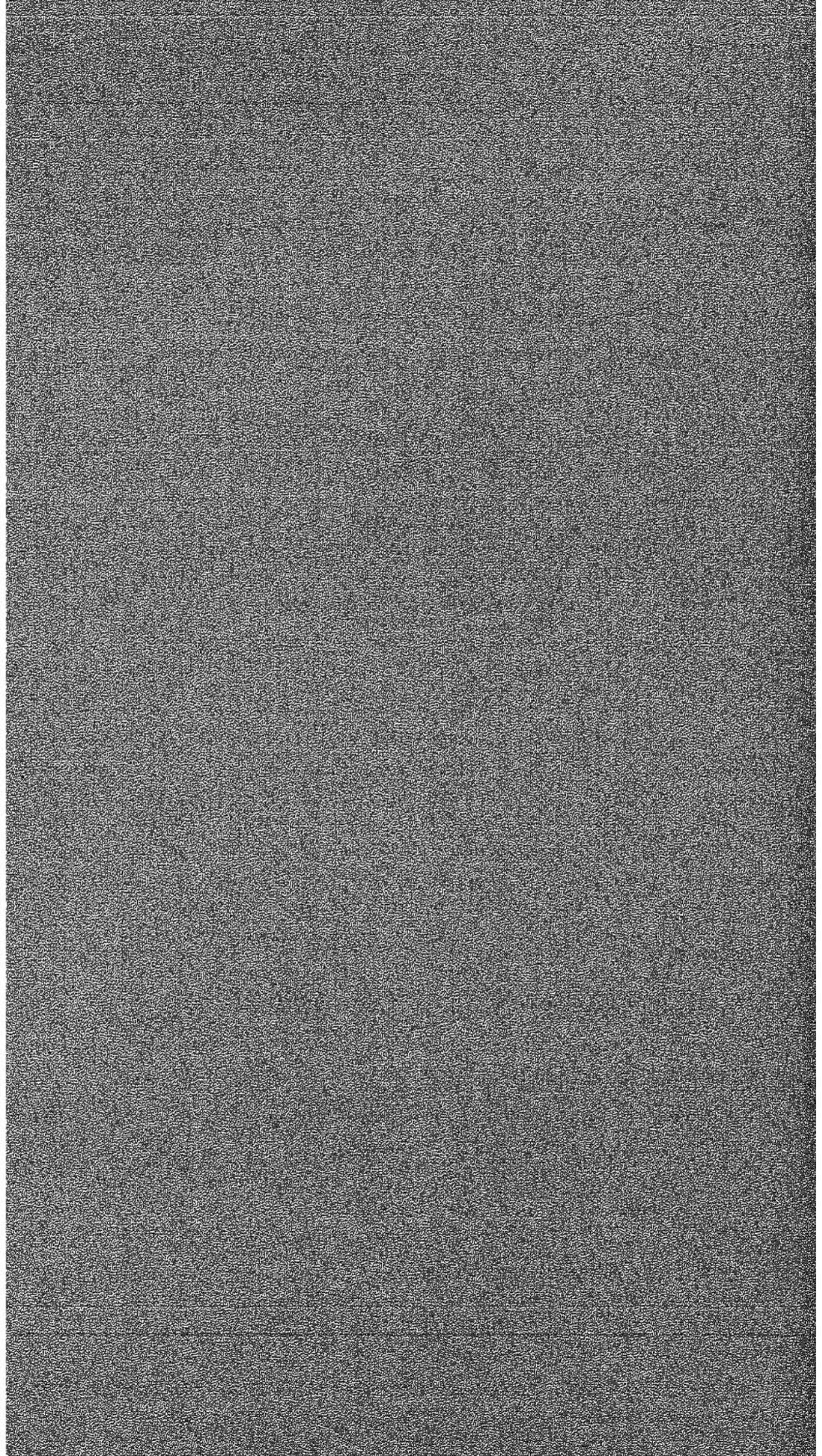


The Journal Of
INDO-EUROPEAN
STUDIES

Reprint



THE ARCHAIC STRUCTURE OF HITTITE: THE CRUX OF THE PROBLEM

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The Anatolian languages retained an archaic form of Indo-European, and had not developed polythematic inflexion in either the adjective or the verb. Belonging to a wave of people who migrated only to the Southern Caucasus, they were not affected by the linguistic innovations of the following waves of Indo-European people. The strata of Indo-European are established as follows: Indo-European I (pre-inflexional); Indo-European II (monothematic, kept by Anatolian languages); Indo-European III (polythematic; its variant IIIa or Indian-Greek is the basis for the traditional reconstruction).

In a recently published article in the journal *Emerita* (Adrados 1979), I discussed the relationship between the ever more widely accepted thesis of the archaic structure of Hittite within the context of those IE languages known to us, and currently widespread ideas on the expansion of primitive Indo-Europeans of the 'Kurgan' culture, ideas which have been postulated mainly by Professor Marija Gimbutas. As is known, this expansion took place in the form of a series of successive migratory waves of the Indo-European peoples from the fifth to the third millennia B.C., from the Turkestan region and the plains to the north of the Caucasus and the Black Sea, both westwards (Europe) and to the south (Anatolia and the Caucasus). Only at a later date, throughout the second millennium B.C., were there subsequent movements southwards, both in Europe (Greeks, Latins, Celts ... or their forerunners) and in Asia (Indo-Iranians, above all).

In the above-mentioned articles, I suggested that the chronology of these migratory waves was reflected in the different grammatical structure of the languages thus transported. I disregarded the first wave, which brought IE languages to Europe during the fifth and fourth millennia, as its reconstruction is far beyond our present capabilities. This was perhaps a non-inflexional form of IE, such as that which is acknowledged for an older period, or in any case a form of IE with incipient inflexion. I then spoke of a second wave (following Gimbutas' chronology), which spread from about 3400 onwards both to the Balkans, and to Asia Minor through the Caucasus. It is to this second branch, which crossed the Caucasus, that the

origin of the Anatolian languages is attributed nowadays (cf. Winn, 1974).

As the Anatolian languages were isolated to the south of the Caucasus, it seems logical that they should not have undergone the same evolution as other languages north of the same, from Turkestan to central Europe. This latter evolution is doubtlessly connected with a further migratory wave, the third, in the third millennium, to which the more recent structure of non-Anatolian IE should be ascribed.

This does not mean that this third wave brought fully developed forms of all those proto-languages which we reconstruct more or less accurately (some, the western ones, were only created at a much later date, cf. Tovar 1974 and 1977). It did, however, at least bring a series of languages with a general system of linguistic structure and certain common evolutionary tendencies: those of traditionally reconstructed IE which we call Brugmannian Indo-European.

It is generally accepted today that it was during the third millenium that IE took on the form which - with certain alterations - has come down to us through those languages upon which traditional reconstruction is based. Doubtlessly at that time PIE began to be differentiated, above all in the east, for we know of the presence of the Indo-Iranians in the Gorgam plain in the third millennium (cf. Ghirshman 1977), still outside Iran (to the south-east of the Caucasus), and from what we may judge, we also know that Greek, which is directly related to Indo-Iranian, came to Greece about 2000 B.C., as a clearly defined linguistic structure.

To return, however, to Anatolian, it is a well-known fact that it consists of a series of languages and dialects which lack some of the categories and functions of the rest of IE (the feminine, adjective gradation, the perfect, the aorist, the subjunctive), whereas others are distinguished in an incomplete way (in the case of the noun and adjective; nominative and genitive, sing. and pl. in the noun, 1st. and 3rd. persons and primary and secondary endings in the verb). Two hypotheses clash here: that by which Anatolian was thought to have lost all these categories and formal distinctions (the traditional thesis), and that which on the contrary, supports the idea that Anatolian represents an older form of IE, which had not yet developed, or which had not completed the development of a series of the traits of Brugmannian Indo-European.

In the first hypothesis, Brugmannian Indo-European is simply Indo-European; in the second it is at most the parent language of those languages which spread in Asia and Europe from a given moment onwards after the separation of Anatolian. If one applies the chronology of the Indo-European migratory waves we refer to above, and if one keeps the name Indo-European I for non-inflexional Indo-European (perhaps carried by wave I), one may conclude that wave II carried Indo-European II, the chief relic of which is Anatolian, and the third wave carried Brugmannian Indo-European (IE III).

This is precisely my own hypothesis, which of course should be backed by linguistic arguments. Nevertheless, before we go on to these, we wish to stress that from an historical, archeological and general linguistic point of view — this is a highly plausible hypothesis. Indo-European II must have been spoken at some time during the fourth millenium B.C., to the north of the Caucasus, in all or part of the Indo-European-speaking area of that time; it crossed the Caucasus southwards and lost contact whilst the peoples of the same language who stayed behind continued to evolve linguistically, doubtless in connection with the new peoples who arrived from the Volga region (wave III). Under these circumstances, the archaic structure of Anatolian (which does not exclude its own innovations) is quite plausibly explained. It was a relegated language, the situation of which was similar to that of others in such circumstances.

It was a language which produced a series of clearly differentiated languages or dialects, all spoken within the vast administrative and political organization of the Hittite empire. When speakers of Indo-Iranian dialects entered Asia Minor from 1500 B.C. onwards, and later Armenians and Phrygians, all of them representatives of Indo-European III, they were unable to substantially alter the structure of the old relic of Indo-European II, the Anatolian languages.

As is well-known, the hypothesis that Hittite did not derive from Brugmannian Indo-European but represents one of the results of a splitting into two branches of an older Indo-European which Sturtevant called Indo-Hittite, comes from this latter scholar (Sturtevant 1929/1933 and 1962), although certain earlier ones have been pointed out (cf. Villar 1979: 182). But Sturtevant was only interested in certain archaisms in Hittite, above all the preservation of the laryngeals. If there was indeed a splitting into two branches, different innovations

in one and the other were to be expected and this is what Sturtevant left out. This gave rise to criticism such as Pedersen's (Pedersen 1938) and that of a long series of later writers: I refer to the history of this matter recently compiled by F. Villar (Villar 1979).

However, as early as 1961 (Adrados 1962, publication of a report to the I *Fachtagung* of the *Indogermanische Gesellschaft* held in 1961), I posed the question in different terms: the crux of the problem lies in the lack of Hittite (and in Anatolian in general) of a series of IE categories and functions and of their formal markers. It was implied that the rest of the Indo-European languages had developed a series of common innovations, in the face of the more commonly held opinion that this has not yet been proved (cf. even now: Cowgill 1975:562). On the other hand, although there are certain innovations common to the whole of Anatolian in the inflexion of the noun, the pronoun and the verb, these are of much lesser importance. Rather than positing a splitting of Indo-Hittite, one should speak of a stage (II) of IE (or rather of PIE), preserved in Anatolian although altered by certain innovations, and of the development of a new stage (IE III) which, as we have stated, did not affect Anatolian.

When I fully developed this thesis of the archaic structure of Hittite in two later books which analyzed the facts in detail (Adrados 1963, 2nd. ed. 1974; Adrados 1975), it was still not widely divulged and in general was judged on the strength of an obviously incomplete and faulty formulation; that of Sturtevant. Certain isolated voices went unheeded, which, quite independently of my own (only late in the day did I hear of them, on the other hand), advocated that Hittite had not "lost" categories which it had never really had because these were innovations as against the rest on Indo-European (Kerns-Schwarz 1946; Hahn and Ivanov 1958). On the other hand, it should be mentioned that these were mere suggestions, not detailed studies which attempted to prove their subject. However, neither my essay of 1962 nor my two books, which then dealt with the subject in depth, received any attention. The "loss" of the feminine, the aorist, the subjunctive, etc., in Hittite was still being postulated without any attempt to prove it (thus, for example, Kuryłowicz 1958). Or, without giving any solid arguments concerning the problem of Hittite, it is still assumed that Brugmannian Indo-European was simply In-

do-European (cf. for example, K. Hoffmann 1970, H. Rix 1977).

However, in the past few years there has been a sweeping change in ideas about the situation of Hittite within the Indo-European languages: this is what leads me to discuss the matter once more in the light of new data, arguments and theories. I should like to point out three aspects:

1. That which I have dealt with so far: since we learned of the double displacement, westwards and southwards, of the Indo-Europeans from the Volga region, the preservation of Indo-European II in Anatolian and the existence of an innovating group, Indo-European III, with which the former lost contact, the hypothesis we are dealing with takes on a feasibility which one might term *historico-geographical*.
2. Compared with what happened at an earlier date, a series of studies, either relating to Anatolian Hittite, or to Indo-European in general, have lately stressed again and again that categories such as the feminine, the aorist or the perfect did not already exist in Hittite, but were later innovations of the rest of Indo-European. These are either general essays or discussions of specific points. I would mention, among others, papers by Kerns-Schwarz (1972), W.P. Lehmann (1974), W. Meid (1975 and 1979), W. Cowgill (1975/1979), O. Carruba (1976), E. Neu (1976), W.R. Schmalstieg (1977), W.P. Schmid (1979) and B. Rosenkranz (1979). Papers by Neu (from Neu 1967/68 on), Watkins (from Watkins 1969 on), Bader (above all from Bader 1971 on), Puhvel (1970), Cowgill (from Cowgill 1968 on), Meid (1971) and Jasanoff (1979), which deal with the perfect and the middle voice, also refer to archaisms in Hittite, although with very varied interpretations. It is noteworthy, although not strange, that part of these papers should take ideas from J. Kuryłowicz, who combines his thesis on the recent nature of the peculiarities of Hittite with a reconstruction of the origin of the Indo-European inflexional system (cf. mainly Kuryłowicz, 1964, 1977 and 1979): it was quite easy to conclude that some categories created by IE which do not appear in Hittite emerged after the isolation of the latter instead of having existed and having been lost later in Hittite. See, however, item 3. On the other hand, bibliography which points to the archaic nature of Hittite with respect to the rest of Indo-European on this or that specific point, is far more abundant (cf., e.g. van Brock 1964, on its as yet non-grammatical-

ized use of reduplication), as is that which discusses the evolution of Indo-European in such a way as to permit the justification of the archaic nature of this or that trait in Hittite (cf., e.g. W.R. Schmalstieg 1977).

3. It is easy to establish a relationship between the existence of a whole school of thought which advocates the archaic structure of Hittite, or its non-possession as yet of certain Indo-European III categories, and the fact that new bibliography has now appeared which is not content to state that Hittite lost the categories and inflexional traits I mentioned, but which has at last undertaken the task of trying to prove this. I refer above all to E Risch's (1975) and H. Eichner's articles. This latter writer has given his point of view clearly: neither the school of thought which states that Hittite did not have certain categories and forms, nor that which says that it did have them but lost them, have done more than to make statements: neither of them has tried to prove anything (Eichner 1975: 73). Eichner makes the attempt from the point of view of the latter school, but his statements with regard to the former are inaccurate, both with respect to my book of 1963 (which he does not know of) and a considerable part of the bibliography mentioned in item 2. On the other hand, there are not only Risch and Eichner. Some of the works mentioned in item 2 (works by Kuryłowicz, Neu, Watkins and Bader), together with statements on the recent nature of certain formations within Indo-European, or on the antiquity of this or that trait of Hittite, state that this latter had an aorist and a perfect (among other traits) which it later lost. And naturally, they try to prove this.

To sum up, one might say that above all at end of the fifties and beginning of the sixties, diverse manifestations on the antiquity of Hittite as heir to an archaic Indo-European which did not experience the innovations common to the rest, went unnoticed or were unheeded, or at most, provoked mere statements to the contrary. The same occurred with my book of 1963 and the later one of 1975, both of which were widely neglected. However, both conflicting possibilities or interpretations appear increasingly often in bibliography with arguments for and against, although at times these are detailed discussions on one or another specific point and lose sight of the problem as a whole. This is what I shall try to shed light on here.

The Two Conflicting Theses: Data and Arguments

It is easy to state the traditional attitude with regard to Anatolian and Hittite in general: a series of categories and forms of Brugmannian Indo-European were lost, this latter being the only one which we can reconstruct beyond speculations which Kuryłowicz disparagingly called glottogonical (1975:93). These speculations are really not at all glottogonical and they are, on the other hand, assiduously cultivated by Kuryłowicz himself despite his anathematizing of them. He does well in this and the same goes for the others, for not only by comparison but also through internal reconstruction on the strength of "fossils" which evolution has left intact, are we equipped with scientific instruments for a reconstruction of the successive stages of Indo-European. Nobody can deny its fundamental stages: pre-inflexional and increasingly inflexional, although at times some prefer to forget them. To return to our subject, we repeat that, for this school of thought, Anatolian has lost a series of fundamental traits of Indo-European and has introduced certain innovations. This is all.

The opposite school of thought is not quite so easy to summarize, among other reasons because there is no absolute unity in the ranks of its supporters. As a starting-point, I would prefer to give my own attitude to the facts very briefly, an attitude already summarized in Adrados (1979) from former books; and I wish to add to mine that of W. Meid (1975) who, although unknown to me when I wrote my paper of 1979, closely coincides with mine, as likewise those of Schmid and Rosenkranz (1979). This coincidence between scholars who have worked independently may be significant. I shall later give an account of other studies.

For practical reasons of convenience, my starting-point could be the use of tense in verbal inflexion. It is well-known that tense is expressed in two ways:

- a)– By means of the opposition of two series of endings, primary and secondary. Independently of the variants presented by these endings, the primary series tends to be characterized by means of an added *-i*; the secondary one is the older series without *-i* which, when opposed to the former, tends to indicate the past (sometimes also the moods and even occasionally it is kept in the present). The secondary charac-

ter of the addition of the *-i* and, therefore, the older atemporal nature of the Indo-European verb, is today acknowledged by practically everybody; *-i* is an old deictic particle which relates verbal action to reality and present time.

b)– But in Brugmannian Indo-European, tense is not only marked with the aid of endings, but also through the opposition of stems. This is a redundant system: to a stem A which indicates the present and past, according to which ending it takes, a stem B is opposed (an aorist stem) which, followed by the secondary endings, indicates the past.

The linguistic system in which each verb has only one stem is that which we term monothematic and is peculiar to Indo-European II: this thesis implies that tense marking with the aid of endings alone, that is, system A, is older than tense marking by means of a combined use of stems and endings, that is, system B. System B corresponds to what we term polythematic inflexion and is peculiar to Indo-European III. We use the term polythematic because this is not a case of the mere opposition between present and aorist in the indicative. There is also opposition of a third indicative stem, the perfect, independently of its temporal definition (doubtless a more recent one and pertaining only to some languages or groups of languages of Indo-European III).

Moreover, there are modal stems which are marked by means of a combination of characteristic stems and endings. Furthermore, the modal stems at times remain subordinate to the temporal ones. There is, for example, an aorist subjunctive or present optative, whilst at other times (in Tocharian, Celtic and Latin, above all), there are clear traces of direct derivation of the moods from the root. In this case, we speak of simple inflexion, and in the former, of complex inflexion.

To sum up, in Indo-European II, of which Anatolian is a relic, there is monothematic inflexion; in Indo-European III or Brugmannian Indo-European, there is simple polythematic inflexion (with stems derived from other stems). On the other hand, as far as the indicative stems are concerned, one should differentiate Indo-European III a), or southern Indo-European III, which we also call Indo-Greek and which preserves the three stems of present, aorist and perfect, from Indo-European III, which creates purely bithematic inflexion by fusing the old aorist and perfect into one sole stem. The

first group is that of Indo-Iranian, Greek and Armenian (and perhaps Trace-Phrygian), which tends to split the perfect into a present and a past (past perfect) and to keep an independent imperfect; the second is that of the languages from Tocharian to Celtic, through the Baltic, Slavonic, Germanic and Italic languages.

The decisive factor in evolution must therefore have been the creation of oppositions between stems with grammatical value. This occurred not only in the verb, but also in the noun, in which Indo-European III opposes masculine and feminine (though not always) by means of two different stems. In the adjective system there is even a complex polythematic inflexion: by means of a second suffix, both masculine and feminine are able to have comparative and superlative degrees. In this case too, Indo-European II keeps to a monothematic model. Of course, there are other archaisms, such as the above-mentioned remains of the non-distinction between noun and adjective, nominative and genitive, persons and voices, etc. With regard to the noun and adjective, which will be only summarily dealt with here, I refer to Villar (1974) and Adrados (1975), cf. also Laroche (1970) and Meid (1979: 165 ff.).

I offer below a schematic table as follows:

| Migrations | | Linguistic Stages |
|---------------------------|--------|---------------------------------------|
| Wave I (V/IV mill. B.C.) | IE I | Pre-inflexional (?) |
| Wave II (3,400 ff. B.C.) | IE II | Monothematic |
| Wave III (3,000 ff. B.C.) | IE III | Polythematic (a: multiple, b, binary) |

The traits still considered to be non-existent in Indo-European II or Anatolian and which were developed at a later date are approximately the same as those which are given in the above-mentioned works by W. Meid, W.P. Lehmann, B. Rosenkranz, O. Carruba, etc. There are naturally differences in the details: Rosenkranz, for example, insists on a series of syntactic data and launches the stimulating hypothesis that the opposition of stems as we have described it may have taken as its model the opposition between a basic and a deverbative verb, which already existed in Anatolian. But even more remarkable is the fact mentioned above that W. Meid (1975), although he does not give a general definition such as ours on

the most important traits which differentiate the various "Indo-Europeans", does imply this in practice. The above-mentioned fact that he should draw up a table so clearly resembling our own is even more remarkable. Thus, when he distinguishes within "recent Indo-European" a group III a) or Indo-Greek, and a group III b) or "older Indo-European", which according to him, derived directly from "middle Indo-European" or II, represented in general terms by Hittite. Both of us also sustain that "early Indo-European" or IE I contained a series of archaisms which survived in the whole of the later Indo-European area, or at least in part of it. Likewise, Meid (1979), without exactly supporting the Indo-Hittite hypothesis, offers arguments in favour of the recent nature of the feminine, aorist, perfect and moods, thus refuting Eichner (see below).

We shall deal no further with the ideas of other scholars who, like Schmalstieg (1977 and 1977 bis), attribute the creation of the Indo-European desinential system to an earlier date than that of the system of stem oppositions and who, therefore, whole-heartedly support the theory of the archaic structure of Hittite. We shall now examine in closer detail the suggestions which have recently been made to the effect that the Indo-European perfect is a relatively recent formation, which was created later than Anatolian (that is, Indo-European II).

It all began with Stang and Kuryłowicz's study of the parallel or close coincidence between the middle voice endings of Hittite: *-ḫa*, *-ta*, *-a* on the one hand, and on the other, the Indo-European endings of the perfect *-a*, *-tha*, *-e* (there is often an *-r* too in both series in the 3rd. pl.). They added that in Indo-European a 1st. sing. middle voice *-(m)a(i)* existed and also a 3rd. sing. middle voice *-o* (cf. Ambrosini's data 1965 and Cowgill 1968). From this point, they reached the now widespread theory that the two desinential series of Hittite *-mi*, *-ši*, *-ti* and *-ḫa*, *-ta*, *-a* definitely preserve an Indo-European model older than the commonest in the other languages. According to Kuryłowicz (1964) the starting point is to be found in a verbal adjective in *-e* which has been reinterpreted as a perfect and recharacterized as such by opposition to the present; according to Cowgill (1979): 34) a thematic noun or adjective.

This gives rise to a series of very complex problems which I cannot examine in detail here. Various scholars, among whom the most distinguished are Kuryłowicz (creator of the theory and outright supporter of same, Kuryłowicz 1977 and 1979),

Neu, Watkins and Bader, state that there was a split between perfect and middle voice and identify the former with the Hittite *-hi* conjugation, the endings of which: *-hi*, *-ti*, *-i*, according to them come from **-hai*, **-tai*, **-ai*, that is from a contamination of *-ha*, *-ta*, *-a* with the *-mi* conjugation. In the preterite of these verbs, the 1st. sing. in *-ha* (Luw.) or *-hun* (Hitt., a form which is given as contaminated with the *-un* of verbs in *-mi*, but see Adrados 1963: 117) also should come from the the same perfect. The truth is that this hypothesis poses serious problems, not only phonetic and semantic ones, which are obvious, but also morphological ones: the *-hi* verbs are not stems opposed to others of the same root, but represent a monothematic inflexion of certain roots; and their formal similarity to the Indo-European perfects displays serious irregularities as far as vocalism and reduplication are concerned. The real parallel to some of the traits of the Indo-European perfect is really not in the *-hi* verbs but in the middle voice in *-ha*, which occurs in both conjugations, that in *-mi* and that in *-hi*. But this in turn poses problems.

All this has received scant attention from the school of thought to which I refer, more interested in developing a theory on the origin of thematic inflexion, supposedly at first a middle voice form, from the 3rd. sing. in *-o* or *-e* of the old Indo-European perfect. However, this has given rise to studies such as those of Puhvel, Cowgill and Meid mentioned above, who demonstrate that the Indo-European perfect can neither be explained by the *-hi* conjugation nor vice versa: that both forms definitely come from an Indo-European pre-form (cf. in this respect Adrados 1963, see below). This may be said to be Kuryłowicz's latest attitude (1979). Only the Preterite-presents of Germanic and their equivalents in other languages are strictly comparable for Meid (1971: p. 36 ff; 1979: 173 ff.) with a few *-hi* verbs of Hittite with stative value (and, at times, the vocalism *o*). The Indo-European perfect as one stem opposed to another, a present one, and presenting a series of formal developments, would have originated later than Hittite; van Brock's study of reduplication (1964) leads to the same results.

One should note that these arguments have impressed supporters of the existence of the Indo-European perfect in pre-historic Hittite, who now seek traces of it in this language,

preferably in the last forms of the *-hi* verbs. This is the case of Risch and Eichner in their above-mentioned works (and even in Meid 1979: 174). Eichner certainly finds them too in the present *-hi* forms, which partly coincide with the preterite-presents mentioned above (Eichner 1974: 88). As for the rest, the theory that the *-hi* inflexion is secondary and derived from an older middle voice is practically unanimously accepted; Jasanoff (1979), who thinks it is of the same date and origin as the perfect, puts forward an unsustainable phonetic explanation. However, for greater feasibility, see Becker (1971) and González Fernández (1980) on the same subject.

To sum up, it is now neither our intention nor within our scope to treat in depth the problem of the origin of the perfect and middle voice (we have done this in Adrados 1981). We shall restrict our study to examining the state of the problem critically. This latter consists of the following points:

a) The antiquity of many of the endings of Hittite is generally acknowledged, a fact which does not exclude the existence of innovations.

b) The existence of relationships between Indo-European perfect, on the one hand, and Hittite *-hi* inflexion and middle voice in *-ha* on the other, is also acknowledged.

c) The continuation of the Indo-European perfect in the *-hi* inflexion is not, however, unanimously accepted, but there is a tendency to consider only the preterite-presents to be found among the verbs in *-hi* as archaisms. The Indo-European perfect as one stem opposed to another had either not yet arisen or (according to Risch and Eichner, see below) had been converted into a preterite, being contaminated by the aorist and the imperfect.

I wish to recall in this context that in my books of 1963 and 1975, I suggested the attribution of the Indo-European perfect to the post-Anatolian epoch (cf. Adrados 1963: 186 ff., 756 ff., and 1975: 695 ff.), that is, to IE III. I took as the starting-point for its formation both forms in *-hi* and in *-ha* (middle), which I believed were two different grammaticalizations, with *i* and *o* respectively, from older *-H* stems. Forms such as **deH₃-*, **tr̥neH₂-*, give *dahhi*, *tarnahhi* (and there are comparable cases which produce *-ha*). That is to say that the desinences *-hi* and *-ha* come from a false cut of the end of the roots and stems in *-h* which were originally uninflected. They later

spread and were grammaticalized in two different ways (*-hi* and *-ha* from **-H-i* and **-H-o*). Moreover, it is certain forms in *-ha* (for the rest comparable in origin to those in *-hi*) that, when added to particular characterized stems charged with special meaning, created the perfect in Indo-European III by opposing these new formations to others of the same root. With regard to the special case of the preterite and perfect forms in *-u* or *-ua(i)*, cf. my forthcoming paper on the subject: "More about the laryngeals with labial and palatal appendixes".

To the extent that these studies, with slight differences in details, lead to the more radical conclusion that only Post-Anatolian Indo-European or III reached the stage of polythematic inflexion (simple or complex, multiple or secondarily reduced to two stems), the different chronological stages of Brugmannian verbal inflexion should be set out in a general table as follows:

| | | | | |
|-----------|-----------------------|-------|-------|------|
| IE II | PRES. IPVE. IPF | | SUBJ. | OPT. |
| | Z | | | |
| IE III | ADR. | IPVE. | SUBJ. | OPT. |
| | PERF. | IPVE | SUBJ. | OPT. |
| IE III a) | PLUSC. | | | |
| | FUT. | | | |

The different forms and categories of the verbal inflexion of classical or Brugmannian Indo-European are of different stages, as may be seen.

Indo-European III added a series of elements to the inflexion of II and, within III, III a) (Indo-Greek), added others. The table is naturally incomplete. It does not indicate that some of the categories added in IE III did not reach certain languages or that sometimes complex inflexion did not occur,

but that the imperative, subjunctive and/or optative were derived directly from the root. Neither does it indicate that it is not certain that all the traits attributed to IE III a) existed in each and every one of the languages of this branch or that the future is found outside them. Other details are also missing. Amongst these, the most important are the deverbatives which existed in Hittite together with semithematic inflexion and which, according to the languages in question, were preserved to a greater or lesser extent in IE III.

Even so, I believe that this table sufficiently represents the idea one might get today of the staggered evolution of Indo-European conjugation. It would be possible to construct another parallel one with regard to nominal inflexion, although in this case, I should have to give details of the evolution of monothematic inflexion in the diverse languages, a point which would oblige me to justify a series of personal opinions (cf. Adrados 1975: 327 ff., partly on Villar 1974). As the ideas on verbal inflexion which I expound here are more widespread, it seems safer to argue on this footing.

Compared to the table of Indo-European verbal inflexion I have just given, in which this latter is conceived as a successive creation of forms (although at times there are regressions such as the creation of a preterite on the basis of elements from the older imperfect, aorist and perfect in III b), it would be easy to give another which developed the opposite conception: the archaic nature of the whole of inflexion, with a reduction of forms in Anatolian and eventually in other languages. It would suffice to remove the lines which divide our table into three sections, thus attributing the whole to a remote date. However, even the most fervent supporters of the Brugmannian conception make certain concessions as regards the third sector of our table and, for example, acknowledge the recent nature of the past perfect (thus Hoffmann 1970) or the future.

We are once more faced with the old theory that Indo-European had possessed the system of opposition of stems from a remote era: according to Mme. Bader, who elsewhere pays great attention to the study of archaisms in Hittite, even prior to the opposition of primary and secondary endings (cf. Bader 1974: 24 ff.). At other times, even though the secondary nature of stem opposition is acknowledged, for example, in the use of *-s* to mark the aorist, it is not made

clear whether this archaic feature implies that the system in question existed at a prehistoric date in Hittite (thus Watkins 1962: 90 ff., Cowgill 1979: 35 ff.). It is clear that certain authors stick to the older ideas, for they tend to touch upon the problem of the antiquity of stem opposition, leaving the problem posed by Hittite in this respect somewhat to one side. It does not seem to us (cf. Adrados 1963: 931) to be a very logical attitude when a whole book, and an important one at that, on the history of Indo-European verbal inflexion such as Watkins' (1969), should deal with the endings and the thematic vowel but hardly at all with the stems. However, it may be only human to provisionally keep under one's hat a doctrine one feels to be uncertain, but which carries the weight of tradition with it, when one cannot for the moment see how to dispense with it without provoking a revolution in the most generally accepted ideas.

However, we have stated that in the past few years, apart from papers asserting in general terms the antiquity of the Brugmannian verbal type which was supposedly lost in Hittite, two important articles by Risch and Eichner have been published which for the first time attempted to demonstrate the older existence in Hittite of several stems within each verb, stems which were lost secondarily. According to these writers, Hittite lost the aorists (both sigmatic and radical) and perfects, subjunctives and optatives, but it preserved relics which demonstrate the older existence of these categories.

Any attempt to demonstrate this has been fundamentally based on the aorists and perfects. As far as the former are concerned, there are two arguments:

a) Certain *-hi* presents are radical, thus *kuer-*, *šanḫ-*, *lukk-*, and coincide with radical aorists in Indo-European languages: cf. e.g. O.I. *ákar*, *ásanat*, *árukta*. There are also radical *-mi* presents (e.g. *kuemi*). According to this theory, an aorist stem with the secondary endings has been kept as a preterite; with the primary ones, this has given a present. Moreover, certain present stems take an *-š*, thus *naiš-*, *paš-*: these must have likewise been derived from older aorists.

b) The preterite of the *-hi* verbs has *-ś* in several persons as a desinential element: 3rd. sing. *-šta*, 2nd.-3rd. *-š*, 2nd. pl. *-šten* (and middle voice *śduma(t)*). This must have been an element which came from sigmatic aorists.

This latter idea is completed with the theory that the older

perfects became preterites in Hittite: a phenomenon comparable to the well-known one in the languages of Indo-European III b (Germanic, Latin, etc.). Whilst some perfects must have entered the *-hi* inflexion in one way or another, with its various groups, others must have been fused with the old aorists to create a preterite of the *-hi* inflexion, parallel to that of the *-mi* inflexion, which is derived from the old imperfects. These arguments are based on the *-ha* of 1st. sing. of said preterite in Luwian (Hitt. *-hun* must be a contamination of this form by the *-un* of preterites of the *-mi* verbs); and in the 3rd. pl. in *-er* or *-ir*, which Risch insists on deriving from the perfect supporting his arguments with O.I. *-ur*, Lat. *-ēre*, etc.

These suggestions have met with a certain amount of criticism. Cowgill (1979) and Kuryłowicz (1979) thought it highly implausible that a perfect turned into a preterite should give rise, at a new stage, to a present (**-hai* > *-hi*, in hypothesis); there is no example of this in the Indo-European languages. Moreover, Meid (1979:169 ff) demonstrates that the assumed remains of aorists and subjunctives in Hittite are merely formal elements to which an old aorist or subjunctive meaning, for example, cannot be attributed.

How Should These Arguments Be Judged?

Risch's and Eichner's articles are a good starting point for examining the research method to be followed with regard to the archaic structure of Hittite. They basically present certain forms of Hittite which formally coincide with this trait or that of the aorist and perfect in Brugmannian IE; and once the formal coincidence has been established (really a partial one), they then come to the conclusion that between a) — the forms of Brugmannian IE, and b) — the corresponding forms in Hittite (or Anatolian in general), there is a relationship which proves that the latter are derived from the former. The "working hypothesis" that the Brugmannian verbal scheme is older than the Anatolian system is thus thought to be proved. The above-mentioned scholars then proceed likewise, and in this case with hardly any data, for categories such as the subjunctive and optative. The method may be taken over to the noun system: once the feasibility of the existence in Hittite of old *-ā* stems has been demonstrated, one automatically deduces that if they lack the feminine meaning pertaining to *-ā* stems (not

always, on the other hand), of Brugmannian IE, this is due to the fact that they lost it.

That is to say that this is not really a valid demonstration, but a preliminary statement that there existed an Indo-European conjugation of type III, which was secondarily reduced to type II preserved in Anatolian. This is what Risch's ideas basically come to when he asserts that Indo-European contained "feste Paradigme" (Risch 1974: 248) or Eichner's when he insists (I believe, quite wrongly) on the regularity and the 1:1 ratio of traditional Indo-European (Eichner 1974: 74). Hoffmann is more radical (1970) when he merely advocates that this Indo-European is the older, with hardly any reference at all to Hittite. In fact, they begin with Indo-European III and almost completely refuse to reconstruct its former history, despite the fact that there have been repeated demonstrations of the secondary nature of oppositions such as those of tense, voice and mood and even of the secondary nature of all inflexion.

In the face of this type of argument, it is easy to imagine what the contrary might be. Elements such as the radical stems, the *-s*, the stems with a final *-h* (not only in 1st. sing. if one accepts that $-ta < *t\bar{H}_2o < *-H_2to$ and that $-a < *-H_2o$ with loss of the laryngeal, cf. Adrados 1963: 103 ff.), are considered from this angle as formal elements of old Indo-European (II and even I) which were used to create the new categories and functions of new Indo-European (III). It is therefore clear that if the aorist, perfect or subjunctive did not yet exist in Indo-European II, there were no markers of the aorist, perfect or subjunctive either. When they were created, for the expression of these categories and functions formal traits had to be used which originally had a different value. This is a key principle on which I have repeatedly insisted (cf. Adrados 1963: 71 ff., 1965: 147 ff., 1968: 27 ff.) and which is rarely acknowledged.

In fact, when we are faced in the system of Indo-European II with a form comparable to those used to mark categories and functions which only exist in Indo-European III, we are confronted with a dilemma:

a) It may be thought that this form is a relic of the system of Indo-European III, when it lost certain categories and produced the more recent system II. Those linguists who, at least in general terms, advocate that Indo-European III (Brugmannian IE) is the oldest model of Indo-European available to us, believe

this interpretation to be automatically proved.

b) It may on the contrary be thought that this form is the starting-point for the creation of a series of categories of Indo-European III which did not yet exist in older Indo-European II.

It is, however, obvious that the supporters of this latter interpretation, among whom I count myself, must supply proof of their thesis. It is not enough for us to make "a priori" statements that Indo-European II is the older; although if the oldest phase of Indo-European is IE I or non-inflexional, as is commonly acknowledged, there would be a certain logic in the suggestion of a chronology: I - II- III, with progressive development of morphology (later inverted from a certain stage onwards), One really cannot affirm that a proof has not been attempted; we have given some information of this above. Perhaps, however, this proof has up to now been insufficient, as the traditional attitude is not only still maintained, but is defended with precise arguments for the first time.

I am not going to give here the details of the arguments in favour of the theory that the Indo-European categories missing in Anatolian were not lost by this latter, but that they were created in later languages which developed isoglosses that did not spread to Anatolian, isolated as it was to the south of the Caucasus. For this, I should have to repeat part of our arguments in former books and articles, discuss a series of suggestions by several linguists after this, and, in fact, carry out a new full-length study. We attempt this in a series of articles (Adrados 1981, 1981 bis, 1982, 1982 bis). What I do wish to achieve here, however, is to state a few of the general criteria which, to my mind, should be followed in this research. In short, this means making feasible the theory that such and such a form of Indo-European II is not a relic from a former phase III, but on the contrary, something alien to categories which were then non-existent. Only at a later date, when these categories were created, were the forms in question used to mark them.

What is really needed is a theory which explains how one form, in a newly created system, goes from one meaning or function to another. A series of arguments are also needed to help discover traces of a recent, secondary nature in the already formed new categories and their formal markers; for example, that *-s* has not always been an aorist marker, *-ā* a feminine marker, etc. Thus, instead of automatically judging any form of

Indo-European II to be a relic of its usage in III, relics of their older belonging to a previous system, II, will be found in many forms of III.

The fact that there are semantic displacements within the grammatical categories and that a desiderative, for example, may become a future, has never been doubted. On the other hand, the theory formulated by Kuryłowicz (see, after former statements, Kuryłowicz 1964: 9 ff. and 1977), represents an advancement in this field. It basically consists of accepting the polysemic nature of certain forms, which almost always implies the existence of both a primitive and a secondary meaning: this latter sometimes becomes expressed by a derived form (e.g. Eng. *I am writing* against *I write*); on the contrary, at other times, it is the older form which comes to express the secondary function, the primary one being expressed by a derived form: thus, Aesl. *pripekō* passes into the future whilst it is ousted by the derivate *pripěkajō*, in the first place from its imperfective function and later from the indetermined one.

This theory of ousting has been widely used by Kuryłowicz and Watkins. The latter, for example, (1962: 124 ff.) admits that the subjunctive is a secondary function of the -s aorist, which in certain roots of Old Indian ousts an older -ā subjunctive. Kuryłowicz (1977: 90 ff.), to give an example, speaks (and, I believe, correctly so) of the origin of the *e/o* subjunctive in indicatives which at times take on a modal value (a fact which is treated in detail in Adrados 1963) and whence it was ousted by other forms; but he believes that it first passes into the future, whence it is in turn ousted to the subjunctive by a -s future derived from the desiderative, etc.

These and other explanations permit us to see how new categories such as the durative present, the future or the subjunctive, arose from a semantic differentiation expressed by means of formal markers which previously did not have that meaning. The theory represents a step forwards, but, I believe, still has certain inadequacies.

It indeed gives too much weight to explanations based on supposedly derived forms which are really very often older independent forms. In fact, only secondarily were they felt to be derivates, being opposed to one sole form now thought to be the basic one. This does not mean that the opposition and a sense of derivation cannot be stressed by means of various devices, e.g. a lengthened or *o* degree of the vowel. To be

precise, the so-called IE deverbative verbs are perceived in each language as derivatives, but in their origin they were independent verbs of the same root.

Another criticism which should be made is that the process of ousting seems far too mechanical. Rather than state that the aoristic *-s* transformed into a subjunctive characteristic has "ousted" the *-ā* subjunctive in Old Indian, it would seem more accurate to state that both in this and in many other Indo-European languages *-s* and *-ā* lengthenings existed which were used in some cases to oppose an aorist to a present, and in others a subjunctive to an indicative. A root *R* may have been opposed to the lengthened form *R-s* as present of aorist or as indicative to subjunctive; and the same may be said for *R* to *R-ā*. Moreover, it is clear that in a certain language the derivatives with *-s* and *-ā* of the same root may have been specialized in the two different meanings, in order to avoid homonyms. However, both *-s* and *-ā* can also appear in the indicative when there is no ambiguity. In this way Lat. *amā-s* is an indicative as against *amē-s*, but the same *-ā* marks the subjunctive in *dicā-s* as against the indicative *dicis*. It is clear that both *-ā* and *-ē* have developed, according to context, an indicative or subjunctive value from an older phase of model indifferenciation and that later, for each value, only one of the two remained, according to the opposition into which they passed, thus polarizing the opposite form in the contrary meaning. For further details cf. Adrados (1963: 226 ff., 416 ff., 542 ff., 768 ff., etc.).

Kuryłowicz's and Watkins' theory has important merits but somewhat disregards the fact that certain morphemes which were indifferent of a yet non-existent grammatical category, became markers of one of the terms of same by means of polarization. However, it is a commonplace that, for example, the endings *-mi*, *-si*, *-ti*, are more recent than *-m*, *-s*, *-t*, originally indifferent of tense, and became preterite markers through polarization (except when they kept their indifferent tense value). Cf. Adrados 1962 bis, 1963: 69 ff., 1965, 1968.

Thus, for example, the whole school of thought which follows these authors states time and again that the *-e* or *-o* of the 3rd. sing. perf. or 3rd. sing. of the middle voice originally had a middle value. They thence draw far-reaching conclusions such as to define Gr. ἄγει as an older form of the middle voice and likewise the first persons in *-ō* (supposedly coming from

*-o-eH) and practically all thematic inflexion. Mme. Bader has written several lengthy articles supporting these ideas (Bader 1974, 1975, 1976, 1978 above all). However, -e/o (and -to) are found in numerous active forms. To give preference to the middle voice meaning over the active is just as arbitrary as the contrary. The most accurate thing one may state is that -e/o originally had no voice value. There is no reason whatsoever to qualify the Hitt. forms in -*ha*, -*ta*, -*a* or the IĒ perfects in -*a*, -*tha*, -*e* as originally middle voice. Only within secondarily developed systems of oppositions did these desinences sometimes pass into the middle voice and it is frequent for them to also pass into active or indifferent paradigms here and there. We cannot, however, stop here. There is even less reason to consider these forms as original middle voice if, as Kuryłowicz, Watkins and others suggest, they are thought to spring from a 3rd. sing. derived in turn from a verbal adjective or a noun in -e/o: that is to say from neither active nor middle forms. On the other hand, this -e/o characterizes the indicative at the same time as certain persons, and in other instances the subjunctive. It is moreover this same -e/o which we call the thematic vowel in the noun; a sound principle, established by Meid (1974: 212), is that the parallel between noun and verb should be observed in the different stages of reconstructed Indo-European.

This is to say that the whole problem is to observe how an element of the nominal and verbal stems which is indifferent to later developed categories is used secondarily in order to express these latter. A very frequent case is that of a pure stem which, included in a paradigm with the same stem provided with disinences, was polarized in certain meanings. Certainly at times, the alternations were used for secondary formal differentiation; both in the noun and the verb, stems with the -o, -e and -ō degrees are used with different functions in each case. Besides, one should note the existence of amalgams in Indo-European from the start: for example, Gr. ἄγω indicates 1st. sing. present, active voice, non-imperative: and also of syncretisms: ἄγω is at the same time indicative and subjunctive. However, an amalgam does not always mean the fusion of independent morphs in a 1:1 ratio, neither does a syncretism always mean secondary confusion. These incomplete definitions arose from reasons of the system and are clarified by the context.

Basically (as I already discussed in detail as far as the *-s* is concerned: cf. Adrados 1971), the reluctance to accept an important development of categories and functions in the passage from Indo-European II to III is fundamentally due to the difficulties encountered in explaining how these new categories and functions developed formal markers; at that, formal markers which often have other values apart from the new one. To give an example, that *-s* should appear as a lengthening of the present with no apparent semantic value, and also as a marker of the desiderative, of the 2nd. person (and even of other persons), of the aorist and the subjunctive, does not appear easy to understand in many scholars' opinion. For some of them, the *-s* of the subjunctive is derived from that of the aorist, as we have seen; for others, that of the aorist from desinential *-s*. More often there is vacillation as to whether this is one or two morphs or a vague, imprecise meaning is suggested into which anything fits and of which no explanation is given as to how it could have evolved to give the meanings of a historical era (cf. abundant examples in Adrados 1971).

But we cannot continue to imagine an Indo-European, however old it might be, in which the categories and functions are always expressed by 1:1: ratios, the marker of each being totally different to the others (and not, as at times occurs, created with the help of lengthenings, etc.: *-s* as against *-ās*, *-is*, *-se/o*, etc.). There would be monosemic markers without amalgams, syncretisms and hardly any allomorphs. Such a language does not exist; Proto-Indo-European, at least, does not belong to this type (cf. Adrados 1968: 10 ff.).

On the contrary, the reconstruction in depth of a language such as Indo-European may be carried out thanks only to the fact that, either by comparison or within one and the same language, archaisms may be found which reveal older stages. These archaisms may refer, in the first place, to the non-existence here and there of recent categories and functions, that is the preservation in certain instances of an older stage. Secondly, that the markers of the new categories and functions are several and occasionally present traces of uses prior to the creation of same. This means that these categories and functions are, in effect, recent ones.

These two types of archaisms are to be studied here. They are what might give a general response to the problem of the archaic structure of Hittite; and also to whether Hittite formal

elements which coincide with those of categories outside Hittite are proof of their former existence in Hittite or not. For, if there are archaisms outside Hittite which prove the older existence of a system comparable to that of Hittite, the second hypothesis is the one which should be accepted. However, this of course implies a later task: that of explaining how these elements were grammaticalized in accordance with a theory which is merely outlined here (see for a detailed exposition Adrados 1974: 69 ff.), until they become markers of the new categories and functions. As I said, this task is not going to be undertaken here; I shall be content to touch upon the problem of the archaisms in general terms.

The Archaic Structure of Hittite: Proof by Means of the Archaisms in Other Indo-European Languages

We shall begin with the first of the two paths of research suggested: that outside Hittite there are traces of a grammatical system the same as, or similar to, that of this language and Anatolian in general.

The truth is that a good part of this research was carried out long ago and if it has not been used in favour of the thesis of the archaic structure of Hittite, I believe that this has on the whole been because of the prestige of the Brugmannian tradition and the difficulty of explaining the development of new categories and functions I mentioned above.

To give a very simple example, it is universally acknowledged that the most commonly used stems for expressing the masculine and feminine, that is, the stems in *-o* and *-ā* respectively, were originally neither masculine nor feminine (cf. Brugmann 1897). Any manual gives examples of feminine stems in *-o* and masculine ones in *-ā*. It is commonly accepted as a result, that the masculine/feminine opposition is more recent than that of animate/inanimate, the first term of which was later split in two. Therefore, the non-existence of the masc./fem. opposition in Anatolian is an archaism of which other languages preserve traces (cf. Laroche 1970 and Meid 1979: 165 ff.). Why, therefore, should one put forward the unfounded hypothesis that Anatolian first created this opposition and then lost it secondarily?

It would be easy to continue along these lines. Thus, however far Indo-European III developed polythematic inflexion in the

indicative, it here and there keeps partially to the monothematic system. However, before giving further details of those Indo-European archaisms which may serve as a guide in the reconstruction of the older stages of same, we must expound a theory of the archaism which is not always borne in mind in reconstruction.

Only too often, in truth, a relationship is established between languages on the strength of the existence of traits common to all of them, without taking into consideration that only innovations and choices from several possibilities offered by the parent language are proof of this relationship.

In an already old paper (Adrados 1952) on the prehistory of Greek dialects, I discussed this problem in detail. A common archaism does not imply any special linguistic relationship. That Hittite coincides in certain archaisms with IE western languages, for example, does not mean that it was originally a western language, as has at times been suggested. To give just one example: if Anatolian, Tocharian, Greek and western languages are *centum* languages, this does not imply any kinship between them but does imply a kinship between the innovating languages, the *satəm* ones. It is to be noted that this kinship existed when this isogloss developed but there may have been (and are) older or more modern isoglosses which imply different relationships at these moments.

We should reject a static model in the relationships between the Indo-European languages. Even the division into two branches postulated by the Indo-Hittite theory, may be considered somewhat inaccurate. What happens is that Anatolian, to a certain extent, preserves an older stage which evolved in the rest of Indo-European. At times, however, this older stage must be discovered under the Hittite known to us, which offers innovations. Moreover, this older stage is confirmed by the preservation of certain traces of same in the languages of non-Anatolian Indo-European. These archaisms may appear either in clusters or in isolation; and they may refer to several phases of Indo-European evolution. Any stage is mixed and the same goes for any language. It presents archaisms of several planes of antiquity with innovations or choices which are based on them and at the same time offers its own innovations, choices and characteristics, which one might term autonomous. Elements may also be found in a language which may be considered as a basis or starting point for the evolution of others, and there-

fore, from the point of view of the latter, may be considered as archaisms.

It is therefore necessary to start with a prior, objective judgment of the archaic or innovating nature of those traits common to several languages. One should not judge on the strength of a preconceived, aprioristic or traditional idea of what is the most archaic. For if we advocate that any *-s* in an indicative form was originally aoristic, as Eichner believes, for example, then any *-s* in Hittite indicative would be a relic of the old aorist. However, if in Hittite there is *-s* in:

- a) Present and preterite forms (imperfects).
- b) In certain endings of 2nd., 3rd., sing. above all.

and in the rest of Indo-European there is in:

- a) Presents and imperfects.
- b) In the same endings or persons as in Hittite, with similar distribution.
- c) In aorists and subjunctives.

one may easily draw the conclusion that the older forms are a) and b) and that c) is a newer one developed from a) and b). This is a conclusion which, for the rest, Meillet had already drawn in 1908 before the discovery of Hittite.

Coincidence in form — and, as we have stated, Meid had already pointed this out — does not imply that the function characteristic of Brugmannian Indo-European is an old one: it may well be an innovation. What should be observed is the process of evolution that normally consists of a contextually developed meaning eventually belonging to certain forms which, when opposed to others, in turn polarize these latter in the opposite sense (aorist/non-aorist, subjunctive/indicative, etc.). I have developed this theory elsewhere and have criticized the attempts to find one “original” meaning for each form, as likewise the inability of current theories to reconstruct linguistic models which are more than a type of shadow of those which were derived from them later on (cf. for example, Adrados 1965, 1968, 1971).

Many different types of archaism could be presented if the hypothesis is right that Indo-European I (pre-inflexional) was followed by II (monothematic inflexional), and this latter by III (polythematic inflexional); that within III the group III a) presents certain innovations, whilst III b) presents others; and that finally, within the polythematic type, complex inflexion is more recent than simple inflexion. Evolution does not

occur in a straight line or at uniform speed: archaisms may be present here and there, either combined or isolated. Even so, a few types may be noted:

a) Relics of type I in any language (forms without inflexion, sometimes included in inflexive paradigms and polarized in diverse meanings).

b) Relics of the monothematic type II, in languages of III (also with certain traits of the type, not only of monothematism).

c) Relics of the polythematic type III a), in its derivate bithematic III b) (the presence in it of old imperfects, aorists and perfects).

d) Relics of type III in III a) (lack of certain innovations or traces of their recent nature).

e) Within type III, relics of simple polythematic inflexion (for example, of the derivation of the subjunctive from the root and not from the so-called tense stems).

It is not, however, my intention to explore this panorama in detail here. I merely wish to recall in the way of examples certain archaisms outside Anatolian that show the fundamentally archaic nature of this linguistic type, which is very close to Indo-European II (although certain innovations cannot be denied). This is the proof needed to avoid aprioristic conclusions and to establish that a series of differences between Anatolian and non-Anatolian referred to above, must be explained on the assumption that the former is the more archaic language. That is, in fact, to justify the thesis that Anatolian and Hittite did not lose certain categories of Indo-European III, because they never had them as they were innovations of IE III. That is, all our examples refer to item b) above.

I do not doubt that some of them will require a lengthier explanation although this has really been given already by several linguists, including my own work. On the other hand, I shall disregard the question of the *-hi* conjugation and the perfect which is touched upon above and which would demand long explanations. Neither shall I take into account my laryngeal theory, expounded in Adrados (1961 and later works) which would offer a considerable increase in the number of examples.

The key to the problem is, as we know, in the occasional presence in IE III of the monothematic inflexion of IE II, preserved in Anatolian in general and especially in Hittite.

I shall not insist on the case of the noun: I have already noted a few points. I shall concentrate on the verb.

It is well-known that the same stem gave a present and a preterite in the oldest form of Indo-European, present and preterite being differentiated by two series of endings, and that in an even older phase the opposition of the *-ti/-t* type did not yet exist, therefore being an innovation. This is reflected in the existence of the imperfect, preserved in IE III a) and of which there are relics in III b), notably so in Celtic (cf. Rix 1977: 157 ff.). But it is mainly reflected in the fact that traces exist outside Hittite of the stage of this latter: non-opposition of the types present/aorist/perfect and indicative/subjunctive.

To begin with the first of these oppositions, verbs are occasionally found which have no other opposition in the indicative than that expressed by the endings. This is the case of Gr. *εἶμι* and Gr. *φημί* (*ἔφησα* is secondary). Within Greek itself, one should note cases in which the difference between present and aorist consists simply in that one of them is thematic and the other athematic, which denotes an obviously recent phenomenon of opposition (types *φύομαι/ἔφουν*, *χέω/ἔχεα*). We should add the case of the preterite-presents of Germanic and several other languages (Lat. *odi*, Gr. *μέμνημαι*), atemporal forms from one stem which received a preterite by means of recent devices (weak preterite in Germanic, past perfect in Greek); and also the case of the two stems of Baltic verbs, both presenting originally atemporal endings (on their archaic nature see Watkins 1970; also Adrados 1963:373 ff.). The preterite was certainly lengthened with *-eH*, but the frequent uniformity of vocalism in both stems inherits the older relationship present/imperfect; in other cases, a vowel alternation of a secondary origin was established (W.P. Schmid: 1966/67).

In other instances, one may indirectly deduce the older existence of one stem per verb; thus, in the assignment of other roots to fill in the table of categories of IE III, which is the case of verbs with several roots such as Gr. *εἶμι*, *ἦεω/ἦλθον* (Lat. *eo*, *ibam/fui*); it is also deduced from the existence in aorist stems of forms of an older imperfect, which is notably the case in Slavonic, see for example, Aesl. aor. 2nd. 3rd. sing. *moli* (1st. sing. *molišŭ*, from *moljŕ*); 2nd. 3rd. sing. *žna* (1st. *žnašŭ* from *žnajŕ*), etc., etc. The endings *-tŭ* or *-stŭ* (= Hitt. *-šta*) are sometimes added to these persons. Certainly when there is a

form in $-j\varrho$ or $-v\varrho$ in the present, one should postulate two stems from the point of view of Slavonic. I believe this to be a phonetic development, in accordance with my laryngeal theory (Adrados 1963: 301 ff.). The same thing occurs in Lat. *amās/amāsti* and comparable cases; here, too, my laryngeal theory postulates the original identity of *amā-/amāu-*.

But this theory is not needed to justify the thesis I now maintain: it simply offers many more examples in the most diverse languages. If we leave it to one side, we still find examples similar to the former ones as likewise many other arguments. Among these, there is the case that, if in practically all IE III there is documentary evidence of the older existence of an $-s-$ aorist, there are extremely abundant traces that this was a recent development, for the $-s-$ is frequently limited to certain persons and does not affect all of them. Meillet already observed this in a paper of 1908, which I have mentioned and which I studied in Adrados 1971, also mentioned. For the rest, it is a well-known fact: cf., for example, Burrow 1954, Bader 1974: 15.

Bader's paper on the other hand demonstrates to what extent at an older date the present/aorist opposition was effected with the aid of different lengthenings according to the roots. In fact, as is well-known, any stem on principle belongs to the present or aorist according to the position it takes in the system: the form itself does not determine anything. That is to say that there is not a present characteristic and another for the aorist in IE. Any present stem became an aorist stem thanks to the interplay of the oppositions. It is not that certain roots of the aorist or "telic" aspect were opposed to others of a different aspect as Cowgill advocates (1979); on the contrary, the aoristic aspect (and that of the present) was created on the opposition of stems each of which belonged originally to a monothematic verb, cf. in general terms, Meid 1977: 122 ff., 1979: 170 ff., and details of the explanation in my above-mentioned papers.

Really, certain $-s$ stems which for the rest have survived here and there, with no special meaning or with a secondary desiderative one, were used to oppose either persons (the $-s-$ thus becoming desinential) or tenses, the $-s-$ thus becoming aoristic. When this preterite and the formerly extant one, now an imperfect, were opposed, the category of aspect was created from certain semantic traits of stems which finished up in the

orist. That certain stems tended to be specialized for the orist (thus those in *-s-*) and others for the present (thus those in *-ske/o*), is a secondary fact, for it has exceptions. We have alluded to same as far as the *-s-* stems were concerned; and there is an *-ske/o* preterite in the Armenian orist and in Greek imperfects and orists (Negri 1976).

One should in fact stress that, although Indo-European tended at a given moment to assign one stem to each category and one category to each stem, this was a secondary process and was never totally accomplished. The system founded on the proportional definition of the categories in more frequent: a morph X has a certain meaning when joined with specific roots in specific oppositions; and another in different circumstances. This is quite enough to avoid ambiguity, although at times greater precision is achieved with the aid of lengthenings (*-is-*, *-sā*, etc.). The existence of those proportional markers of the categories is sufficient proof of the recent nature of these latter.

All I have stated above with regard to the present/orist opposition (I have disregarded the perfect) may be repeated approximately as far as the indicative/subjunctive is concerned. As is well-known, this opposition does not exist in Hittite, in which the so-called indicative has a double meaning according to context (on the inadequacy of an attempt to find traces of a subjunctive in Hitt. *-allu*, cf. Meid 1979: 171). Now, it is well-known, that a series of languages, Baltic and Slavonic above all, totally lack the subjunctive, whilst in others there are forms which are ambiguous between indicative and subjunctive. In Adrados 1963: 851 ff., the main details are pointed out, thus Gr. *ἄγω* indicative-subjunctive, and the same in Goth. *salbo*, *-os*, O.H.G. *habēs*, O.I. *dāti*, *prnāti*, in Tocharian examples, etc. Not only this, however, but it has been known since Renou 1925 that the Vedic thematic present has either an indicative or a subjunctive meaning, not to mention the injunctive, which Hoffman, in an important study of 1967, defines as a mood, the function of which is a purely denotative one; it is in fact a pretemporal and premodal form.

As in the case of the present/orist opposition, the formal traits of both indicative and subjunctive are the same: there was a splitting which produced a proportional marker. This has become quite clear now that we know the subjunctive system of Tocharian B, which uses practically all stems that at other times

belong to the indicative (Adrados 1963: 416 ff., before this Lane 1959). As regards the subjunctives in *-s-*, *-ā-* and *-ē-*, the most common in the other languages, it is clear that their formal markers were the same as those which were used in the aorist upon other occasions and yet again in the present. I do not wish to go into details here on my ideas of how the process took place; I only state that formal oppositions were used in order to achieve non-ambiguity between the aorist (of the indicative) and the subjunctive: I refer to my article of 1971, although Kuryłowicz's (1977: 76 ff.) and Meid's (1977: 120 ff.) new suggestions should be further dealt with. I believe that my former position may be upheld, but I prefer to leave the details for another context. The important thing is that here, too, there are proportional markers for the categories, which indicates the secondary origin of same.

With regard to the *-e/o* subjunctive, the same thing occurs. My identification of this marker with that of the indicative stem vowel in 1963, has now become generally accepted, cf. Kuryłowicz 1977: 90 ff., Meid 1979: 172 ff.; both resort, as I did, to opposite explanations. On the other hand (cf. Kuryłowicz 1964: 139), the *-ē* subjunctive is still explained as coming from *-e-e* (the subjunctive characteristic plus the thematic vowel). This is an error: there is not one sole subjunctive characteristic but a proportional definition of both moods with an inventory of morphs which is very extensive in Tocharian and very limited in the other languages. Among these morphs *-ē* is very frequent in the subjunctive as well as in the indicative.

Of course, all this presupposes that the subjunctive was originally derived from the root, the system by which it was joined to the different stems being secondary. But this is easy to prove with the aid of the traces of the old system to be found not only in Tocharian, Italic and Celtic, but also in Old Indian (cf. Vekardi 1955). Simple inflexion is, as is known, older than complex.

Similar things may be said of the imperative; whether it be called injunctive or not, it is universally acknowledged that it comes from the same stem as the indicative, mainly with the aid of the desinential system. Its assignment to the various stems is secondary, cf. on a Vedic type of imperatives in *-si*, Cardona 1965. But in this, Hittite had already reached approximately the same stage as was later kept in IE III. On the other hand, the optative is lacking in it and whatever its origin in

IE III, it is once more proof of the recent nature of the category that it is missing in several languages.

As may be observed, I have offered a rapid review of several traits of IE III which were still missing in Hittite and which were also absent here and there in IE III, or else were marked by obviously recent resorts. By so doing, I wished to advocate the idea that those categories of IE III which were lacking in Hittite were not lost in this latter: they had not yet arisen. Therefore, the formal coincidences between certain Hittite morphs, and others which in IE III mark the above-mentioned categories, should be interpreted in the sense that these morphs were used in a later phase of Indo-European, in III as we have stated, to mark the new categories than created. With the aid of same and in certain oppositions, semantic-grammatical phenomena were thus grammaticalized which at the beginning were purely contextual.

All this does not of course imply that Anatolian, and within it Hittite, was unable to develop certain innovations: for example, those of verbal inflexion (*-i* outside the present, etc.) are quite clear. Neither does this mean that there were no more archaisms, likewise more or less present in IE III, which represented traces of a yet older linguistic stage than that of Anatolian: a stage which did not oppose tenses, moods or, partly, persons, which had only hesitantly begun to oppose singular and plural and to establish a case system. They do certainly exist and relate Hittite to either one or another of the Indo-European III languages. For an archaism may arise here and there without demonstrating any special kinship. But it was not this type of archaism which concerned us here, merely that which is opposed, as a relic of a previous stage, to innovations common to the majority of the IE III languages; not to all of them, for the preservation in certain instances of an archaism which Hittite kept more consistently proves that this latter did not lose those traits we refer to. They were created by IE III, at a date in which the isoglosses in question were unable to reach Anatolian which, according to the interpretation I have suggested (Adrados 1979), had, to the south of the Caucasus lost contact with the rest of Indo-European.

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