

A Panorama of Indo-European Linguistics since the Middle of the Twentieth Century: Advances and Immobilism

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Some advances in Indo-European linguistics.

Since 1948 I have been writing on Indo-European linguistics, my *Estudios sobre las laringales indoeuropeas* dates from 1961. I have subsequently continued writing (lately somewhat less), I hope to publish a book, *Historia de las lenguas de Europa*, in 2007. And I have read very extensively and attended innumerable symposiums. Therefore, I feel I can offer a perspective on the development of studies in Indo-European linguistics during this period, from my own point of view, naturally—a point of view that has been exposed in various articles published in this same journal.

Indo-European is, after all, the origin of our languages; it is the oldest form of Spanish, French, Russian, English, German, and also Hindi, in spite of the fact that nowadays this is widely unknown by the general public. Here I want to say a few things about progress and failure, advances and immobilism in Indo-European studies.

There have, of course, been advances. To begin with, enormous progress has been made in the study of the Indo-European languages discovered just before or during this period: Hittite and other Anatolian languages, Tocharian, Mycenaean Greek, and Celtiberian, among others. When a new language is discovered and deciphered and a well-tested method of investigation is applied, new results are obtained. A different matter is using these results to bring our knowledge of the history of Indo-European up to date. Concerning advances in this matter I am more skeptical (see below).

And there are some new manuals that are good: those of

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Szémerenyi (1970), Georgiev (1981), Beekes (1984), Ramat (1993) and Meier-Brügger (2000), among others, all very traditional, to be sure. I will take the liberty of adding the one I wrote in collaboration with A. Bernabé and J. Mendoza (1995-1998), based on a very different point of view: it does not describe, as the others do, «one» Indo-European, that of the traditional reconstruction, but rather three successive Indo-Europeans: the pre-inflectional (I), the monothematic (II) and the polythematic (III), this last really corresponding to the traditional reconstruction. On this, see below.

And there have been advances, which I consider decisive, regarding the origin of the Indo-European people, their culture and the successive stages of their development, though I am aware of the fact that there continue to be supporters of the hypothesis that places the origin or point of departure of these people in the plains of Germany and Poland. But I consider correct the thesis of Gimbutas and others that puts their origin in the Kurgan culture, in the plains that extend from the Dniester to the Aral Sea and beyond. Between the fifth and third millennia B.C., successive waves of peoples and languages went forth toward Europe and Iran, then southward to the Mediterranean Sea and the Indian Ocean.

Regarding my own position on this topic, I can only refer to my previous works, especially Adrados 1979 and 1982. They combine linguistic and archaeological data that allow us to establish a chronology for the evolution of Indo-European.

The Indo-Europeans, according to this thesis, had destroyed the agrarian culture of the Balkans (Gimbutas's culture of «Old Europe») and had then spread out in various waves. As I see it, the polythematic Indo-European III is not the single, original Indo-European, but that of the invasions that took place shortly before the year 2000 BC: the invasions that produced, among others, the languages of the Greeks and the Indo-Iranians, who emigrated in two different directions. And then there was another, more recent, migration, around the year 1000 BC: that of the other European languages, which probably entered Europe from the north of the Balkans, while the Thracians, Greeks and perhaps other peoples must

have entered earlier through the south of the Balkans.

I have made the distinction, then, between Indo-European III A, the most morphologized one, the origin of Greek and Indo-Iranian, and Indo-European III B, the origin of the European languages in general, which combined the preterites into a single one and presented various archaisms. One group, lost in the steppes, separated from this second branch and, traveling eastward, penetrated into the Xinjiang region, producing the Tocharian dialects.

This is not, to be sure, the only theory. Widely debated but generally rejected is C. Renfrew's (1987), according to which the expansion of the Indo-Europeans is synonymous with the expansion of the agrarian culture beginning in the Anatolia of the seventh millennium BC. And there is also Gamkrelidze's theory (1993), which situates the Indo-Europeans in northern Mesopotamia, near the Caucasus. These theories, widely circulated, are generally rejected, because, among other reasons, they completely lack any linguistic basis. I discuss this in my *Historia de las lenguas de Europa* (in press), where I give the pertinent bibliography.¹ Likewise, the old theories situating the origin of these peoples in the plains of Europe are rejected today; nevertheless, they have been renewed by Bosch-Gimpera (1960), Kilian (1983), Häusler (1995) and various others.

In my opinion, the thesis of the Indo-Europeans' successive invasions, from the steppes of Central Asia and north of the Black Sea in various stages, which has certain earlier precedents, is a decisive gain. Nevertheless, as regards the Indo-European culture, the new expositions differ little from the old ones when they describe the Indo-Europeans as nomads with a tribal organization who knew the horse and had copper or bronze but not iron. In this we are more or less where we were before. We do, though, reject the old implantation of the Indo-Europeans in the areas of the beech, the salmon and the birch, that is, in Europe.²

¹Included there is de Hoz 1992 and my review of the Gamkrelidze-Ivanov book in *Emerita* 65, 1997, 139-141.

²Cf., for example, Villar 1991: 32 ff.; Beekes 1984: 47 ff.; Martinet 1997: 52 ff.

There has been, I believe, an advance in fixing the area of the primitive settlements of the Indo-Europeans and in the chronology and the areas of their expansion. Also with regard to the «neighbors» of the Indo-Europeans and their linguistic kinship, especially with the Finno-Ugrians, who also made their way to the west around 5000 BC. In Asia, at one time, Indo-European was in contact with the Altaic languages and with those of the Caucasus, and later, in northern Syria, with the Hamito-Semitic languages. A great deal has been written on the languages that, as a group, are called Nostratic.³

And also to be considered an advance, I believe, is the study of the hydronymy and toponymy of Europe, beginning with H. Krahe, which demonstrates the ancient occupation of Europe by Indo-European peoples at a date earlier than that of the classic great families of Indo-European, although we cannot say much about their morphology.⁴

There have been advances in all this and also in more specific areas. The great advance in phonology is, in my opinion, the general acceptance of the existence in the oldest Indo-European, of three laryngeal phonemes with the respective timbres of *e*, *a* and *o*, symbolized by H_1 , H_2 and H_3 (also with lower case *h*). As is known, these «sonant coefficients» were identified in theory by Saussure in 1879, and Kurylowicz, in 1927, identified them with the h of Hittite. This would be an inheritance of the oldest Indo-European, eliminated subsequently by the later one. In this way, Hittite was recognized as more archaic than the other Indo-European languages, at least in this phonological feature—but not in the case of the laryngeals with appendix, which I will discuss later, nor in morphology in general. I will discuss this also.

Naturally these are not the only advances that have been achieved: there are many details to be considered. And the same is true of the development of writing among the Indo-European peoples, which was always a loan from other cultures,

³See, among other works, Cowgill 1986; Greenberg 2000; Dolgopolsky 1998; Moreno Cabrera 2003: 1205 ff.

⁴See, among other works, Krahe 1962; Tovar 1977; de Hoz 1963; Villar 1991: 91 ff.

through Greek.

But above all, the greatest advances have been made in the individual languages, both those recently discovered and those long known. There is an infinite bibliography on Greek and its dialects or on Balto-Slavic or Celtic or Italic and its different languages or Armenian or Indo-Iranian. And on minor languages, from Messapic to Venetic, Rhetian, Macedonian and so many others.

More than in Indo-European studies in general, progress is manifested in the study of different Indo-European languages. The Indo-Europeanists tend increasingly to specialize in this or that language (or languages). This makes progress possible, of course, but it also implies a limitation, because we are talking about comparative linguistics, and this requires a knowledge both of the specific languages and of their relations within a common history.

And even so, there are still those who maintain the traditional scheme of the reconstruction of Indo-European as a single language that later split into various branches. This view often renders useless the new contributions to the history of Indo-European that might be obtained from the knowledge of the various branches, which are sometimes forced to fit into a traditional scheme. This is the immobilism that appears in the title of this article and that frequently makes it impossible to study the linguistic history of Indo-European in depth.

Excursus on the laryngeals with appendix

Permit me to introduce here several pages on my proposal (developing earlier ones by Martinet and Diver) that in the earliest Indo-European the three laryngeals which I have mentioned, preserved in Hittite (although here they sometimes appear geminated or have been lost and differences of timbre no longer appear), are actually derived from series of laryngeals either with a labial appendix (H_1^o , H_2^o , H_3^o) or a palatal appendix (H_1^p , H_2^p , H_3^p).

I made this proposal in my 1961 book on the Indo-European laryngeals, which I have mentioned. I feel that this book offers important insights on the vocalization of sonants and laryngeals in various Indo-European languages, as well as

on the origin, also in different Indo-European languages, of suffixes and formative elements in *-w* or *-u* and in *-y* or *-i*. The book was preceded by various articles of mine on the vocalization of the sonants (later included in the 2nd edition of the book, in 1973).

It is precisely these vocalizations of the sonants that are the focus of the completely negative review of my book by G. Cardona⁵, which was, apparently, the main source of information on it.⁶ My subsequent explanations, in the “Prologue” to the 2nd edition of the book (Adrados 1973b: XIII f.), as well as the inclusion in this edition (1973b: 357 ff.) of various essays of mine on the subject, with abundant bibliography, and the publication, later, of several more papers that advanced the topic further⁷ were all in vain.

And when I published a condensed version of my ideas in German (Adrados 1994a), this same phenomenon was repeated in the form of a review by C. Melchert⁸ in which it was clear that they had not been understood at all. It seems that this review became the sole source of information (or rather of misinformation) regarding my ideas⁹.

As a result, they are still absolutely unknown. I am not going to give résumés of them here; in just a few pages (of the several hundred I have written) I only aspire to awaken interest so that someone will decide to look at the facts directly. But Cardona insisted that I did not say in what precise circumstances each phonetic result occurs—and therefore I was breaking phonetic law.

But phonetic law is nothing more than the generalization of the majority results, the triumph of a

⁵*Language* 39, 1963, 91-100.

⁶See also Mayrhofer 2004: 35. There is also a critique by Melchert of my ideas, which I will mention.

⁷Especially Adrados 1980; 1981a; 1981b; 1984 (all of them included, in Spanish, in my Adrados 1988).

⁸Published in *Kratylos* 42, 1997, 170-171. In view of the fact that this periodical did not accept my reply for publication, I mailed a copy to the members of the Indogermanische Gesellschaft, with the title “An answer to an unfair review.” I provide an extract of this reply below.

⁹Likewise in the case of Mayrhofer 2004: 39.

solution alongside which, for reasons of evolution, phonetic contagion, or optional change of the place of vocalization, there could be others. All this has been commented on by many phoneticians, many Indo-Europeanists, who have given bibliography and examples. But for the high priests of Phonetic Law, this was perverse.

Cardona's criticism was clear: it was a simple allegation against my book because it broke the sacrosanct phonetic laws. I proposed, and others have done the same, that in various languages the sonants, on vocalizing, sometimes produce anomalous timbres taken from the consonants they are in contact with (*υρ*, for example, after a guttural, in Greek, instead of *αρ*; or the neutral timbre *ar* instead of *or* or *ur* in Latin and Germanic); that in a number of languages there are vacillations between *tro* and *ʎro* > *toro* (in Spanish we can occasionally find *corónica* for *crónica*, *Ingalaterra* for *Inglaterra*); that there are likewise vacillations between *tara* and *trā*: that in Hittite, where the laryngeal was disappearing but was still producing occasional geminations, there are vacillations *h* / *hh* / \emptyset .

This doctrine was condemned to remain unknown, as were proposals of morphologization that are, I believe, important for the origins of Indo-European morphology: for example, for the origin of inflections like *χρός* / *χροφός* in Greek, *dyām* / *divás* in Old Indic, of the relation between Greek *γάλως* and Old Slavic *zūly*, all this in the noun. In the verb, for the origin of perfects in *-u*, like the Greek *τεθνηφός* together with *τέθνηκα*, or the Latin *amau-* as opposed to *amā-*; of the verbal themes in *-ye/o* (Latin *mone(y)o* beside *monēs*). And a thousand other things, which can be read in my books.

At least Cardona's review was based on a general principle that I contravened to a certain extent, justifying my position, of course. Melchert's is made up of small specific attacks with no comprehensive vision. «Individual words or word-classes are discussed atomistically, with little or no regard to the larger context of which they are part», he said. «That is exactly the opposite of the truth», I replied. «It is a coherent system. Each phonetic result has many parallels within the adequate

contexts».

Melchert points out examples in which a word has *w* in one language and *y* in another; but there are suffixes and desinences that extended beyond the place where they originated, a situation I would be the last to deny. Then he argues about something that is a misprint. And he says that «the purpose of this book remains obscure to me». «But the purpose was perfectly clear», I replied, «namely, to offer, in German, a brief and clear exposition of a system previously presented principally in Spanish and therefore perhaps less accessible or comprehensible. Prejudice or the lack of information», I concluded, «has kept the reviewer from any understanding of the book at all».

I must admit that reviews like these two were quite demoralizing for me. I realized that I was making proposals that required study and criticism. But there was no study and no criticism, a result of a perspective that focused on its own tradition and rejected on principle any new viewpoints.

I limited, or almost desisted for some time in my publications on Indo-European; other studies attracted me more. But now I am returning in order to at least make my viewpoints known. They are presented more extensively in my *Manual de Lingüística Indoeuropea* of 1995-98 (in collaboration), which will now appear in English. This will be an opportunity for these ideas to be known directly and not hidden behind reviews like the two mentioned—which did indeed receive wide dissemination! But it was gratifying at the time to see that the new ideas in morphology that I and others proposed were being considered, although, as I have recounted, there was a terrible immobilist reaction.

Immobilism and advances in the history of Indo-European morphology

Really, to imagine a static, unitary Indo-European language that only disintegrated when the diverse linguistic families were created (be it around 2000 BC or around 1000 BC or even later) is to imagine the impossible. A unified and relatively static language can only exist, and that partially, if there are a unified culture, state and society in a period already

alphabetized and literary. Otherwise, languages exist in a state of geographical, dialectal and evolutionary dispersion.

But the Indo-Europeanists, ever since the studies of Schlegel, who so admired the culture of India, including Sanskrit, the «perfect language», and those of Bopp, have attempted to reconstruct a unitary language, with a maximum morphology, which should classify everything within its categories and functions. And in spite of this, Bopp¹⁰ still speaks of the growth of Indo-European and the origin of certain forms based on agglutinations.

It was Bruggman who insisted on the reconstruction of a perfect and unified language—the old Indo-European. He projected onto it the maximum of categories and functions of the various languages, especially of Sanskrit and Greek (which sometimes goes beyond Sanskrit in its innovations). Every suffix, every desinence, according to him and many of his followers, had a single original value. For example, the case endings in the noun or the desinences of person in the verb, and the formative elements and suffixes.

This, in the long run, creates terrible problems: how is it that -s in the noun can function sometimes as nominative and sometimes as genitive? And in the verb, sometimes as 2nd person or as 3rd person singular? Or how is it that, in the case of the formative elements, the thematic vowel *e / o* can mark sometimes the indicative and sometimes the subjunctive? Or that the -s can be used for the desiderative, or the aorist or the subjunctive or the future? This can only be explained by various developments in different contexts; there are no various -s's in IE, each with a different meaning.

But this was the model that was being offered. And if in the languages derived from Indo-European there was lacking one of the proposed elements of reconstruction (the perfect or the subjunctive, for example), the explanation was that it had been lost. Evolution was, in short, corruption; the perfect state, the linguistic paradise, was the original state: the Indo-European reconstructed in this way.

This way of thinking, implicit to a certain extent, was

¹⁰ See Adrados 1994b.

what kept the newly discovered languages, especially Hittite but also Tocharian and, I would add, Etruscan,¹¹ from being used to redraw the history of Indo-European. Hittite, deciphered in 1917 by Hrozný, was, as I have written (Adrados 1994b: 11), like “the member of the family who turned up at a family gathering at the wrong moment and was therefore unwelcome”. It had no feminine, no comparative, no aorist, no perfect, no subjunctive, no optative! It was decreed that they had all been lost.

This was the opinion, dating from the twenties of the past century, of Pedersen, Pisani and Bonfante. In 1937, in the new edition of his well-known manual, A. Meillet himself said that “the Hittite deciphered by M. Hrozný does not require us to change anything essential in the doctrines presented here”. And certain linguists like H. Eichner (1975), E. Risch (1975) and more recently J.H. Jasanoff (2003), have gone to great lengths to discover in Hittite vestiges of these forms that were supposedly lost.

I have criticized this again and again, and will do so in my *Historia de las lenguas de Europa*, where I give the bibliography and the arguments. Other authors, as we shall see, either simply do not mention the information on Hittite or declare apodictically that Hittite «has lost» the forms I refer to.

In other words, Hittite, which contributes so much, has been practically unused, and nevertheless, before its decipherment and also afterwards, there were many linguists who, like Meillet or Specht, spoke of the recent character of the feminine, of the thematic vowel or of the inflection of the noun, for example. All this has been confirmed by Hittite. Later there have been many others, like Brosmann, Carruba, Fairbanks, Lehmann, Schmalstieg, etc.,¹² who have continued in that same line, as have I.

In 1935, E. Benveniste spoke of the monosyllabic Indo-European root, of its lengthenings, of the use of roots and

¹¹On Etruscan see, after other works, Adrados 2005b. Tocharian should be taken into account for the history of the creation of the subjunctive; see Adrados 1974: 416 ff.

¹²For details see my *Las lenguas de Europa*, in press.

pure themes in inflection. Actually, all those who have written about the development of inflection, and there have been many, have postulated the early existence of a non-inflected Indo-European, earlier even than Anatolian. But Anatolian preserves numerous vestiges of forms with no desinence in its inflection. I have written about this on many occasions.¹³

I have no choice but to bring myself into this story now, in order to then go on with it. Beginning in the fifties and for many years I taught Indo-European Linguistics at the University of Madrid (later called the Universidad Complutense de Madrid). My guide at the beginning was mainly A. Meillet's *Introduction*, an excellent book, as is well known. But at a certain moment, particularly when I learned about Hittite, I felt that what this language had to offer for the reconstruction of Indo-European was not being taken into account.

I found especially disappointing the work of J. Kuryłowicz (1958), where, following Pedersen and others, he posited that the categories of the traditional reconstruction of Indo-European that were missing in Hittite had simply been lost in that language. I have already said that Meillet himself affirmed this. But they all depended on a tradition that required them to express themselves that way. I had no tradition whatsoever behind me, I looked at things directly, with new, unclouded eyes, and this way of looking led me to think that it was the other Indo-European languages that had created new categories.

On the other hand, I was reading, in articles by Meillet himself, in Hirt, in Specht, in Benveniste, in others, things about the recent character of many elements of the traditional reconstruction of Indo-European: the feminine, the thematic vowel, the sigmatic aorist, the perfect, the future, the entire nominal inflection. Hittite, for me, confirmed all this.

And it added something more: the fact that the preservation in Hittite of the Indo-European laryngeals proved that it was an archaic language. No one was denying this by

¹³Cf., for example, Adrados 1988b. And many other publications, for example, Adrados - Bernabé - Mendoza 1996.

then. Starting from there, I wrote my *Estudios sobre las laringales indoeuropeas* (Adrados 1961), with considerable audacity, I have to admit. I was challenging doctrines accepted by everyone and I was doing this from a position with no tradition in these matters.

But this book took me in the same direction: toward the recent character of a series of suffixes and elements of Indo-European: *-ā*, *-ā* and *-ei* in the noun, *-ye/o* and the perfect participles in the verb, etc. My point of departure was my previous studies on the vocalization of the sonants¹⁴ and studies of mine on the phenomenon of grammaticalization: conversion of lengthenings and various suffixes in morphological characteristics, this in the context of various oppositions.¹⁵

But the laryngeals that I proposed were not the decisive factor in my new proposals for the reconstruction of Indo-European; I will return to this later. The case is that structural consideration was not popular among the Indo-Europeanists, who felt, in general, that the different morphological elements came from an older phase and had not changed their meaning since then. I proposed, instead, that several of them, whatever their origin, could have acquired the grammatical values that we know only in the context of new oppositions that were created, especially those elements that did not exist yet in Hittite.

It was a very risky position to take, as the future proved: it clashed with a venerable tradition. But there was a moment that encouraged me: when I held in my hands E. H. Sturtevant's little book (1942). He reached the obvious conclusion from the preservation of the laryngeals in Hittite: that it was an archaic Indo-European language. For Sturtevant there were Hittite and Indo-European, «two sister languages» derived from an ancient Indo-Hittite.

¹⁴Later included in the 2nd ed. of the book, now entitled *Estudios sobre las sonantes and laringales indoeuropeas* (Adrados 1973).

¹⁵Adrados 1962a, among other works. See later, with reference to Indo-European, Adrados 1965 and 1968. Of a later date, and related to my laryngeal theory is Adrados 1981a, among other works.

Sturtevant did not mention anything except the laryngeals, but it was a beginning. However then, in 1946, Kerns and Schwartz (1946) proposed a hypothesis, in brief and rather confusing form: the existence of various opposing themes in the verb of the traditional reconstruction could be an innovation with respect to the monothematic conjugation of Hittite.

I developed this thesis further in an essay published in 1962 (Adrados 1962b), which included a paper given by me in the *Fachagung* of the *Indogermanische Gesellschaft* held in Innsbruck in 1961. I presented more extensively and explicitly the thesis of two phases of Indo-European: one represented, for us, by Anatolian, in which inflection (not only verbal but also nominal) was monothematic; the other, more recent, being the one described in the traditional reconstruction, from which were derived Indo-Iranian, Greek, Germanic, etc., etc. In this latter phase, at times, various themes with different grammatical meanings were opposed: the feminine, the grades of comparison of the adjective, the themes of the aorist, the perfect, the future, the subjunctive and the optative in the verb. In some cases not all the verbs were affected; and the heteroclitic inflection of the noun and the coupling of a base verb and another verb with a specific *Aktionsart* were antecedents of polythematism in Hittite.

All this was quite bold when expressed by people who were not included in the strict circle of traditional Indo-European scholars. My case was worse because I was writing, with that one brief exception, in Spanish, a language scarcely read in Germany. And I spoke of structuralism, which was not very popular. But all considered, I must admit that the German journals devoted to Indo-European studies later published my articles both in German and in English.

I don't include the extensive bibliography¹⁶ here. I will only make specific reference to the two books, both in Spanish, in which the theory was first launched: my *Evolución y*

¹⁶The most important works can be found in *Historia de las lenguas de Europa* and "Must we again postulate a unitary and uniform Indo-European?", both in press.

estructura del verbo indoeuropeo (Adrados 1963, 1974) and *Lingüística indoeuropea* (Adrados 1975). The bibliography mentioned, whether in Spanish, German, English or French, defended and expanded the original theory.

These books and articles were very explicit with regard to the morphology of Hittite and of Anatolian in general: these would be not a new phase of Indo-European, but a derivation of a former phase of Indo-European, a new phase prior to the known one, since they have innovations of their own. This is what I call IE II (IE I, which I arrived at later, is the pre-inflectional or Proto-Indo-European, PIE). From IE II a second branch would stem: IE III, that of the traditional reconstruction. In the books and articles cited I explained in detail how they were created through the organization, within a single inflection, of apparently independent themes.

These books, little read outside Spain, received scarce attention, and the articles published in English or German in various journals not very much either, since the books mentioned, on which they were based, were hardly known and received, in the best of cases, incomprehensive reviews. As occurred with my *Laringales*, this impaired the diffusion of my ideas on Indo-European morphology.

And nevertheless, there came a moment when these ideas began to be known, partly, in spite of everything, because of the influence of my books, and also because the facts are absolutely clear to anyone who considers them without prejudice. If the feminine, the aorist, the subjunctive, etc. presented vestiges of recent morphologization, why must we suppose that their absence in Hittite was the result of having been lost? These categories were simply more recent than Hittite.

But there was an episode in which I found myself unintentionally implicated. Beginning at a certain moment in 1975, there was an extensive movement in Germany supporting the thesis of the recent character of polythematic inflection (in the noun, the adjective and the verb). In an article published in 1992 in *Indogermanische Forschungen* (Adrados 1992), I gave a relatively detailed version of this movement in favor of the archaism of Hittite and of the

recent character of polythematism in the Indo-European of the traditional reconstruction.

I was not the only factor in this evolution in the thinking of the Indo-Europeanists; the facts, as I have already said, were clear in themselves. But it was very disappointing for me to see that frequently the hypothesis was attributed to W. Meid and E. Neu, very distinguished Indo-Europeanists, of whom, they said, I was a follower. This is what K. Strunk wrote, for example, in 1984, E. Polomé in 1985, W. P. Lehmann in 1987, J. Tischler in 1989 and J. H. Jasanoff in 2003.¹⁷ I refuted this in my article in *IF* in 1992, but to no avail. The facts are as follows:

The point of diffusion of the new ideas was an article by W. Meid in 1975. It did not cite me, which was the origin of the error, compounded by the fact that in 1982, I had published a book in German (Adrados 1982) with almost the same title, which was a translation of a 1979 article in Spanish (Adrados 1979). The doctrine was very similar and it was believed, without giving it further thought, that as these works came after Meid's, they were based on it.

But this is not the case; quite the contrary. Meid had read my much earlier 1963 book, *Evolución y estructura del verbo indoeuropeo*, which I have mentioned. Meid was familiar with it and had written a review of it in *IF* 70, 1965, pages 346 to 350, an unfavorable review, to be sure. It would seem that he later changed his ideas and followed me. Indeed, his article of 1975 follows the ideas presented in my book of 1963.

These are the facts and they are the reason why what was really an «Adrados hypothesis», with certain precedents, became a «Meid» or «Meid-Neu hypothesis». Not providing bibliography and not giving adequate attention to it are the cause of “lapsus” like this one.

In the article mentioned, «The New Image ...», I described in certain detail, as I said, the diffusion of this theory after the appearance of Meid's 1975 article. As I said there, it is very difficult to decide in each case if the ideas asserting the recent character of traditional IE come from me

¹⁷Cf. also Rieken 1999: 505.

or from Meid, or if they simply come from a dispassionate study of the facts. Certain of Kurylowicz's and Watkins's ideas on the Indo-European verb can easily come from me, as well as from Meid. I gave Kurylowicz (who never cited anyone) a copy of the *Verbo Indoeuropeo*; a student of his told me once that it was in the Library of the University of Krakow, heavily annotated by Kurylowicz. Watkins cites the *Verbo Indoeuropeo* explicitly, though with errors. On the other hand, authors such as O. Carruba and B. Barschel do not cite the *Verbo Indoeuropeo*, but they do cite the article "Hethitisch und Indogermanisch". The details can be read in the article mentioned.

Nevertheless, alongside the unpleasant inaccuracy which I have just mentioned, there was at the time one pleasing thing: the ever-increasing diffusion of the theory of the strata of Indo-European. In «The New Image ...» can be seen the long, though incomplete, list of the Indo-Europeanists who were adhering to the new theory. Neu is one of them, but there are many others. I also make reference there to those who did not even mention the theory or argued against it, adducing supposed vestiges of the feminine or the aorist, for example, in Hittite. In short, they continued to adhere to the old tradition.

The new immobilism since the nineties

Now, unexpectedly, from the nineties on we have the decline of the theory which proposes the two strata of Indo-European, the monothematic and the polythematic. There is one exception: the three laryngeals H_1 , H_2 and H_3 are generally accepted with their descendents h and hh in Hittite. But even so, what the manuals include most frequently is the traditional reconstruction of Indo-European: the archaism of Hittite and even Hittite itself are hardly mentioned at all, or if they are mentioned, it is considered that the categories that are missing «have been lost». Of course there are exceptions, like W. P. Lehmann, who continues not to give credit to the Brugmannian reconstruction, which he calls «a kind of storehouse» (1993: 28 ff.).

But from the seventies on there has been a series of manuals on Indo-European like those by O. Szemerényi

(1970) and V. Georgiev (1981), and later R. Schmitt-Brandt's (1998), in which the only possibility mentioned is the traditional scheme of Indo-European. In other cases there is some vacillation, but in the end the theory of the archaism of Hittite is not accepted. This is the case of A. Giacalone Ramat and P. Ramat (eds.), *Le lingue Indo-europee* (1993): no conclusion is reached, but the general presentation by C. Watkins and the exposition of Anatolian by S. Luraghi follow the traditional lines, with some hesitation as regards Anatolian.

The theme is barely touched on in Warren Cowgill's «Einleitung» to volume I of his and M. Mayrhofer's *Indogermanische Grammatik* (1986): the traditional unitary and uniform Indo-European is taken for granted. This was also the position of J. H. Greenberg (2000), and, I should add, that of H. Rix in a recent work on the lexicon of the Indo-European verbs (2001). He presents the primary verbal themes (present, perfect, aorist, causative-iterative, desiderative, intensive and essive) and the modals as characteristic of all the Indo-European languages. He accepts, then, with no explanation given, that they also existed in Anatolian. In the end, the most he does is to mention at times the two hypotheses: the loss of categories in Hittite and its archaism, leaving the subject undecided.¹⁸

All of this represents a backward step. Only as regards phonetics is it recognized that Hittite comes from a stage older than the rest of Indo-European, a stage in which the three classical laryngeals still existed.¹⁹ A curious imbalance that I have already noted: Hittite could be archaic in phonology but not in morphology. Of course, there is no attempt at serious criticism of my theory of the laryngeals with labial and palatal appendices, merely unfavorable references made in passing.²⁰ But I do not want to go into this here; it has

¹⁸This also occurs in Mallory-Adams 1997: s.u. Anatolian Languages.

¹⁹Thus in works such as Bammesberger 1988; Mayrhofer 1986; Lindeman 1997; Mayrhofer 2004; Kimball, 1997.

²⁰Mayrhofer 2004: 35, where it simply makes reference to a critique by G. Cardona in *Language* 39, 1963, 91 ff. Melchert, *Kratylos* 42, 1997, 170 ff, does a critique, which I consider partial, of my work *Laryngale mit Appendix?* (Adrados 1994a). I replied with a commentary (unpublished): «An answer to an unfair

been discussed above.

To return to morphology, new publications even affirm that the grammatical categories that do not exist in Hittite «have been lost». Thus the manual of R. P. Beekes, who says (1984: 31), speaking of Hittite Morphology, that «its simplicity does not suggest antiquity, but could rather be owing to loss». Pure assumption, with no attempt at proof.

And C. Melchert (1994: 122) affirms that «the evidence for a common prehistoric development (of the Anatolian languages) does not settle the issue of whether the rest of the Indo-European languages underwent a period of common development». He presents Anatolian as a subgroup of Indo-European, alongside Germanic, Slavic, etc. All of this is mere personal assumption with no arguments as proof.

Let me cite finally the book by S. Zielfelder (2002) on the theme. She vacillates, declaring only the feminine as truly recent.

This is the recession to which I alluded above: by way of mere asseverations with no attempt at proof, once again it is affirmed, as it was in the time of Pedersen, Kurylowicz and others, that Anatolian and, within it Hittite, had lost categories that appear in the other Indo-European languages. Categories that many had maintained—and we continue to maintain—were created secondarily by the IE III that I have been discussing. It is the heir to IE II and conserves vestiges of it, as I have said.²¹

The only one, as far as I know, who has recently tried to demonstrate this “loss” of categories (the feminine, the comparative and the various themes of the verb) within Anatolian is J. H. Jasanoff,²² who in 2003 attempted to demonstrate the same thing that H. Eichner and E. Risch tried in 1975. I do not think that he really provides any proof

review», to which I have already referred.

²¹Nevertheless, there continue to be published works that sustain the idea of the secondary creation, in the later Indo-European, of categories lacking in Hittite. See, for example, on gender Matasovic 2005 (and my review in *Emerita* 74, 2006, pp. 169-170).

²²Jasanoff 2005. This is a continuation of another work, Jasanoff 1994.

whatsoever.²³

This, unfortunately, is the immobilism that, with the pertinent exceptions, is what we have today. We have returned to the traditional descriptions of Indo-European, with no arguments or with arguments that are absolutely insufficient.

This is not always the case, of course. The immobilist reaction is not complete. For example one can find affirmations saying that the augment was a recent innovation of Greek, Indo-Iranian and Armenian (Meier-Brügger 2000: 166), that the subjunctive and optative come after the separation of Hittite (Meier-Brügger 2000: 170), that the sigmatic aorist is also recent (Drinka 2005), and that the opposition masculine/feminine is recent as well (Matasovicz 2005; Zieffelder 2002). On Hittite archaisms in nominal inflexion, cf. E. Rieken (1999: 505), who proposes «eine frühzeitliche Abspaltung des Anatolischen».

Much more bibliography could be cited. Nevertheless today the doctrines most widely disseminated point in the other direction.

On Proto-Indo-European

This immobilism extends to the subject of Proto-Indo-European, because, as I have already said, all the hypotheses on the recent character of the inflection in Indo-European (in IE II and III) point, in the end, to one conclusion: that in its earliest stage, Indo-European (IE I or PIE) was not inflectional. Pure roots or pure themes were organized in groups, in phrases, by means of various procedures, not by inflection. And sometimes, in later Indo-European (IE II and even in III), pure roots or themes appeared in the inflections of the noun (nominative singular in $-\check{a}$, or nominative plural in $-\check{a}$, dative singular in $-ei$, etc.) or of the pronoun (accusative singular of the personal pronoun of the type, for example, accusative singular Greek $\mu\epsilon$), or of the verb (thematic 1st singular, present indicative or subjunctive, in $-\bar{o}$, 2nd singular

²³On this see my article in *IF* (in press: “Must we again postulate a unitary and uniform Indo-European?”) and my arguments therein.

imperative of the Latin type *-ei, -i*, or Greek ἄγε, Latin *age*, etc.).

Now really no one denies this possibility of an uninflected Indo-European, but it has ceased to be an object of interest. Very few give detailed attention to the subject. There are exceptions, naturally. The main one is W. P. Lehmann (2005), who proposes an old «active» Indo-European with a system of classes. And I myself.²⁴

The objection is sometimes raised as to how a sentence can be constructed in a language with no inflections, based on monosyllabic root-words. But this is habitual in, for example, Chinese and in various languages of southeastern Asia. Indo-European made use of the fact that some of these root-words were only nouns, others only verbs; it made use of word order, lengthenings, reduplication, tone, the determination of one word by another, composition. Vestiges of all this have remained in later IE.

But it is noteworthy, really, that Indo-European began as a language with a minimal morphology that later germinated and grew gradually, its complexity reaching its peak in Indo-Iranian and above all in Greek, and then gradually diminished (cf. Adrados 2001; 2005a). The culmination of this process is found in the languages that have eliminated, or practically eliminated, the inflection of the noun, have reduced to the minimum the inflection of the verb, have made maximum use of uninflected words and of word order, etc.

In spite of everything, the different phases of Indo-European have much in common: the same word classes, transitives and intransitives, lengthenings and thematic vowels added to the right, etc. But it is a fact that much less attention is being devoted to this evolution of Indo-European and there is, increasingly, a return to a unitary language with a maximum of morphology. I have fought against these tendencies all my life, but as can be seen, they are very strong. The idea persists that evolution is, more than anything else, the destruction of an old, complex and perfect system, whose elements, to the

²⁴Cf. Adrados 1972; 1973a; 2000. I have emphasized this theme in Adrados - Bernabé - Mendoza 1996: 134 ff., 381 ff.

extent that they are preserved, have not changed their semantic and grammatical values. I believe that this is an error.²⁵

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²⁵See more details in "Must we again postulate a unitary and uniform Indo-European?", cited.

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