

R. Anttila, *Proto-Indo-European Schwebeablaut* (= *University of California Publications in Linguistics*, 58). Berkeley/Los Angeles, University of California Press, 1969. xviii + 238 pp.

The author of this book (which is based on a dissertation written under supervision of Cowgill) shows a profound knowledge of the literature on his subject, and makes high demands on his method; the book is, indeed, a serious contribution to the discussion on its subject. Nevertheless I must add immediately that I am not convinced by the main conclusion of the book, and that I have many problems with details.

The structure of the book is as follows (my captions): Preliminaries, 51 pp. (History, 1-23; principles, semantic, 24-9, formal 30-5, segmentation, 36-51), material, 106 pp. (Indo-Iranian, 52-66, Greek disyllabic roots, 67-87, other, 88-157), evaluation, 15 pp. (Benveniste, 158-62, analogy, 163-72); summary (173-6); appendix on Kartvelian (177f.); bibliography, indexes.

A few words may first be said about the purpose of the book. The author himself is not wholly clear about it; after giving a new theory he says on p. 173: "It turns out that there is no need to link schwebeablaut with a new theory" and on p. 174: "Currently ... handbooks use schwebeablaut as a solid fact of the protolanguage, or treat it otherwise inadequately." From this quotation one would expect that now schwebeablaut has been eliminated, but it has not. A new investigation of the material is of course useful, but it is hardly to be expected that all supposed instances would appear wrong. One might say that the conclusion concerns *Pre-PIE*: schwebeablaut has been demonstrated to occur in a number of roots of PIE; here one could stop. Anttila, however, gives a historical explanation for the phenomenon: State I would be analogical; see below.

The historical survey is very useful. As Anttila states, "in this century scholars generally do not refer back beyond Benveniste or Kuryłowicz, who in their turn do not accredit earlier work either" (p. 19).

The semantic requirements are not specific for schwebeablaut, of course. The author states that he will use strict semantic principles, which he does and in which he is right; that he will discard onomatopoeic words (note *πέδρομαι* — *Οἶκ. freta*, where there is no formal evidence of abnormal treatment), and words liable to tabuistic remodelling (*γυμνός*: *naked*; but *οἶφω* — *yábhati*) should be retained: there is nothing against *\*(h<sub>3</sub>eibh-* or *\*(h)oi**h**-/\*(h)ie**h**-*).

In chapter III ("Formal requirements") it is pointed out that the zero grade is independent from the full grade, i.e. that there is no zero grade schwebeablaut ( ${}_eR : R_e$ ), so that on a zero grade only one cannot distinguish between state one or two in the full grade. This is right, but I do not agree with the statement (p. 30) that "many treatments with reduced vowels or anaptyctic vowels within the zero grade must be disqualified for schwebeablaut." Of course, anaptyctic, i.e. secondary, vowels are not relevant for PIE, but reduced vowels are. The point is that Anttila does not recognize reduced grades: "There are no good reasons for a reduced grade" (p. 15; litt.). I think we cannot do without them. In two cases I tried to demonstrate that we must accept them, *Dev.*<sup>1</sup> 206-9 (Gr. *aRa* <  ${}_eRH$ ) and pp. 235-7 s.vv. *καλέω*, *ἄνυμι* (*τανύω* from  $*t_e neu-$  etc.). The importance here is that to my mind  $C_eC(C)$  allows the reconstruction of a full grade  $C_eC(C)$ .

In Chapter IV ("Principles of segmentation") it is stated that nasal presents will be excluded from the discussion. This may be wise. (In 4.2.3, however, "infixed resonants other than the present infix" are introduced. What is meant here, are forms that have been shown to be the subjunctives of the nasal presents, type OHG *springan* from  $*spr-en-gh-$  (Kuiper, *Nasalpräsentia*, 175f.). This, more than anything else, determines the segmentation of the indicative as  $*li-n-ek-$ , not  $*li-ne-k-$ , which is preferred by Anttila, p. 39). The thematic vowel is regarded as a separate morpheme, not as part of the root (4.5). A (two consonant) vowel jump rule is formulated in 4.6: "When the vowel jumps over two consonants, it indicates that there is a morpheme boundary in between." This rule is for practical use. "It follows that alternation that one can call schwebeablaut occurs only within the interior of three consonant sequences  $C_eCC : CC_eC$ ." Of course, the rule and the conclusion state the same factual situation. Rightly Anttila adds that the first consonant may be absent. As for his example (pp. 50 and 41), OIc. *okkvinn* <  $*eng^-o-$ , Lat. *inguen*, *ἰδῆν* <  $*ng^-ēn$ , Rix has demonstrated (*MSS*, 27, 1970, 102) that this form cannot have had a laryngeal:  $*eng^-$  could only have had  $h_1$ , but  $*h_1ng^-$  would have given  $*ēδ-$  in Greek (other indications *Dev.* 91). With *s* movable one has  $sC_eCC$  etc.

We come to the material. Anttila states (p. 170) that only one fourth of the material mentioned in the literature survived the screening. In my opinion Anttila is often too critical. Often evidence is rejected because it is based only on one or two languages, but it is clear that a language may be the only one to have preserved PIE facts. He states (p. 173) that "the investigation was undertaken with an open program." Of course, we have no right to doubt the author's sincerity, but the impression one gets is that the final conclusion (State II is old, I is secondary, i.e. schwebeablaut is a limited phenomenon, to be explained as recent) has induced the author to reduce the evidence as much as possible.

Chapter V ("Indo-Iranian evidence"). Future forms of the type  $CRaCs_yati$  from  $CaRC$  roots are due to analogy (the term metathesis, p. 66, should therefore be avoided). Many roots are discussed in which Sanskrit innovations are assumed; I will discuss them (together with the evidence from the other languages) in chapters 8 and 9.

Chapter VI deals with the Greek disyllabic roots, which I discussed in *Dev.* 186-253. As regards the question whether or not the three laryngeals remained distinct in Greek Anttila takes no position. In fact, however, he argues as if they were distinct (as I think), to some extent because he must do so: if he does not assume  $nh_1 > \nu\eta$ , he must for  $-\gamma\nu\eta\tau\omicron\varsigma$  assume a state II, which might unnecessarily suggest schwebeablaut. It is pointed out in this chapter that  $R\bar{V}$  might represent zero grade (which is of no interest for schwebeablaut) and mostly does so.

A result of the fact that no reduced grade is allowed is that assimilation is called

<sup>1</sup> *Dev.*: R. S. P. Beekes, *The Development of the PIE Laryngeals in Greek* (The Hague, 1969).

in on a large scale (p. 68f.):  $CeRa > CeRe$  or  $CaRa$ , etc. He thus arrives at two patterns (p. 69):

I	$eRa/R\bar{a}$ $eRe/R\bar{e}$ $eRo/R\bar{o}$	II	$aRa/R\bar{a}$ $eRe/R\bar{e}$ $oRo/R\bar{o}$
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I don't see why there are *two* patterns. As  $aRa$  is due to assimilation in  $eRa$ , we have  $eRa(>)aRa/R\bar{a}$  etc. Moreover, if we indeed allow  $eRa > eRe$  (above, p. 68), we should have  $eRa/aRa/eRe/R\bar{a}$ , and if  $oRa$  would likewise give  $oRo$  and  $aRa$ , we would even have  $eRa/aRa/eRe/oRa/oRo/R\bar{a}$  (and why not add  $R\bar{e}$  and  $R\bar{o}$ , then?). This amounts to complete chaos, but the actual material does not point in that direction. We must admit assimilation in Greek, also with the outcomes of laryngeals ( $\acute{\epsilon}\rho\epsilon\varphi$ -/ $\acute{\omicron}\rho\omicron\varphi$ - must represent  $*h\acute{r}ebh$ -/ $h\acute{r}obh$ - with either  $\acute{h}_1$  or  $\acute{h}_3$ ), but it did not seem necessary to assume it on a large scale. Anttila often refers to 'Pattern I/II' to 'solve' difficulties, and this gives a method of explaining away States I (analogic on  $R\bar{a}$ ,  $R\bar{e}$  or  $R\bar{o}$ ). I found little or no reason to assume such analogy. (Analogy is widespread, of course, but this does not allow us to discredit given forms arbitrarily as analogical. Methodically, I think, we may assume analogy only when we have serious arguments for it. Otherwise we would lose all certainty.) For zero grade before vowel and the reduced grade the following are the Greek representations, to my mind:

$R\acute{h}_1-V > eR$	${}_eR\acute{h}_1 > aRe$ or $eRe$ not decided
$R\acute{h}_2-V > aR$	${}_eR\acute{h}_2 > aRa$
$R\acute{h}_3-V > oR$	${}_eR\acute{h}_3$ no evidence ( $aRo$ or $oRo$ expected).

Chapter VII deals with the evidence for schwebeablaut within each group separately, as far as not discussed in V (Indo-Iranian) and VI (Greek disyllabic roots). In 7.2.2 the Greek prothetic vowel is made much more mysterious than it is. In *Dev.* 18-97, table p. 68f., I counted 63 good cases and 33 less sure ones; that is enough to definitely establish Kuryłowicz's explanation of the phenomenon.

For Germanic (pp. 90-4) there is abundant material, but there is also evidence for metathesis to some extent. Much schwebeablaut evidence is Germanic only. Suspicion is aroused by the remark (p. 92): "The evidence from the zero grade is important, because it makes PIE schwebeablaut unnecessary." If this means that always a full grade might be secondary on the basis of the zero grade, this is true, but it offers the possibility of denying schwebeablaut anywhere (and the formulation suggests that the author is going to do that).

In Balto-Slavic there seem to have been many analogical creations. Here, too, much of the evidence finds no support elsewhere. To my mind the Germanic and Balto-Slavic material requires a separate detailed treatment. The other groups, when taken by themselves, contribute virtually nothing.

Chapters VIII and IX give the cross-PIE evidence. The division between VIII and IX is not altogether clear to me. The evidence discussed in IX is said to be better, that in VIII to be less good, but in VIII there are cases that survive the screening and in IX some that do not. (Captions are those of Pokorny, but the author tacitly reinterprets them, which is not always easy; e.g. p. 137, 9.32,  $*g^wey$ - 'to live' opens with "The only evidence for state two"). I shall discuss the material after reviewing chapters X and XI.

In Chapters X and XI the material would not seem to be thoroughly evaluated. The reader is confronted with a theory, which is not demonstrated on the basis of all the facts, but illustrated with some facts. The reader has to check it for himself. In general, the discussion is not clear and not penetrating enough.

Anttila agrees that Benveniste's analysis is valid (for the type  $*doru$   $*dreus$ ), but "it must be confined to the cases presented here" (p. 161), i.e. in § 10. And on p. 163

he concludes "But the three consonant sequences in the material not yet discussed cannot be so easily segmented. It will be evident that this lack of susceptibility to segmentation does in fact correspond to a different natural explanation." To my mind this is not evident. There are two things. One is that it is possible that PIE in its last stage had *CCeC* (or *CeCC*) roots beside which the original *CeC* form did not any longer exist. This means that Benveniste's assumption that all roots ultimately derive from a two consonant sequence may be true (only) for an older phase of PIE. The second point is that *we* may not be able, owing to a lack of material, to demonstrate the existence of the forms, and the correctness of the analysis given. I see, then, no need for another hypothesis. (Anttila states that, if this analysis means that two morphemes are combined so that only one may have full grade, *schwebeab-laut* is reduced to normal ablaut. I think this is indeed the case. Nevertheless the term may have some practical usefulness).

Anttila's theory is that state one is analogical, or at least secondary. It is based on Kuryłowicz (*Apophonie*, 147-50; now also *Idg. Gramm.*, II, 221, 301-4). The basic assumption is that derivation from root nouns was based on the zero grade of the oblique stem with the introduction of a full grade, e.g. *\*luk-* gives *\*leuk-o-*. Nobody will deny the possibility that on the zero grade *-RT* a full grade *-eRT* could be built, but Kuryłowicz then rashly states: "Wenn aber auch in Einzelfällen, ja sogar in der Mehrzahl der Fälle, das Nebeneinander (...) auf einer Neuerung (...) beruht" (*Idg. Gr.*, II, 221). To my mind there is no evidence for this assertion, which in one line in passing makes an exception to the rule. It is typical of Kuryłowicz, who gives much theory and little demonstration of it and who often presents his theories as facts. In general it must be said that, if such a type of derivation existed, it must have had a basis, and the cases Anttila tries to explain in this way may rather be that very starting-point (for a few — other — cases), of which the origin can be easily explained with Benveniste's analysis. E.g. of the forms *\*dieu-*, *\*diu-*, *\*deiu-o-*, the last is explained by Anttila as a case of the above process, *\*diu- → \*deiu-o-*. This is to my mind an unexplained process, and an unproven hypothesis. *\*Deiuo-* may as well (and preferably, I think) be supposed to be derived from *\*dei-ey-o-* with zero grade in the element preceding the suffix. The problem we arrive at here is how ablaut originated. Let us state frankly that we know nothing about it. (An essential point in Anttila's considerations is that thematic forms must be recent. Of course there are many recent thematic forms, and perhaps most are recent, but the thematic type as such (or the thematic vowel, however this is defined) must rather be very old, were it only for the fact that one could not explain where it can have originated. E.g. Anttila says of Kuiper's (State I) thematic subjunctives that they are "another late thematic formation." I doubt that there is any indication that this form category *as such* is late. Of course its secondary use as indicative is late, but that is another matter).

Anttila construes the following opposition (p. 171), which would confirm the difference between 'Benveniste roots' and the other category:

<i>*dor-u</i>	<i>*dr-eu-</i> ,	deriv.	<i>*dreu-o-</i>
<i>*dieu-</i>	<i>*diu-</i> ,	deriv.	<i>*deiu-o-</i>

This would be an important observation, but (1) it does not prove that *\*deiu-* is in any sense secondary; (2) the scheme is misleading in that it is incomplete. The *\*dreu-o-* derivation is from the oblique stem, and this occurs also in the hysterodynamic type, i.e. from the zero grade. And the *\*deiu-o-* type also occurs in the proterodynamic type, as appears from *\*h<sub>2</sub>eiu-o-*, Lat. *aevum*, Goth. *aiws* beside *\*h<sub>2</sub>oiu* *\*h<sub>1</sub>ieu(-s)*.

I do not see, then, that Anttila has proved his theory. On the contrary, while he rejects the existing explanation, his own theory asks itself for an explanation.

Methodically, therefore, Benveniste's hypothesis must be preferred, and Anttila has not put forward any serious argument against it.

As regards the discussion of the material, I have many objections (which does not mean that it does not contain many useful remarks). I must limit myself to a few cases, taken from the list on p. 175f. (I must say that this final list is not very clear; it contains items that cannot even have schwebeablaut, as \**Aek-/Ak-ē*).

\**Aenk-/Anek-* is an extremely doubtful conglomerate that should not have survived the screening. Let me remark only that νεκρός should not be connected with forms that have prothetic vowel (ἐνεκ-, ἀνάγκη ?). Reconstructions \**Anénk-ā*, \**ana-Ank-ā* and \**an-ane-k-ā* for ἀνάγκη are all pure speculation. In such words the oldest meaning should first be established. Note Chantraine, *Dict. étym.*: "Aucune des étymologies (including ἐνεγκεῖν, OIr. *ēcen*, Hitt. *henkan*, Lat. *nex*) ne rend compte du sens propre [established by H. Schrekenberg, *Ananke*, Munich, 1964] de ἀνάγκη et des ses dérivés: 'contrainte' et d'autre part 'parenté'. La notion qui pourrait justifier ce double développement sémantique serait celle de lien." Chantraine prefers Schwyzler's ὄν-αγκ- 'prendre dans les bras'; this kind of etymologies would seem to belong to the past. If it is not a compound and not reduplicated, it cannot be analyzed into root and suffix (-*k*, -*nk*?), nor is a PIE root -(*e*)*n*(*e*)*n-* probable. On account of its double *a*-vocalism it is likely to be of non-IE origin.

\**Aeus-/Aues-*. Anttila rightly accepts Kiparsky's explanation (*Lg*, 43 (1967), 624-6) of ἠώς as \**ausōs* > \**auhōs* > \**ahūōs* > \**hāūōs* (as against \**ahūōs* > \**amūōs* in Aeolic). This points to a root \**h<sub>2</sub>eus-*. However, among the words with State II connected by Anttila is ἔαπ 'spring', which cannot represent \**h<sub>2</sub>ues-ŕ*. The two groups cannot be cognate; this is not necessary semantically either ('spring': 'morning'). It appears that all words with State I mean 'dawn' (ἠώς, *aurora*, Lith. *aušrà*, OE *ēastre*<sup>2</sup> etc.) and that the words for 'spring' have only the shape \**ues-* (Skt. *vasantá*-<sup>3</sup>, Av. *vayri*, Arm. *garun*, ἔαπ, OCS *vesna*, Lith. *vāsara* 'summer', Lat. *vēr* (?), OIc. *vār* (?)). There are two words for 'dawn' with State II: Ved. *vāsarā*- 'morgendlich leuchtend' (the interpretation of *vasarhán-* is not sure), and Celtic words, e.g. Welsh *gwawr* (< \**uōs-rī*). Zero grade also is known only from 'dawn' (Skt. *uśás-* *usrá*-). This situation is understandable: as 'spring' has no initial laryngeal, its root is \**ues-* (beside which \**eus-* would be possible, but it is not found). We have then:

1. \**ues-* 'spring' (ἔαπ, Skt. *vasantá*- etc.);
2. \**h<sub>2</sub>eus-/h<sub>2</sub>us-/h<sub>2</sub>ues-* 'dawn' (*aurora*, ἠώς | *uśás-/vāsarā*-).

Skt. *ucchāti*, Lith. *aušti* 'dämmern' probably belong to root 2, but they cannot be connected with Hitt. *au(s)-*, because this does not have *h-* (< *h<sub>2</sub>*).

\**dleEgh-*. Anttila's treatment of Greek is unacceptable: "ἐνδελχιής does in fact represent the normal replacement of the expected \**dlē-*", and δολιχός, that stands for \**δολοχο*, is "more plausibly" derived from \**dloEghos* > \**δλωχο* > \**δολοχο*. This means that the (double) evidence of Greek is ignored. There is no evidence that *RV* was ever replaced by *VRV* in Greek.

<sup>2</sup> Knobloch, *Die Sprache*, 5 (1959), 27-45 has pointed out that there is hardly any evidence for a goddess of 'Spring', and that OE *ēastren*, OHG *ōstarūn* 'Easter, Pascha' was derived from a word for 'dawn', not for 'spring'.

<sup>3</sup> Cf. Renou, *EtVedP.*, III, 4 n. 2 on this and other forms. He thinks that the meaning 'spring' of *vasantá-* developed from 'dawn': "le lien entre les deux acceptions est démontré par l'expression *uśásah śaradaś ca* (RV) 4.19.09 'printemps et automnes'." However, in the article cited in the previous note, p. 36 n. 48, he prefers an interpretation 'pour de nombreux jours et (de nombreuses) années'. That there was a secondary development 'dawn' > 'spring' (not in the other direction) seems a priori possible, the more so if there existed originally two different words, of which the roots became identical, *vas-*.

\**kreA-*. “The Greek alternation *κερα-/κρᾶ-* finds support only for its state two” (in Skt. *śrāyati*). “In this situation we must resort to pattern I ... as the origin of *κερα-*, rather than assign it to PIE.” This is unacceptable. Anttila forgets Ved. *á-sírta-*, and one could as well say: as *κρᾶ-* is probably zero grade, only Sanskrit zero grade finds support elsewhere, so that Skt. *śrā-* must be analogical. In this way both full grades can be ignored.

\**kyeA-* (Skt. *sávas-/śvātrá-*, *πᾶμα*). Anttila considers the Sanskrit nominal forms to be secondary formations based upon *śū-*. Even if Celtic would point to *\*kawar-* (< *\*k<sub>e</sub>h<sub>2</sub>-r-*), this “would not add essentially to schwebeablaut, except for chronology, i.e., the ablaut would be of PIE date rather than an Indo-Iranian ... innovation.” Here it appears that the author starts from the preconceived idea that the form must be secondary, even if it dates back to PIE. In that case, however, there would be no indication that it was secondary. (I see no reason why the Sanskrit forms should be secondary).

\**peIE-/pleE-* ‘to fill’. Anttila is not right in rejecting the evidence of Skt. *páriṇas-*, Av. *paranah-vant-* and Skt. *páriman-*; the structure of *\*pelh<sub>1</sub>-n-os* is paralleled by *dráviṇas-*, Av. *draonah-* and *τέμενος* < *\*temh<sub>1</sub>-n-os*.

\**ters-/tres-*. Anttila thinks early metathesis more likely than original schwebeablaut, because of the meaning. Though metathesis is not impossible, there is no evidence for it. In general we should not discard a word as onomatopoeic, unless there are formal aberrations that prove that the word cannot be explained in a normal way. Otherwise we could do away with all forms we consider difficult.

\**weg<sup>w</sup>h-*. Anttila denies the antiquity of *εὔχομαι*, Av. *aogadā* etc., but Johanna Narten has now shown that these forms are old, *Pratidānam*, 9-19.

I add a few forms that are not found in the list of p. 175f., but which I consider to be instances of schwebeablaut. (I add the paragraph where Anttila discusses it).

(8.8) *\*h<sub>2</sub>eut-/h<sub>2</sub>yet-* ‘blow > understand’, Av. *aotāi*/Skt. *vátati*. Anttila’s arguments to dismiss the group in fact confirm it (p. 57): Thieme gave the semantic basis, while Bailey showed that Ossetic confirms Avestan. Greek *ἀετμα ἀετμόν ἀετμή* (*\*h<sub>2</sub>yetm-*) establishes *h<sub>2</sub>*.

(9.68) *\*yerg-/yreg-* ‘work, do’. State I is sure (*ἔργον*, Av. *varəzam*, OHG *werc*). It is stated that *ἔρρεξα* is “obviously secondary”, but without one argument, and it could hardly be explained without *ρέζω*. Then Celtic words with *\*yreg-* are presented, but “British alone is hardly cogent for PIE.” But British is not alone.

(9.19) *\*(h<sub>3</sub>)embh-/(h<sub>3</sub>)nebh-* ‘navel’. Anttila follows Szemerényi (*Syncope*, 238-49) and posits *\*nebh-/ṅbh-*, as “ready made words” would not have schwebeablaut. But there is no evidence that all forms belonged to one paradigm. On the contrary, there is evidence for a root noun, an *n-* and perhaps an *l-*stem. Skt. *nābhi-* is an *i-*stem replacing a root noun; the root noun explains the long vowel, *\*(h)ñōbh-s*.<sup>4</sup> Lat. *umbo* points to an *n-*stem; so do OHG *amban* ‘Wanst’, OS *ambon* ‘abdomina’, but Anttila separates them, because of their meaning; I think the semantic development is not too difficult. For an *n-*stem we expect full grade in the nominative and *\*h<sub>3</sub>émbh-ōn* (or *\*(h)ómbh-ōn*) is well possible. Lat. *umbo* cannot be explained by a zero grade *\*ṅbh-*, which would give *emb-*, not *umb-*. *Hemo, homo* probably has two different vowels retained (thus Ernout-Meillet), and is not so close a parallel as *imber* < *\*embhro-* < *\*ṅbhro-* (thus Anttila p. 143). The development *ṅbh-* > *umb-* supposed by Szemerényi is simply wrong. Latin, then, has *\*h<sub>3</sub>embh-* or *\*(h)ombh-*, and the German forms confirm this. Rix, *MSS*, 27 (1970), 94f. supposes *\*h<sub>3</sub>ṅbh-* > Lat. *umb-*. This seems to me very improbable, as we have no sure indication elsewhere

<sup>4</sup> The spirant of Av. *nāfa-*, as against *nabā-*, can perhaps be explained from this nominative *\*(h)ñōbh-s* > *\*nāf-š*, cf. *\*dhrugh-s* > *druh-š*. (Cf. the problem of Skt. *nakhá-*).

that the three laryngeals kept their colour in Latin. (Rix does not mention OHG *amban* etc.). For Greek he also supposes  $*h_3\eta bh-$ , but this would have given  $*\delta\phi-$  (-αλοϛ), not  $\delta\mu\phi-$  (cf. ἀδῆν <  $*ng^u\bar{e}n$ ). Greek, then, also points to full grade, which is thus attested by Latin, Greek and German (for State I). I have no opinion on the different paradigms:  $(*h)\acute{n}ébh-\acute{o}l$  is of course possible, but not all *l*-forms must derive from an old paradigm. Anyway, the phonetic evidence cannot be ignored.

(9.20)  $*h_3engh-/h_3negh-$  'nail'. Here we have much the same situation as in the preceding item.  $H_3-$  is shown by  $\mu\acute{o}\nu\nu\zeta$  that can only be explained from  $*sm\dot{h}_3nogh-$  (my article which is to appear in *Orbis*). Szemerényi here also explained Lat. *unguis* by a zero grade. Again, this is impossible, as is demonstrated by *inguen* <  $*eng^u\bar{e}n$  <  $*ng^u\bar{e}n$ . We must assume State I  $*h_3engh-$  for Latin, which establishes schwebeablaut.

(p. 62f.)  $*dhemh-/dhmeh-$  'blow'. Anttila is at pains to explain away State I forms: "To use *dhámati* as evidence for PIE schwebeablaut is not very elegant, inasmuch as the meaning makes onomatopoeic remodelling possible" is his last resource. I see no reason to assume remodelling, and beside *dhámati* we have *-dhama-*. These forms cannot have  $*dhm\acute{h}-o-$ , as this gives  $*dhima-$ . The most natural explanation is  $*dhemh-e/o$ , and  $*-dhom\acute{h}-o-$ .

$*h_2eud-/h_2ued-$  'voice'. The group is not mentioned by Anttila. Gr. αὐδή etc. against Skt. *vádati*; *Dev.*, 89.