

G. CARDONA, *On haplology in Indo-European*. Haney Foundation Series 1. Philadelphia, Univ. of Pennsylvania/ London, Oxford Univ. Press 1969 87 pp. 48/-.

This booklet was begun as a study of the instrumental singular in *-yā* of Vedic *ā*-stems with *y* preceding the *ā*, where the expected ending would be *-y-ayā*, the form *-yā* being explained as due to haplology (*-yayā* > *-yā*). It was then extended into a study of the phenomenon of haplology (restricted to Indo-European). The book strongly shows the traces of this origin: of some sixty pages of text thirty-five are devoted to the *yā*-instrumentals, where one finds many details that are not relevant to the problem of haplology, to

which I shall confine the following remarks. To my mind the material and the ideas could have been better presented in the form of two articles, one on *-yā* and the other as 'Some remarks on haplology'.

The central problem discussed is the question whether haplology, in general the 'minor sound laws', are regular as are the major ones or not, a problem to which recently Hoenigswald drew attention (*Phonetica* 11 (1964) 202-15).

On p. 48ff the writer gives a classification of the 'contexts' for haplology: in a sequence CVCV (or VCVC) of which C's are identical (a) across the boundary of separate syntactic units in a sentence (1) in a 'standard, frozen sequence' (Vedic infinitive *-tavai* < **-tavai vāi*) (2) in a not standardized sequence (AV 13.2.9b *āpāvṛkṭāmo* < *āpāvṛkṭa tām-*; Hesiod, *Aspis* 254 βάλλ' ἔνοχας < βάλλον ἔν.); (b) in a single syntactic unit (1) in a compound or suffixal derivative (Skt. *śevṛdha-* < *śevavṛdha-*; ἀμφορέως < ἀμφιφορέως; *voluntas* < **voluntitāt-*) (2) in certain forms of a paradigm (RV 1.54. 11d *svapatyāi* < *svapatyādyai*; Lat. *dixti* < *dixisti*). C. then states that there is a proportion $a_1 : a_2 = b_1 : b_2$, as in a_1 and b_1 the conditions for haplology are present regardless of the larger context. This also applies for b_2 , but here C. means systematic context: 'these conditions obtain only in certain paradigmatic forms'. There is the effect that a_1 and b_1 are without exception (according to C.), while a_2 and b_2 are only seldom generalized. This classification seems instructive, though one could raise some objections. First, a_1 is extremely rare; I doubt this explanation of *-tavai* as much as that of *-μεναι* which would contain a particle *ai*. Other instances of this type are not known to me; evidence, then, is very scarce. As regards a_2 — b_2 , there is this important difference that in a_2 the very sequence CVCV obtains only accidentally, while in b_2 it is constant in the given form.

C. considers cases of type b_2 . The normal development is here that the full form is restored by pressure of the system, as in Lat. *dixisti* (against haplogized *dixti*) by analogy of *dixi*, *dixit*, etc. (pp. 51-4). This case calls for some comment. For, if Cardona is right that a sequence CVCV or VCVC is the primary condition for haplology, then *dixti* itself (from *dixisti*) must be due to analogy. The forms *dixisse(mus)* also do not have CVCV (but CVCCV). In fact the relevant forms of the original system were as follows:

CVC	<i>dix(is)ti</i>	VCVCC	<i>promis(is)ti</i>
CVCCV	<i>dix(is)se(mus)</i>	VCVCC	<i>promis(is)se(mus)</i>

If we admit that CVCV or VCVC (not VCVCC) was essential, the shorter forms could not have arisen at all. If we assume that VCVCC and VCVCC are equal in this respect, the short forms must have arisen after vowel (the type *promis(is)-*); I do not know if there is any indication for this. It seems better to dismiss this condition (CVCV), or to assume that *dixti* is not a case of haplology at all. It has on the other hand been remarked (e.g. Sommer, *Handbuch der Lateinischen Laut- und Formenlehre*, 1913², p. 589) that these forms cannot be due to syncope, since there is no **fexti* from *fecisti*; the recurrence of the consonant (*s*) is apparently essential, and one might like to include the case under the term haplology. Further the forms are frequent in Plautus and in poetry, so that they are both colloquial and archaic. The Romance forms on the other hand derive from the longer ones. As the basis of the Romance languages may no less be called colloquial, one is inclined to look for an explanation. One might then consider the possibility that, though the shortening is not due to syncope, the (archaic) initial word accent together with the recurrence of the consonant (*s*) -- with a short vowel in between -- were the cause of these shorter forms. It would then be understandable that, once the initial accent had disappeared, the analogically restored longer forms were not again shortened (so that Proto-Romance only had *dixisti* as a basis). Thirdly, it seems necessary to consider the interaction with the type *amasti* (mentioned p. 51); it seems probable to me that the two types reinforced one another. These points should have been considered by C., as they are more relevant to haplology than much that found its way into the discussion of the Vedic *yā*-forms; this is of course due to the history of this book.

The haplogized *yā*-instrumentals are remarkable in that they exist at all in a paradigm, and even more so because in other cases there was no haplology (or very seldom): dat. *-yāyāi*, gen. abl. *-yāyāh*, loc. *-yāyām*. C. explains this as due to the support of adverbial forms with an (instrumental ?) suffix *-yā*. E.g. *vacasyā* would be liable to interpretation as instrumental of a *yā*-stem or as containing *vacas-* with a suffix *-yā*.

The third case considered by C. is the Vedic imperative in *-si*, e.g. *neṣi* 'lead'. C. accepts Szemerényi's explanation of this form as

the haplogized form of a second singular subjunctive, **naisasi* (sic) > **naisi* > *neši*. Against Szemerényi C. convincingly holds (I do not consider the probability of the explanation as a whole) that the generalization of the haplogized form *followed* on its separation from the subjunctive paradigm (where the full form would have been restored as in *dixisti*).

On p. 50f and 63 the existence of unhaplogized forms as ἀμφιφορέως, *allitudo* is discussed. If I am not mistaken, three possibilities are distinguished (two in each case, which gives four, of which two are identical): (a) the haplogized form is kept from being generalized; (b) it is a reformation of a haplogized form; (c) it is simply an unhaplogized form. To my mind this distinction is not useful. The essential fact seems forgotten that the formation of the unhaplogized form remains possible in the system of the language when the haplogized form has appeared, because the primary condition for haplogy (the sequence CVCV or VCVC) arises only from the combination of morphs (we limit ourselves to Indo-European). This is an essential difference with the 'normal' sound changes: by the time an *s* before vowel at the beginning of the word has turned to *h* (ἔδος < **sed-*), the original form with *s* cannot be restored, or 'created anew'. In any case haplogy is not a gradual development as are those of the normal sound laws, but a sudden one, so that of necessity the longer and the shorter form coexist. One looks in vain for such considerations on the nature of haplogy in the book. In this respect, then, there is a difference with the normal sound laws (which formulate a 'modification d'un mode articuloire', which is not the case with haplogy), and in general, I would conclude, there is essentially a struggle between the longer and the shorter form, which is not essentially present in the case of the 'normal' sound changes. I am not, therefore, convinced that we should put haplogy on a par with (all) other sound changes. What is needed, of course, is a study of all cases of (possible) haplogy in one language, rather than an illustration from several languages.

At the end (p. 63) C. dismisses Hockett's statement 'sound change tends to irregularize, while analogy tends to regularize', because the irregularity caused by the sound change *-yā* < *-yayā* 'was buttressed analogically'. I don't think this refutes the general statement. Here too one might say that the analogy creates a new

regularity (in fact the extension of the adverbial suffix *-yā*), though this regularity is not one within the paradigm: in fact the haplogized forms are removed from the paradigm (p. 46: they are 'commonly the only forms attested in the Rigveda of the *-yā*-abstract'). We have here two analogical forces at work, one within the paradigm (internal), the other in another subsystem (external) into which the forms are drawn. This is, then, no refutation of Hockett's rule.

I should like to object to the words 'we can also abandon the propagandistic wording of the young grammarians' (n. 65), inasmuch as 'propagandistic' arouses feelings of antipathy, which should be avoided in scientific argumentation (the more so because it seems to become a fashion to rail at the young grammarians).

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