Peters, Martin: Untersuchungen zur Vertretung der indogermanischen Laryngale im Griechischen. Wien, Verlag der Österreichischen Akademie der Wissenschaften, 1980, gr.-8°, X, 364 S. (ÖAW, Phil.-Hist. Kl., SB. 377 Bd., Veröffentl. d. Komm. f. Linguistik u. Kommunikationsforschung, 8). Brosch. 80 DM.

The book consists of two parts, the development of *HuC-*, *HiC-*, and that of *-CRih*$_2$; it has two appendices and there are three excursions.

The author is very well informed about all recent developments. The result is an extremely learned book, where the subjects are treated in every possible detail.

The subjects treated are difficult ones. In both we are at the limits of what can be demonstrated. This fact may be the main reason why I feel not very happy with the book, notwithstanding the competence of its author. But there may be another reason. Perhaps there is an attitude, somewhat like this: every problem has its solution; so if we consider all possibilities, we must through elimination find the right solution. Such a procedure is allowed, but we should realize that our theoretical considerations may not always include the right solution. It is well known that for many years scholars doubt between two or three solutions, whereas the right one appears to be a quite different one. But even if our theoretical approach includes the correct solution, we may not be able to decide. And even if it is a recognized approach, one might well ask whether it is good to present the whole process of one's thinking to the reader. The result, as in this case, is a very difficult, tiring book. Every time we find "prinzipiell" or "grundsätzlich denkbar wäre", then follow two or three possibilities, each with two or three objections, which are then discussed. The reading is further delayed by many very extensive notes. They are often important, and the author did already defer much material to appendices, nevertheless I feel unhappy with the presentation. (An other objection is also that such notes are hard to find: they deserve a decent article or a separate chapter.)

Then the author uses modern representations of sound laws, which are often very complicated (e.g. 119ff.). I don't see why he did this. As simple statements they are much more difficult than the ones traditional in IE linguistics, and I have not seen any advantage. For in the few cases where he considers the possibility that a simple manipulation of 'rules' would solve a problem, he admits that they don't.

The title is not quite adequate. Only the first part is really a laryngeal problem. Further the book is concerned with a great number of Greek developments. It had better been entitled Studies in Greek Phonology. And the laryngeal problems that are discussed are found in footnotes, where they are treated too briefly.

The book is printed very carefully. I noted no misprints. The index is not complete, which is to be regretted with such a book.

There are two basic assumptions about PIE with which I do not agree. One is the existence of a phoneme *a*. The farther we go, the more it appears that the *a*'s can or must be explained through a laryngeal. It is not wise, then, to reintroduce a phoneme *a*. I know that a few forms still present difficulties, but then we should not expect to solve all problems in one generation. I think it is typical of several younger scholars nowadays not to realize that there will always be a number of problems which we cannot solve, and to force a solution instead of recognizing our limits. If we had not (yet) found the laryngeal theory, imagine how many problems would get a false solution. It should be realized that PIE was a language in which the whole morphological system was built up with the vowels *e* and *o*. It should be realized that the details of the developments of the laryngeals are indeed extremely complicated, and that even in a language so well studied as Greek there are still many details to be solved (see below). It is disguising problems and barring progress to posit *a* where we don't immediately see another solution.

The other point is Benveniste's theory that PIE had no roots beginning with a vowel. P. states (9, 107) that this theory cannot be proven. That is correct, but it could be disproven. I tried to do that (also in my inaugural lecture De wortels van het Indo-europees, Leiden 1974), but I have changed my opinion. The decisive point is that languages that have phonemes like the PIE laryngeals, do not have initial vowels. The argument is comparable to that about the structure of the PIE stop system (*t, d, d^h* etc.). It is possible that PIE was an exception, but it is better to start from the conception that it was not. (Vowels in this respect are *e* and *o*. You could have *i*C-, *u*C- as zero grades of *ie*C, *ue*C-, e.g. **uek^w*-, Gr. ἔπος, **uk^w*-, Skt. *ukthá-*.)

In part one P. tries to demonstrate that *Hu*C- resulted in Gr. *Vu*C-. but *Hi*C- in *i*C-. He tries to establish whether Gr. *Vu*C- derives from *Hu*C- or *HVu*C-, and whether Gr. *u*C- represents PIE *u*C- or *Hu*C-. The first can only be decided on morphological considerations. The second is very difficult to verify, because one needs very special forms to decide it. Given these difficulties, it would have been well to give due weight to the question of phonetic probability. Its discussion is very short (123). The comparison with the Hebrew patah furtivum is no more apt than that with *-ih* > Gr. -ια, but this is of no use, as *h_2i-* did not become Gr. *ai-*. But the author gives a parallel from Spanish Arabic. A. H. Kuipers informs me that such a development is impossible in the North American Indian languages. For the different treatment of *Hu*C- and *Hi*C- P. refers to the non-vocalization of the laryngeal before *i̯-*.

For Gr. *VuC-* he tries to show that *HuC-* is probable in several cases. Thus αὐγή would represent **h₂ougeh₂* or **h₂ugeh₂*, and the first is impossible (but he admits *e*-grade in **h₂uerseh₂* > ἀέρσα 23n). Thus αὔξω would be **h₂ugs-* (notwithstanding Lat. *aux-ilium* and Toch. *oks/auks-*), because of Skt. *úkṣati* and because the *s*-enlargement would belong originally only to the full grade II and the zero grade: **h₂eug, *h₂u(e)g(s)*. Thus αὐχέω based on *-αυχης would have zero grade as this type of noun was hysterokinetic, and because Schwebeablaut **h₂eug-/h₂ueg-* (if Hitt. *huek-* belongs here) should be avoided. εὔληρα would be **h₁ulēr-* (Lat. *lōrum* < **h₁ulōr-*). The difficulty with morphological considerations is that there are almost always exceptions. Thus *ro*-adjectives have as a rule zero grade, but the author accepts full grade for φαιδρός (77); *u*-adjectives as a rule have zero grade, but the author admits Ital. **aisu-* as full grade (83). In general my opinion is that we are not in a position to deny the existence of specific morphological types, and therefore, to draw the conclusions the author does, especially as the phonetic side is also problematic. In some cases the author's conclusions show their weakness. For 'ear' he reconstructs as most probable pre-form **h₃us-* (> Gr. *ous*) on p. 59, but on p. 296 he accepts **h₂eus-*. (I am inclined to accept the possibility that the Greek *o*- was taken from **okʷ-* 'see', but I am not convinced that we are in a position to deny the existence of **h₂ous-*: this is a very archaic word, that may have retained an old inflection; cf. the aberrant *o*-vocalism of πολύς.) He objects to **h₂eus-* 'to pass the night', but his suggestion that **h₂ih₂usi̯ō* developed into ἰαύω (37) is quite unacceptable to me. In general it is my impression that the same case could have been made for *HiC-* > *ViC-*, if only the author would have slightly shifted his evaluation of probabilities.

As to Gr. *uC-*, the author retains the possibility that ὑφαίνω represents **Hueb^h-*, in part because of my interpretation of Myc. *ewepesesomena* as 'that must be woven'. I agree that this interpretation is not certain, but I still think it is very probable as it explains so nicely the problem posed by the combination of *pawea₂* 'cloaks' and the ideogram WOOL. (I don't see why one should posit anything but *h₁*-: Greek is not a language with 'Vokalharmonie', though incidental assimilations are found, and **a(w)eps-* would be quite as acceptable as **a(w)eks-*.)

As to ὕδωρ, Lith. *vanduõ* and Lat. *unda*, which must be archaic (Kortlandt, ZbFL 22/2, 1979, 61), show a root *uend-* beside *ued-*. If we stick to Benveniste's rules, this must be analysed as *Hu-ed-*, *Hu-en-d-*. But it appears that *u-ed-*, *u-en-d-* is also possible, cf. *gʷ-em-*, *gʷ-eh₂-* 'to go', where there was no initial laryngeal (Gr. βαίνω, βη-).

A problem remains ἀυτμή beside ἄετμα. It is probable that it represents **h₂utm-*, but the disyllabicity is surprising also in P.'s view. He suggests that *αὐτμή got disyllabic ἀυ- after ἀε-τμ-, which is quite improb-

able, cf. αὔξω beside ἀέξω. Or that ἀυ- preserved an older phase of the development *h_2ut- > *$h_2əut$- > *$haut$-. But the development *Hu-* > *Vu-* rather supposes that *u* was non-syllabic at the moment the epenthetic vowel originated. ἀυτμή can hardly be a direct development from a PIE form (would *$h_2uotmeh_2$ be possible?). Therefore the idea that the *a-* was added to *υτμη may be correct. Perhaps the word, only found in Homer, is artificial and due to false split under the influence of ἄετμα. If so, it would have *υτμ- < *h_2utm-.

The group ὑπ-(ὑπό, ὑψηλός) has forms with *eup-, oups-*. P. assigns the word the lowest degree of probability, because it might have had *s-* as in Latin *(sub, super)*. But this *s-* is found nowhere else, so I would regard it as almost certain that ὑψ- continues *h_1ups-, which would refute the author's thesis.

The interpretation of εὔληρα, Lat. *lōrum* as *$h_1ulēr$-/$ōr$- could be correct, but it would not prove the author's thesis. The distinction between *u* and *u̯* is post-PIE, PIE had only one phoneme */u/*. Lat. *lōrum* presupposes *$u̯lōr$-, and there is no reason to assume an earlier *[ulōr]*, nor is there reason to suppose that Greek behaved differently. Just as Greek and Latin had *u̯lV-*, not *ulV-*, besides *u̯l̥C-*, they had *Hu̯lV-*, not *HulV-*, besides *Hu̯l̥C-* (e.g. *$h_2u̯l̥k$- > αὐλακ-; *Hu̯l̥Hn-* 'wool', where there is no reason to posit a vocalic *u* in Greek and Latin given the development to λη, *lā*; Hitt. *hulana-* and Ir. *olann* might point to *[HulHn-]*, but these are different languages). So we had PIE */$h_1ulēr$/*, Gr. *[$h_1u̯lēr$]* > εὔληρ-α. But this development may only be assumed before *r* and *l*, not before other consonants: you have *u̯r-, u̯l-*, but never *u̯n-, u̯m-* etc. (perhaps you had *Hu̯i-*). (Development 1969, 64 is aprioristic.)

At the end of his discussion P. invokes the principle of lexical diffusion to explain that the development *HuC-* > *VuC-* did not reach ὑφαίνω (if it had a laryngeal); they would be "vom Lautwandel nicht mehr erfaßte Reliktformen" (117). He stresses that he does not want to deny the regularity of sound laws, but the effect is the same. Suppose that we could definitely prove that ὑφ- continued *Hub^h-*, then the discussion would have ended. If now we could say that this form was not affected by the sound law, we could say that everywhere, the regularity would be gone and arguing would become useless: it would be the end of historical linguistics.

The second part (127–205) discusses the outcome of *-CRih_2*, for which we find *-CRia, -CRa* and *-CaiRa*. The author considers *-CRia* (πότνια) as the regular development. He asks what the relation to Sievers' Law was. He considers the possibility of a sequence: 1. *-ih_2* > *-i̯a*; 2. Sievers, "infolge Regelumordnung". The latter is added, because Sievers would be of PIE date. To my mind 'Regelumordnung' is tantamount to saying that it is impossible. The term 'rule inversion' is asking to accept what has been proved impossible. He states, however, that Sievers requires *$potn̥i̯a$*, as

the essential thing would be that a resonant between consonants becomes syllabic. As this form leads to the wrong result, he leaves Sievers in PIE, considers *-ih₂ > -i̯a* as post-Sievers (which here means that the *n* remained consonantal) and has to accept a Greek *i*-epenthesis. The more interesting point of Sievers' Law, however, is that *i̯* after two consonants is realized as *ii̯*. This is exactly what we have here. Thus the obvious interpretation is: *-ih₂ > -i̯a* followed by *-Tni̯a > -Tnii̯a* (Sievers). This means that Sievers is post-PIE. Thus we do not have to adopt a separate *i*-epenthesis, which is identical to Sievers. (That 'Sievers' is the rule that an interconsonantal resonant is vocalic is a matter of terminology. But that */atywa/* is realized as *[atyuwa]* is not false (for *[atiwa];* n. 77) but probably correct: PIE had a vowel after every two consonants.)

-CaiRa (-αιρα, -αινα) are explained as having -αρ-, -αν- from other nouns, introduced in older **-erih₂, -(e)nih₂*. P. then tries to demonstrate that such stems with -αρ-, -αν- existed and influenced the relevant forms. For -αρ- he can point to forms in -αρος, which require a form in -αρ(-). This must have been an *r/n*-neuter, as there were no other relevant *r*-stems in PIE. This explanation may be correct, but it may not be true in all cases.

For -αινα the situation is much more difficult. The long discussions of μελαν- and κοίρανος are unfruitful. The problem is, of course, that *-n̥-* gives *a* before consonant, and is impossible before vowel. The author did not convince me of the existence of *-an-* in Greek nouns, let alone that this would have influenced **-(e)nih₂*. – I find here, though the author knows the book, no reference to Furnée, Die wichtigsten konsonantischen Erscheinungen des Vorgriechischen, 1972, 171 n. 117, where it is pointed out that -αινα is found in several fish names which are of pre-Greek origin. Of non-Greek origin is also λέων, and how certain is the etymology of δράκων? (Either it is an old *n*-stem and then hardly from *derḱ-* 'to see', or it is an *nt*-stem and then -αινα must be secondary.) And so are names like Λάκαινα. On the other hand μέλαινα from μέλαν- could have been support for a limited productivity of this expressive suffix. (Furnée doubts whether ἄκαινα is IE.)

-CRa is found only in ἄρουρα. The author compares *CRa*: *CaiRa* with αἶσα : νῆσσα, where the absence : presence of a morpheme boundary is assumed to explain the difference.

In general in this section I miss a discussion of the material, which should precede the theorizing. I wonder why a much simpler approach has not been followed. I expect *V-CRih₂ > V-CRi(i̯)a,* but *V-CCRih₂ > [V-CCₑRih₂] > VCCaRi̯a.* ἄρουρα must have a phonetic explanation, too. The development must have been *ourih₂ > /-ouria/ > [-ouri̯a] > -oura,* the absorption due to the nature of the sequence *-u̯r-*, or a dialectal development (see P. 214 on πότνα; see below): it is a poetic word that may be of Aeolic/Mycenaean origin.

App. 1 discusses some forms in -ν(ι)α. πότνα θεά is explained as an Aeolic formula with syncope. However, this syncope is found only after *d* and *r*. The argument that it may have been more widespread, as in Mycenaean, loses its force because Mycenaean always writes *potinija* (which the author too reads (216) */potnia/*).

App. 2 discusses more forms in -αιρα. **isu̯o-g^{h}esrih$_2$* would not lead to -χεαιρα, but to *-χειρα (or *-χειρια). P. suggests an analogical, artificial replacement of *-χειρα at the end of the hexameter. On -κραιρα see below.

Exc. 1 is a comparison of the development of *ti̯* and *ki̯*.

Exc. 2 (οἰωνός, shortening of diphthongs before long vowels, and Attic reversion) defends Schmeja's interpretation of οἰωνός 'bird' as a *no*-formation from ᾠόν 'egg'. The non-Attic occurrences would be Atticisms of the tradition. It contains a long discussion of παρειαί 'cheeks'. Important is the relative chronology (of 15 developments) on p. 303.

Exc. 3 (χερ- and Osthoff's Law) demonstrates that this law operated after the lengthening of the vowel at the loss of *h* (*k^{h}ehr* > **k^{h}ēr*). P. first tries to demonstrate that *V̄R* was shortened before single consonant (in inlaut), not only before two consonants. He adduces as evidence (τριά)κοντα etc. from *-*ḱōmt-h$_2$* (the *-h$_2$* a later addition). This might be right, but better evidence provides ἐνεγκ- < **h$_1$neh$_1$nḱ-*, which first developed into **enēnk-* (as is shown by μην- 'month' < **meh$_1$ns-*, on which see my forthcoming article; the author assumes the same development p. 315, but still writes *eHn̥C* with a vocalic nasal, which only makes things more difficult to understand: the (non)vocalization is a development of the separate languages).

App. 2 on the PIE paradigm of Gr. κάρη 'head'.

After Nussbaum's long article P. devoted another sixty pages to this word family. I think both authors are on the wrong way. The kernel of the problem is simple, and I would like to present my view on it as short as possible, because it is of methodological interest: the authors rely too much on preconceived paradigms.

Both authors assume an *r/n*-paradigm, **ḱrh$_2$s-r̥*, *ḱrh$_2$s-n-*. I think this is wrong, because *r/n*-neuters are not derived from existing stems, and because the evidence for an *r* in the paradigm is too weak: in Greek only a gloss (καράρα· κεφαλή) and outside Greek only Lat. *cerebrum* (which does not require an *r/n*-stem). Also, it would be quite un-understandable why Greek would have given up a quite regular *r/n*-stem. It is a construction (there is nowhere an *r/n*-stem actually attested), which is improbable and which is not based on sufficient evidence.

On the other hand the evidence there is is not given due weight. The oldest Greek and Sanskrit paradigms are:

sg. nom. κάρη			*śíras*	< **ḱrh$_2$-es*
gen. κρᾱ́ατος	< **krās-at-*	< **ḱrh$_2$-s-n̥t-*	*śīrṣṇás*	< **ḱrh$_2$-s-n-és*
pl. nom. κάρηνα	< **karas-n-a*	< **ḱrh$_2$-es-n-h$_2$*	*śīrṣā́*	< **ḱrh$_2$-s-n-h$_2$*

There can be no doubt on the Sanskrit pre-forms, supposed that they directly continue PIE forms, except perhaps the nom. *śīrṣā́*, which is the normal plural in the Rigveda (5x; *-áni* once). The numbers show that *-ā́* is the old ending for this word. Therefore it is evident that *-ā* represents *-aH* < *-nh₂*. Kuiper has shown (Shortening of Final Vowels in the Rigveda, MKNAW NR 18,11, 1955, 28ff.) that *-ā* goes back to **-aH*. He thinks the ending is recent, because Avestan does not have it. But Avestan may have lost this ending. He explained *-ā* as analogical (*vásu*: *vásū* = *nā́ma*: *x*). But this is not necessary, and it is hard to accept that it would have had so much success beside *-āṇi*, which is better marked and which becomes exclusively used soon after the Rigveda. It is easier to assume that *-ā* was old and died out early in Vedic, as it had done in Avestan. The difference between Sanskrit and Greek (-να) is typical: Sanskrit vocalized the nasal, Greek did not (cf. the difference between *CR̥HC* > Skt. *aH* > *ā*, Gr. *naH* > *nā*). The full grade of the suffix in Greek is old, because it is divergent.

Gr. **krās-n̥t-* is an innovation for **krās-n-*, perhaps found in κρανίον, -κρανος. **ḱrh₂es* gives **karas* in Greek. This form is found in κάρηνα. **ḱrh₂es* is found in Sanskrit in the nom. sg., and as Greek has there καρ-, as in the plural, it is evident that the nom. sg. in Greek is based on **karas* < **ḱrh₂es*. There is no reason to posit **ḱrh₂s* in the nom. sg., as P. does. (The development of accentuated **ḱrh₂s* to **karas* is most uncertain. In any case this word cannot be used as evidence, as the comparative evidence points to **ḱrh₂es*.)

κάρη can be explained, as has been seen long ago, by assuming an analogical nom. **karas-n̥* > **karaha*. P. (277–80) has shown that there is no difficulty with *-aha* > -η. He has also refuted the objections against such an analogical form (278). (For a new nom. compare also κάρηαρ on the basis of καρήατα.) Nevertheless he rejects this nominative (280), because of Myc. nom. sg. *qoukara* (not **-karaa*, which one expects from **karasn̥*, as opposed to *-karaapi*/*-krāaphi*/), and because of Ionic forms with **krāh-*. The objections are not decisive. It has been shown that *-h-* had already disappeared between identical vowels in Mycenaean (hence *-karaapi*, not **-karaa₂pi* with *a₂* = *ha*). It is evident that after the loss of the *h* the two *a*'s cannot have remained uncontracted for long. It seems quite possible that contraction occurred earlier in the nominative. First, here the contraction is between the second and third syllables, whereas in **krā(h)a-* it is between the first and second syllables. Also there may have been a difference between long vowel + short one and two short ones. And there may be another reason which we don't (yet) see. One must dare to jump from stone to stone, even if not every detail can be accounted for yet. As to P.'s second objection, the forms **krāh-* (ναύκρατος, κρανίον) they do not show that the nominative had **krāh-* (if I understand him correctly). The form **krāhar*, which he posits to explain -κραιρα (and which may be correct), is shown to be an innovation by its ablaut, because both κάρη and *śíras* show that the nominative did not have **krāh-*.

P. assumes (with others) a form **ḱreh₂* in the nominative. The evidence for such a form is extremely unreliable, and even if it existed, it is by no means evident that this form, rather than (a reshaped) **karas* < **ḱrh₂es*, was introduced as nom. sg. into the paradigm. Thirdly **ḱreh₂* does not give **καρᾱ* but **κρᾱ*. To solve this difficulty either a form **ḱr̥eh₂* is proposed (280), which is an arbitrary assumption of a form which contradicts PIE phonology (Lindeman's observations ['Law'] do not prove that we have the right to posit such forms for PIE ad libitum) or a contamination with καρ- from other forms, which is another auxiliary hypothesis. It should be noted that the agreement in ablaut between the nominative singular and plural is certainly old. The PIE paradigm, then, was clearly:

sg. nom.	*ḱrh$_2$-és	śíras, (κάρη)
gen.	*ḱrh$_2$-s-n-és	śīr̥ṇás, (κρᾱν-ίον, κρᾱατ-)
pl. nom.	*ḱrh$_2$-és-n-h$_2$	κάρηνα, (śīr̥ṣā́)

This paradigm does not conform to the types we have established until now. In this respect it is liable to the same objection as *ḱrh$_2$s-r, ḱrh$_2$s-n-. But, as opposed to the latter (re)construction, this paradigm is actually found in Sanskrit (where it cannot be explained as an innovation) and it can explain the Greek facts without much difficulty. This combined, comparative evidence is sufficient to establish an unusual paradigm: the facts ultimately have priority.

As *es/s* is not determined by the *n*, the ablaut must be old. This means that the word was hysterodynamic. In this respect it is important to note that the nominative does not have *-os* but *-es*, as appears from *καρας in κάρη (*ḱrh$_2$os* would have given *καρος, which would have resulted in *καρω from *καροσ-α). The normal neuter *s*-stems are proterodynamic. The fact that this word alone had an aberrant inflection may be the reason why an *n* was added in the oblique cases.

I don't go into all the cognate forms. I only repeat the warning that they should not be used too soon as evidence for the oldest inflection of the basic word. And I would like to add that we should not imagine that we are able to retrace every detail.

Laryngeal problems: As the title of the book says that it deals with laryngeal developments, I may be excused for giving a list of the more important notes on laryngeals found scattered in the book.

1. *Hi̯-*. From *Hi̯-* P. expects *h/Ø-*, but in Add. ad p. 3 he admits the possibility of *h$_3$i̯-* giving ζ-. This is probably based on the supposed voicing in **pibe-* 'to drink' from *-ph$_3$-*. This voicing, however, is very doubtful, as there is nothing else to confirm it. Kortlandt (priv. comm.) came to the conclusion that **pibe-* results from **bibe-*, an initial *b-* not being tolerated in PIE. He found that this solution had already been proposed by Thurneysen, see IFAnz. 22,65.

2. *-(R̥)Hi̯-*. Before *i̯* the laryngeal disappeared without trace. (But μέζων is no evidence: the comparative suffix was added to the root, **meǵ-*, not to the stem, **meǵh$_2$-*.)

This will also explain the development of *-R̥Hi̯-*, for which P. assumes *-aRi̯-* (80 n. 38). Evidence is not very strong. καινός is not necessarily identical with the stem of Skt. *kanyā́* 'girl' (if **konHi-Hon-*). θαιρός from **d^{h}u̯r̥Hi̯o-* (323 ad 81) presents the problem of *d^{h}u̯-* (Ruijgh, Lingua 27, 1971, 272). For αἴνομαι there is no evidence for *-H-*. Reliable seems (ἀ)σπαίρω < **spr̥hi̯e-*. I would suppose that the *i̯* was so much vocalic (cf. Lat. *medius*, OIr. *-e* < **ios*), that in *-RHiV-* the laryngeal was lost just as before vowel. In IIJ 18, 1976, 90 I demonstrated that in Sanskrit the *H* was not vocalized before the comparative ending. (It is possible that *varī-* was taken over from other forms, like *várīman-*.) *sakhyá-* will continue **sok^{w}H(i)-iHo-* or a later formation with *-iHa-*. So I disagree with the author on this point. Lith. *kraũjas* seems to confirm that the *H* was lost early, already in PIE? (*H* > *ə* with lengthening of the vowel is not the correct explanation of the Lithuanian acute: it is the glottalic feature which caused it directly, see Kortlandt, Baltistica 13/2, 1977, 319–25 and KZ 92, 1978, 277–80.)

3. *HCL̥-*. The author holds (23 n. 18) that *HCL̥-* (*L* = *r, l*) developed no prothetic vowel in Greek, e.g. **h$_2$u̯r̥d-* > ῥαδ-. He found six cases. He gives a nice explanation of βλαδύς: ἀμαλδύνω. The first is regular from **h$_2$ml̥d-*, the second is secondary after *ἀμελδ-. (*HCN̥-* however would have prothetic vowel: ἐλαχύς < **h$_1$ln̥g^{wh}us*, but here there is not much evidence.) He explains this development as dissimilatory loss of the first anaptyctic vowel in *H$_e$CL$_e$-* or *H$_e$C$_e$L-*. This

explanation seems not probable to me, as nothing comparable is known from Greek. Therefore I doubt the rule. Also the total picture the author gives, that *Hul-* got a prothetic vowel whereas *Hu̯l̥-* did not, is improbable. Note that five out of six cases have *Hu̯-*; and that the development we expect would be *Hu̯R̥-* > *Vu̯Ra-*. Two forms are problematic. ἄρσην < **h₂u̯r̥sēn* is not a certain etymology; we would expect **F*ρασην; and the connection with ἔρσην is given up (to Hitt. *ars-* 10 n. 4), which is hardly acceptable. (P. states (9) that Homer points to a wau, but seven out of eleven do not tolerate a wau, and only ι 438 could have one, but this is a formula which may have been used after vowel after the bucolic caesura, as often happens.) Further it is hard to believe that the prothetic vowel was analogical, e.g. in ἤμβροτον (after ἀμερτ-). In ἁλής, ἀολλής the α- is a fixed part of the root, found in all ablaut forms; therefore it does not contain α-copulativum (see Chantraine, Dict. étym.), but a laryngeal. Then it shows **h₂u̯l̥n-* > **α*Fαλν-, **α*Foλν-and perhaps also ἀ*F*λανεως. The α- can hardly have been taken over from the full grade ἀελλής, which occurs only Γ 13 (though there is no reason to consider this form as artificial). P. assumes that **h₂n̥Htih₂* > νῆσσα is regular, parallel to *HCL̥-*. Here one might think of dissimilatory loss of the first laryngeal (as in 'wool', **h₂u̯l̥Hn-*).

4. *R̥HV*. P. thinks (27 n. 19) that *R̥h₁V*, *R̥h₃V* became both *aRV*, not *eRV*, *oRV* as I held, following Kuiper. He argues that ἔτεμον etc. can be explained from an athematic aorist, and ἔπορον etc. also, **eperh₃-t* > **epero-t* giving ἔπορε through metathesis (well known from *lewo-* > λοε-). His positive evidence is then ἔταμον < **etm̥h₁-* (from the plural) and φαρέτρα < **bʰr̥h₁tréh₂* (with *r̥h₁* giving αρε under the accent), and πεπαρεῖν < **pepr̥h₃-* (from the same root as ἔπορον) and βάραθρον, ζέρεθρον from *βαρεθρον < **gʷr̥h₃edʰro-* (*gʷrh₃-* 'to devore').

I am not convinced by these arguments. In an original paradigm τάμνω, aor. sg. τεμ-, pl. ταμ- the -ε- could hardly have conquered the whole system (as in Att. τέμνω, ἔτεμον). In an aorist sg. **eperot* > *epore*, pl. *epar-* one would expect that the *a*-vocalism was generalized, but there is no trace of it. The explanation with laryngeal 'umlaut' remains the most simple. The interpretation of φαρέτρα is not convincing (I do not accept *R̥HC* > *VRVC*, see below). See Chantraine, Dict. étym., where it is suggested that it is of foreign origin. πεπαρεῖν must not have the root **prh₃-*. (*peprh₃-* would have lost its laryngeal, giving *πεπρ-, cf. ἔτετμε < **etetmh₁et*, and γίγνομαι). The connection of βάραθρον with βιβρώσκω is not at all evident. *βεραθρον < **gʷerh₂-dhro* is quite possible. I argued (Development 1969, 193) for foreign origin (cf. σπῆλυγξ). Note that, if εὐρύς comes from **F*ερυ- (53), and this form from **u(e)rHu-*, it could be **u̯r̥h₁u-* > **F*ερυ-. More convincing seems to me καλε- 'to call', which could now be explained as **kl̥h₁-e* (I do not believe in the type **kl̥eh₁-*).

If the colouring by the laryngeals becomes doubtful, we must perhaps reconsider the development of *HR̥C-* also: as ἔρσην/ἄρσην requires an initial laryngeal, it can only be explained as **h₁ers-/h₁rs-* > ἀρσ-.

5. *R̥HC*. P. holds that accentuated *R̥HC* became *VRVC*, not *RV̄C* (2 n. 1 ἄνεμος, 29 n. 19 φαρέτρα, 245 **kr̥h₂sr*, 317 ἀνάεδνον). There is no systematic treatment, and the evidence given is not convincing.

6. *h₂o*. P. supports (1 n. 1) the interpretation that *h₂* did not colour *-o-* in PIE. For the Greek forms with *a* see now Kortlandt, Lingua Posn. 23, 1980, 127 (*h₂o* > *h₃o* in PIE, but when *h₂* was restored in Greek, it did change *o* to *a*.).

7. *HRo, oRH*. The author also holds that a laryngeal disappeared before *Ro* or after *oR* (3; 61 n. 30). I accepted it in Development 1969, 74–6, but I am very sceptic now, as I cannot see what phonetic cause it could have had.

8. h_1s-. *h_1su- in ὑγιής would have got no prothetic vowel, because it is a compound with accent on the final syllable (208 n. 160).

9. *mehur*. P. supports Eichner's *mehur*-law: 61 n. 30 (very doubtful) and 314: *ǵnē-/ǵnō-* from *ǵnēh₃/ǵneh₃* in an akrostatic (protostatic) root aorist (but there we would expect the lengthened grade generalized, as in the *s*-aorist, so that *ǵnō-*, which is the better attested form, could not originate from the aorist).

10. *R̥HV* would give *aRV* in Latin: **mn̥h₁-ē-* > *manēre* (188 n. 143). It should be noted that this requires a kind of laryngeal umlaut, the normal development of *n̥* being *en*.

Hu̯- in Armenian is discussed in a long note p. 40 f.

I have been rather critical. Therefore let me repeat that the book contains numerous important discussions of phonological and morphological problems of the history of Greek, of which the relative chronology is perhaps the most important contribution, and that the author is extremely well informed and that he has a sound judgment. Without a doubt he will prove a scholar of great importance, and we welcome him into our field of research.

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