

**The Triple Representation of Schwa in Greek, and Some Related Problems
of Indo-European Phonology**



Review Author[s]:
Brian D. Joseph

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representing numerous viewpoints, many of which are different from those which he presents in his text; readers of *Lg.* may find especially valuable the references to eastern European publications of the last 40 years.

In many other respects, however, E's book leaves much to be desired. Perhaps the most serious defects are that E is not concerned enough with getting straight the facts of the attested languages—e.g., he twice cites the Gothic for 'mother' as *mōdar* (45, 113)—or with the relatively short-range reconstructions that can be won with hard work from closely observed data; and he is too fond of trying to determine the original (*prvotní*, 28) functions of morphemes, and the origins (*počátky*, *ibid.*) of grammatical categories. E's Pre-IE reconstructions suffer from the same basic error as Schleicher's 'indogermanische ursprache': by internal reconstruction of PIE, we can push back in time only a few thousand years (say, to be generous, 10,000); and this, added to the 5,000 to 6,000 years that separate us now from PIE, gets us only a little way toward the origin of human speech—at the very least 50,000 years ago, to take an extremely conservative estimate. By internal reconstruction of PIE, we can see (more or less dimly) something of what the structure may have been like perhaps as much as 15,000 years ago; but if we think that by so doing, we are getting at the ORIGINAL structure or function of anything, we are deceiving ourselves. Moreover, E's technique of tracing all morphs of similar shape to the same pre-IE root leads to a pre-IE different in structure from all known languages; cf. his table (161) of CV-shaped pronominal roots—a sort of glottogonic broth, liable to appear in IE languages as conjunctions, numerals, pronoun stems, case endings, or personal endings of verbs.

Sometimes E's unconcern for exact data contributes to non-recognition of a true advance in the last 70 years. Thus his view (181, 190–91) that the thematic aorist was an important category of PIE presumably represents an informed rejection of the contrary view of C. Watkins, *Indogermanische Grammatik* III/1.63–4 (listed by E on p. 171). But of the five Sanskrit thematic aorists cited by E, only *avidat* occurs in the Rigveda; and three—*aricat* C., *avrtat* AV.B.C., *ayujat* C.—are manifestly replacements of older athematic aorists RV *rikthās*, *āraik* (on which see J. Narten, *Die sigmatischen Aoriste im Veda*, 1964, 223–4), *avart*, and *ayuji* (on the ending of *inj. yujanta*, see K. Hoffmann,

Der Injunktiv im Veda, 1967, 227 n. 225). And while *asrpat* AV.B. has no older attested aorist, the facts that its earliest occurrence is in the Atharvaveda, and that Gk. *hérpō* has no old aorist, strongly suggest that PIE **serp-* was praesens tantum. As for RV+ *avidat*, Watkins 152 has, by paying attention to the actual usage of La. *vidi*, shown that this comes from an athematic aorist of which *avidat* must be a thematization.

Such examples could be multiplied indefinitely. Only rarely does E present something true which is not part of the common stock of Indo-Europeanist belief—e.g. his recognition, 148, that Go. *ugkis* and Skt. *āvām* 'us two' both come from PIE ** η Hwe* (but then his pre-IE *NA-H₂Á* is wild again). E's errors in fact, and his mistaken or dubious judgments and reconstructions, make this book of little use as an un-supplemented introduction to Indo-European; probably its greatest value is as a key to a rich selection of recent literature. [WARREN COWGILL, *Yale University*.]

The triple representation of schwa in Greek, and some related problems of Indo-European phonology. By FREDRIK OTTO LINDEMAN. (Institutet for Sammenlignende Kulturforskning, Serie B: Skrifter, 65.) Oslo: Universitetsforlaget, 1982. Pp. 75. [Distributed in the US by Columbia University Press; \$15.00.]

L's main purpose in this interesting and meticulously documented monograph is to argue against the hypothesis that the IE laryngeals were maintained as three distinct phonological elements in the non-Anatolian languages. The conclusion he wishes to draw is that there was a 'syncretism of the laryngeals under one color' (70) at an early period. The evidence for a three-way distinction which L discusses includes the now common argument that the *e a o* which developed in Greek out of PIE **ə* (the 'triple representation' issue of the title) are 'original and [reflect] directly the original quality of the different laryngeals in interconsonantal zero-grade forms' (38), as well as the occurrence of prothetic vowels in Greek with three different timbres (*e a o*) and the putative coloring of **o* to **a* by an adjacent **H₂*. In addition, L takes up several problems which he believes to be re-

lated to the central issue, e.g. whether a sequence of postvocalic resonant + laryngeal yielded a double resonant in Germanic and Hittite, and whether a Palaic graphic *g* can represent PIE $*H_2$. In fact, though, these latter issues seem only indirectly related, in that they concern the development of the PIE laryngeals.

L's approach is necessarily polemic. He examines the evidence, i.e. the relevant forms in various languages that have been cited and analysed by others with regard to the questions mentioned above; and he attempts to evaluate the success of the particular analyses put forward for these forms. In general, he is able to find questionable features in these analyses, and he offers alternative accounts which are more in line with his hypothesis of a laryngeal merger. Sometimes he is quite successful, and is probably correct in claiming that the quality of the evidence has been overstated, as with the notion that Palaic *g* continued PIE $*H_2$ in some words. Often, though, his alternative accounts are less than convincing, and create more problems than they solve.

For example, L relies on a putative dissimilation of $*x^w$ ($= *H_3$) to $*x$ ($= *H_2$) / — ... w ... , on numerous occasions (e.g., p. 25 re La. *auris*, Gk. *oûs* 'ear'; p. 26 re Skt. *aviḥ*, La. *ovis* 'sheep'; p. 27 re Gk. *pôu* 'flock', Skt. *pāyu* 'protector', and re PIE $*ak'$; p. 29 re Arm. *aganim* 'I dress', even though dissimilation is usually a somewhat rare and sporadic process—and in this case makes sense only if $*H_3$, the *o*-coloring laryngeal, really was phonetically something like $[x^w]$, a claim which itself requires independent justification. Similarly, L often posits extra morphological steps and unattested forms where a recognition of the triple representation of $*\partial$ in Greek would simplify matters considerably. A case in point is his discussion of verbs like Gk. *emēō* 'vomit' and parallel forms (including the *ane*- stem of the isolated noun *ánemos* 'wind'). L assumes that Gk. inherited pairs of athematic and thematic present stems, e.g. $*wema-$ (with *a* from $*\partial$) vs. $*wemH-e-$ (with prevocalic $*H \rightarrow \emptyset$ eventually), and that the more productive thematic type was re-analysed as athematic, i.e. $*wemH-e- \rightarrow *weme-$, replacing the old athematic stem (p. 43); but if we assume that $*wem\partial_1- \rightarrow *weme-$ directly, we avoid the need for such a poorly motivated re-analysis.

Frequently also, L falls back on the argument that 'there is nothing to prove definitely' that a particular form is related to another, or derived

from a particular pre-form (e.g. p. 45 re Arm. *di-k'* 'gods' and Gk. *theós*). While relatively few etymological connections can ever be viewed as absolutely certain, this line of argumentation becomes extremely weak when over-used.

L has succeeded in demonstrating that it is possible to construct an account of the development of the PIE laryngeals in which $*H_1$ $*H_2$ $*H_3$ all merged in non-Anatolian IE. However, he has not shown that this account is preferable in any significant way to the standard view. L relies too much on ad-hoc explanations of troublesome forms—so that even though he is undoubtedly correct in saying that not all the evidence put forward in support of the standard view is valid, one is still left with a residue that is hard to explain away convincingly (e.g. Gk. *ánemos*). Nonetheless, this most interesting and thought-provoking book certainly merits the attention of serious Indo-Europeanists in general—especially those with an interest in laryngealistic matters, who will find here important challenges to many popularly held views. [BRIAN D. JOSEPH, *Ohio State University*.]

Homer and the Ionian epic tradition:

Some phonic and phonological evidence against an Aeolic 'phase'. By D. GARY MILLER. (Innsbrucker Beiträge zur Sprachwissenschaft, 38.) Innsbruck: Institut für Sprachwissenschaft der Universität Innsbruck, 1982. Pp. xvi, 192. öS 480.

M here presents a discussion of the data bearing on the issue of whether 'the [Greek] epics were originally composed in Aeolic and later translated into Ionic' (p. 9), thus going through a so-called 'Aeolic phase' before reaching their now-familiar form. The central problem is thus one of the development of Greek epic—a question properly in the domain of the Classics, some would maintain; but M brings to bear upon it the full resources of the linguist schooled also in the Classics, i.e. a true classical linguist (with equal emphasis on each member of the epithet). As his title indicates, M uses the data to argue against the postulation of a separate Aeolic phase—though importantly, he does not deny that interaction occurred between concurrently developing Aeolic and Ionic traditions.

M argues his case in two separate parts. In the first, Chs. 1–3 rehearse commonly held views on formulaicity in Greek epic, the origins