *emh₁*, *omh₁* etc. (I may have misunderstood details because I don't understand the tables on pp. 63–65). But there is not the slightest evidence for *ᾰ* (one would expect relic forms); while *μᾰ* for *ᾰ* could be considered, insertion in *ᾰ* is linguistically horrible; the analogy (giving *εμε* etc.) won't work as full grade *αμα* is much less frequent than *εμα* etc. (*ερε* would be analogical after *εμε*; it is not said what the phonetic reflex of *r̥H* was). These proposals show to what monstrosities one comes to avoid the classical laryngeal theory.

31. Even the brilliant explanation of the middle participle is denied by B. I use to present the problem to my students as an example of the methodology (to show how simple it is to find the right solution; and that it takes 200 years to find it): which (single) form must be postulated for the suffixes Skt. *-V-māna-, -C-āna-*, Av. *-V-mna-, -C-āna*. The form must have been *-mXna-*. The *ā*, which is found in both languages and will therefore be regular, can only be explained from *-m̥H-*, which is the vocalization expected after consonant. Av. *-mna-* is regular, Skt. *-māna-* must be analogical and can easily be so. Prk. *-mīna-* confirms the explanation as nicely as can be. If *H* was h_1, Gr. *-menos* is also regular. It is, I think, because B. does not accept h_1 > *e* that he rejects this extremely simple solution to this old problem. (His alternative is unacceptable, especially the "Vrddhiableitung" *-meno-* from *-mn-*, which is never found in suffixes.) OPr. *-manas* (once) could be for **-mnas*.

32. In the "ablaut *ēi/ī*" B. shows not to understand the great progress made by Kuryłowicz's interpretation *-eH(i)-* / *-Hi-*. It implies e.g. that *ēi* < *eHi* before consonant is impossible. The zero grade *ī* ("völlig unklar") requires metathesis of *-Hi-*, which must have occurred very often (see Development 174, and now Kortlandt, Ériu 32, 1981, 15 f.). B. assumes a root ***poiH-* „to drink" (with *-o-*!), with which the facts cannot be explained at all.

Positive are the clarity and briefness with which B. introduces the problems, and the explicitness with which analogical changes are studied. E.g. § 6: The aor. *ἐβίων* is explained from $*g^w ieh_3$*-t*, $g^w ih_3$*-me*, $g^w ih_3$*-ent.* The *-ō-* of the singular was added to *βῑ-, βι-* of the plural. (B. assumes wrongly that $g^w ih_3$*-V* gave $g^w y$-, and *ǵn̥H-V* > *ǵnV-*. These forms remained disyllabic, as is shown by many forms, e.g. *ἔκαμον, βίος*, the latter also according to the author from $*g^w ih_3$*-o-*.) A nice parallel is the explanation of *ἑάλων* suggested to me by Lubotsky: $*\underset{\frown}{u}leh_3$*-t*, $\underset{\frown}{u}l̥_3$*-me*, $\underset{\frown}{u}l̥h_3$*-ent* giving **u̯lōt, u̯lōme, u̯alont;* these forms were levelled to **u̯alō*. (Slightly different Rix, Gr. Gr. 74. This form is strong evidence against the laryngeal umlaut in Greek: we do not find ***u̯olō-*. I think it must be given up. Cf. KZ 96, 208[3].) *βίοτος* derives from $*g^w ih_3$*-eto-*, not from $*g^w ih_3$*-to-*.

A few short remarks on other sections: 4. Skt. *deyām.* B. assumes introduction of the full grade. Kortlandt takes the full grade from the optative itself (FS Hoenigswald, in the press). – 11. Cret. *ἄρατρον* would be the regular reflex, *ἄροτρον* would have its *-o-* from **áro-* "das Pflügen" or **aró-* "der Pflüger". This is quite improbable, but the Cretan form is a problem. – 14. B. rejects Skt. *ījа-* < $h_2 i$*-*$h_2 ǵ$*-e-* because such presents would only occur beside a root aorist. I don't believe rules of this type can be found. And cf. *μένω : μίμνω.* The laryngeal explanation is better than analogy after *sad-, sīda-*. – 15. I agree that Lat. *sēdēs* may have its long root vowel from a root noun. But that **sēs* was replaced by *sēdēs* after oblique *sēd-* seems very doubtful; cf. *pēs, pedis.* I agree also that OIc. *sjǫt* does not represent **sedH.* But for *sádhiṣ, sadhástha-* the best explanation remains **sed-H-s*, gen. *s(e)d-H-és-s.* B.'s *sa-dhi-* is semantically less probable, and so is an analogical *s*-stem. – 16. B. objects to $*h_1 e$*-*$h_1 d$*-* > Lat. *ēd-ī* as there was no old perfect and as it is doubtful that the laryngeals were preserved till one was formed. This is a point to be considered. His explanation that in sg. **e-od*, pl. *e-d* the latter was replaced by *e-ed-* implies a reduplicating vowel without a consonant, which is more doubtful than the laryngeal explanation. B.'s suggestion that the long *ē*-preterites originated from *Te-TeK* with dissimilatory loss of the second *T*, is extremely improbable. – 21. Germ. *kann, kunnu-* is derived from a nasal present. B. thinks that the *-u-* cannot have arisen in this paradigm, but an impf. **ǵn̥-n-*h_3*-me* would have given *kunnum*, just as is assumed for the 1 pl. of the perfect. – 28. For the gen. sg. of the *o*-stems in Balto-Slavic B. rejects Stang's *-o-*h_2 *et.* He proposes *-oio* (for *-osio* after dat. *tomoi* (read *-ōi*?) for *tosmoi*) > *-oo.* Both steps are very uncertain. I prefer Vaillant, Gr. Comp. I 112. – 33. I don't see why *rayíḥ* could not have its *-y-* from the oblique cases. A quite different proposal in my Origins. – 34. On *píbati* see Thurneysen/Kortlandt ap. Beekes, Origins n. 1. – 35. As to the suffix "*-ai̯i̯o-*", it may be asked whether *-e*h_2*-i*h_2*-o-* did not give the actual forms. If one simply 'strikes' the laryngeals, one forgets the chronology. Another suffix *-io-* (after *-e*h_2*i*h_2*-*) seems to me improbable. – 36. That **ǵen*h_1*tis, ǵn̥*h_1*teis* is improbable because *nātio* continues "die reguläre *ti*-Bildung" is circular. (36.6) On *bhū̆-* see Kortlandt, Ériu. (As to p. 133[1]: The examples in Kortlandt, Ériu 32, 1–16, are not his but Dybo's. An evaluation of the material is in preparation.) – 37. On *vā́ta-* etc. see my Origins. – 38. On *pánthāḥ* B. says: "in den starken Kasus zwei Hochstufen, in den schwachen ... zwei Schwundstufen ... Ein solches Bauprinzip widerspricht den Regeln des grundsprachlichen morphologischen Systems" (138 f.). In Origins I try to show that this is exactly 'das grundsprachliche System'. (It is extremely improbable that a nominative ****pā* (I would expect ***pan*) was reshaped after acc. ***pantam* into **pantaH, patH-!*) – 39. The word for "name" I will discuss in Die Sprache (showing $*h_3 nh_3 m$*-* > PGm. **nam-*).

Other sections treat the Verschärfung (19, 20, 26) and Goth. *iddja*; $*th_2$ in Germanic (24, 25; it has the same reflex as **t*) and other questions (*πρίατο,* Gr. *-γα, πρῶτος, facio*, the *ā*-subj., Lith. *žinóti, daviaũ,* and the them. opt.).

On the whole, then, I find this a regrettable book: it proposes improbable alternatives (almost all of them analogical developments) for a few explanations provided by the laryngeal theory, and it shows a number of misconceptions about that theory. There is not one point, to my mind, where B.'s explanations are acceptable, let alone preferable.

Rijnsburgerweg 88
NL-2333 AD Leiden

Robert S. P. Beekes