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# KRATYLOS

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OTTO HARRASSOWITZ · WIESBADEN

*Vom Verfasser überreicht · Durch den Buchhandel nicht zu beziehen*

Beekes, R. S. P.: The development of the Proto-Indo-European laryngeals in Greek. The Hague—Paris, Mouton, 1969, 4°, 24, 324 S. (Janua Linguarum, series practica, 42).

I have already elsewhere expressed my belief that there can be no progress in the problem of the Indoeuropean laryngeals if one doesn't consider the laryngeals with labial and palatal appendixes; if one doesn't proceed with a renewed concept of phonetical evolution; and if one doesn't apply phonological criteria. The author of this book does none of these three things; as a consequence he confines himself fundamentally to repeating the opinions of Kuiper and Kuryłowicz, with some minor contributions.

The book is important because of its gathering of material, and some contributions which come out in the study; above all that concerning the irregularity of the vocalizations (occasional dropping of *H* in indetermined circumstances) and the alternation between the vocalized and consonantal forms of laryngeals.

The book then, and this must be said, is written in a very systematic and scholarly manner and analyzes an important subject thoroughly: it has what the Germans call "Fleiß". It is then, let's say, an honest book.

However, B. fails when dealing with theories or authors with which he disagrees. If I'm not mistaken there is nowhere in the book a single mention of the hypothesis of the laryngeals with labial or palatal appendixes, neither in the form which I have given it nor in any other.

I think that this attitude which is beginning to creep into certain studies and which consists in not mentioning the work of others when their theories differ from the author's, will not contribute much, in the long run, to the scientific prestige of our studies.

I cannot avoid referring more concretely to my studies on the subject touched in this book. My articles in *Emerita* on the sonants are quoted in the Bibliography, but the author shows no signs of being acquainted with them; only with one of them—that relative to the prothetic vowel—he is indirectly acquainted through the *Estudios sobre las laringales Indoeuropeas* in which it is reproduced to a great extent. But it is unintelligible without knowledge of the other articles and this is why B. hasn't completely understood it.

This can be seen in his criticism of my ideas pp. 29 and 30. I, in fact, propose that in the treatment of the type gr., lat. *au-* there is a vocalization of the laryngeal; but the prop-vowel is no more than a phase in that vocalization. In other words, the author is agreeing with me, unawares. And I do not oppose in principle his idea (or to be more exact Kuryłowicz's idea) that there could be a prothetic vowel derived from the vocalization of the laryngeal in languages different from the Hittite and in distributions different from those corresponding to the Hittite *ḫw-*. Our discrepancy consists in this: that the prothetic vowel comes from the laryngeal is for me a possibility which must be proved in each case with concrete data; for B. it is an assumption, an a priori, and so, he gives long lists of words in which there might be ("could", "is possible", "allows", etc.) laryngeal. But to explain the prothesis by way of the laryngeal and the laryngeal by the prothesis, is a vicious circle. So much so that if the laryngeal can vocalize, developing a prothesis, so can sonants and even consonants. That is to say, the laryngeal hypothesis is a possibility which must be proven in each case: nothing more. But B. has confused ideas on the vocalization (which lead him to make mistakes on p. 190ss. when studying the dissyllabic roots, where he deals with the vocalization of the sonants and the reduced grade with prop-vowel as if they were two different things). He totally isolates the problem of prothesis from that of interior vocalization, and that of prothesis before a consonant from that of other prothesis. Also his phonology is confused. He considers (11) the reduced or supporting vowel which always alternates with forms without it, to be a phoneme, not realizing that we are dealing different allophones of the sonant or the laryngeal.

My *Estudios sobre las laringales Indoeuropeas* are known to the author, who criticizes the theory of the vocalic prothesis therein exposed, as has been said. But evidently his reading of the book has not passed beyond that page. Setting aside everything concerning the laryngeals with labial and palatal

appendixes, he doesn't even go into subjects like that of the vowel plus laryngeal groups and vice versa, nor that of the zero grade of the disyllabic roots. On these I bring forward facts contradicting B.'s solutions, which are more or less the traditional ones:  $H_1e/o > e/o$ ,  $H_2e/o > a/o$ ,  $H_3e/o > o/o$ ; the same vowels — but with long quantity — when the laryngeal follows the vowel; the following solutions:  $r\bar{a}/ara$ ,  $r\bar{o}/oro$ ,  $r\bar{e}/ere$  in the zero grade of disyllabic roots (here his acceptance of the influence of the timbre of the laryngeals is carried further than is usual); analogical character of the type  $tr\bar{a}$  development; and before a vowel, acceptance of developments  $rH_3 > or$ ,  $rH_1 > er$ , with the dropping of  $H$  ( $\epsilon\beta\alpha\lambda\omicron\nu$ ,  $\epsilon\tau\alpha\mu\omicron\nu$  would be analogical), as is also admitted occasionally for the full grades.

I cannot resume the discussion on all these points already dealt with by me. I would like to make it clear that I am not referring to the sections in which the incompatibility of my phonetical theory with the traditional one induces the author, scandalized by my phrase that "the neogrammatic concept of the phonetical law is not adaptable to our material" (cf. 29), to refuse to discuss them. (I think however that it would be more adequate to examine the data to see whether in fact a new idea on phonetical evolution is required instead of supposing a priori that the traditional one is correct.)

There are theories strictly within traditional phonetics and having the morphological support, that the author, without abandoning his own points of view, could have taken in account. For example, when I postulate that each laryngeal tinges the immediate vowel with its timbre and that the cases in which the vowel timbre prevails against that of the laryngeal are analogical. B. is simply unaware of this when he says for example on p. 166 that  $oH_1 > \bar{o}$  "is, as far as I know, undisputed".

In short the book is useful as regards the gathering of material and opinions and sometimes as regards its criticism, but it is written from a too aprioristic position: it is rather an exploration of material to adjust it to determined ideas than an unprejudiced examination of the same. Even so, of course, there are useful things to be found. I have mentioned above the acceptance of doublets (whatever the explanation may be) in the phonetic evolution of disyllabic roots, in the dropping or vocalization of the consonantal laryngeal; also I could point out the admission, after an attempt to minimize them, of analogical facts in the timbre of the prothetic vowel. A thing which on the other hand should not surprise our author considering his acceptance of the influence of the timbre of the laryngeal through the vocalic sonant on the vowel which develops before it.

On the other hand I do not think that there has been a vocalized  $a$  then altered by the timbre of following consonants or vowels, but that the prop-vowel, having in origin an indefinite timbre produced

different vowels. A position diametrically opposed, indeed, to that of B., who thinks that three prothetic vowels being given, three different Indo-European phonemes are implied. By this system the number of Indo-European phonemes that we could reconstruct would be incredible.

In general, it can be said that the study of prothesis as the foundation of the whole book, that is to say, the case in which the presence of the laryngeal is less sure constitutes a basic error. An error which, moreover, damages the rest of the book as it establishes that to each timbre of a Greek vowel supposedly derived from a laryngeal there must correspond in Indo-European a different laryngeal.

I insist on the fact that the presence of the laryngeal as a source of the prothesis can only be proved when there is a Hittite *hw*-form.

In the remaining cases B. suggests that the structure of the roots "permits" a laryngeal and adds certain interpretations, proceeding nearly always from Kuryłowicz, which presuppose a laryngeal. So we have his interpretation of the Attic reduplication: the material shows no independent proof that a laryngeal may have intervened. These lengthenings must be placed in connection with others in interior syllable without a laryngeal anywhere, like the latin type *uēnī* etc. Kuryłowicz himself has discarded this explanation. The same could be said of the attribution to a laryngeal of certain lengthenings in the first term of compound words. In fact, the cases in which the laryngeal is probable are rare.

So, for reasons of root structure in gr. *ἀλέξω/ἀλή*, *ἀέξω/got. waksjan*. I would like to repeat that I don't, in principle, object to the theory as B. thinks: only, the theory must be proved in each case and all kind of prothesis (whether laryngeal or not) must be studied when dealing with the vocalization whether in an initial or interior syllable.

To sum up, the book is laborious, traditional, a little ingenuous. It follows and develops in a determined, unitarian and systematic way, the ideas of Kuryłowicz. It contains things which can be very useful to future scholars, but it has the defects and deficiencies already pointed out which lead us in the end to find few novelties in it with regard to the laryngeal theory of the fifties. In a certain respect, it represents an even more backward state, as there is no track whatsoever of laryngeals with labial or palatal appendix.

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