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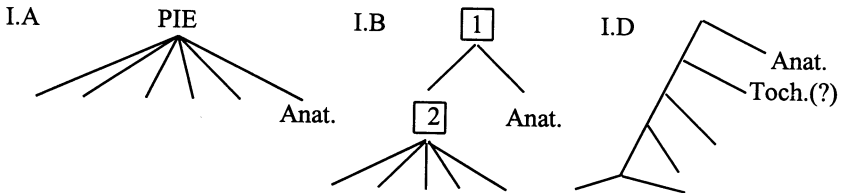
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More than eighty years after the identification by Hrozný of Hittite as an Indo-European language, the question remains open of how we are to best integrate the facts of Hittite (Anatolian)¹ into our overall picture of Indo-European. The same remark applies to Tocharian, but for various reasons—some valid, some not—the question of the dialectal status of the latter has not achieved the same prominence. We will have occasion to return to this point later. Despite the understandable impatience of some colleagues, who are weary of yet one more discussion of the “Indo-Hittite” question, the issue will not go away. Until we come to a consensus regarding the position of Anatolian—and the related question of to what extent we must modify our reconstruction of Proto-Indo-European itself—even those whose focus is on the history of other subbranches risk basing their analyses on a proto-system that may suddenly be invalidated.

The following remarks are divided into three unequal parts: a larger section dealing with conceptual and methodological issues; a short cautionary dissent to what I see as a developing consensus; and a mercifully short version of my now standard sermon on the importance of using *all* the available Anatolian data in deciding the question of the subbranch’s dialectal position.

There have been to my knowledge four basic conceptual approaches to the issue of relating Anatolian to the rest of Indo-European. The most prominent of these over the past half-century has been that Anatolian is merely one more subbranch of PIE like any other. This schema is represented in terms of the family-tree model in Figure I.A. This point of view, often termed the *Schwundhypothese*, is exemplified by the treatments of Pedersen (1945:190f) and Eichner (1975:100ff), among many others. As the label ‘loss hypothesis’ implies, this point of view assumes that Anatolian inherited essentially all of the formal and functional categories of traditional, “classical” (“Brugmannian”) PIE, and that their absence in Anatolian is due to loss. As one would expect based on their overall oeuvre, the version of this approach advocated by Pedersen and Eichner actually is quite nuanced and sophisticated. Both scholars explicitly make clear that the development to Anatolian (and specifically to Hittite) is far more complex than a simple loss of categories (Eichner 1975:73 speaks of *Umschichtung und Neugliederung*). It is my perception that this point of view, dominant in Europe for more than half a century, has recently lost ground (for the basis of this judgment see below). There are nevertheless still respected, mainstream specialists

who defend it: Eichner himself, e.g., presented this point of view at the Pedersen Kolloquium in Copenhagen in 1992.²



The second most famous model is that which views Anatolian as a collateral branch of PIE, as illustrated in Figure I.B. The best-known exemplar of this approach is the “Indo-Hittite” hypothesis of E. H. Sturtevant: see e.g. Sturtevant (1933a) and (1933b:29-33). As per the late Warren Cowgill, however, the labeling of the nodes numbered 1 and 2 in figure I.B is a point of decidedly secondary importance. As also stressed by Eichner (1975:72), it matters little whether one labels node 1 “Indo-Hittite” and node 2 “Indo-European”, or prefers to call node 1 “Indo-European” and invents a new term for node 2 (Eichner ventures *Rest-indogermanisch*). The crucial point is the claim that Anatolian did not share in a significant set of innovations common to the rest of the Indo-European languages. These are absent in Anatolian not because of loss, but because these features never existed in the prestages of Anatolian. This viewpoint usually implies the passage of at least a millennium between stages 1 and 2, but strictly speaking this assumption is not necessary, and not all adherents of this model commit themselves on the thorny issue of the speed of linguistic change. This account, never popular in Europe, now has relatively few proponents, but see Lehrman (1996). In essence, such a scenario is also presupposed by the very original theories of Adrados (1982:1-4 and elsewhere), although he does not frame the issues in family-tree terms.

A third approach is to claim that Anatolian is an archaic descendant of a PIE that must be radically revised vis-à-vis the “classical” reconstruction. It is somewhat difficult to compare this model with those cited above because it typically is presented in a framework that either is explicitly not that of the family-tree schema or one that is very vague as to relative chronologies. See among others Watkins 1969, Meid 1975:216, Neu 1976:243 and 1985, Drinka 1988:254ff (the last not in family-tree terms!). However, Meid (1988:11) explicitly places Hittite in a geographically central group extending roughly northeast to southwest that shows some “middle Indo-European” innovations versus those dialects to the east (Indo-Iranian and Greek) and to the west (Celtic and Germanic). This point of view became quite popular in the field in the sixties, and I believe it is fair to say that the principal debate for the succeeding thirty

years was between those who defended the schema of I.A and those who asserted that only a radical revision of PIE could accommodate the facts of Anatolian. I do not mean thereby to say that proponents of I.B did not continue to exist, but they were distinctly in the minority.

A fourth alternative that appears to have gained widespread acceptance in recent years is that which says that Anatolian was the first subgroup to “separate” from PIE (note the implicit family-tree conception). In some cases (e.g. Oettinger 1986:25), there is an explicit claim that the time-depth from PIE to the “separation” of Anatolian is quite shallow (a matter of a few generations). With or without this specification, what is common to this approach is the idea that the common innovations shared by the non-Anatolian subgroups consist not in the wholesale creation of grammatical categories (feminine gender, optative or subjunctive mood), but rather in the development of new formal expressions of existing categories, or reinterpretation of the latter: see for example Jasanoff (1994:167) on the aorist or Strunk (1994:430f) on the development of aspect from aktionsart. The same basic conclusion is also reflected in the dialectal stemma of Hamp (1984:153), although his criteria are not necessarily the same.

The debate just outlined involves conceptual issues as well as concrete facts. In a term paper written as a student thirty years ago for a class taught by Anthony Arlotta at Harvard, I referred to the possibility of “early separation” of Hittite. He dismissed this idea with the comment that the question of early or late was irrelevant. This remark reflects the dominant view in the U.S. of the family-tree model, namely what I may term the “sunburst” version illustrated in Figure I.A. As indicated explicitly by Bloomfield (1933:312), this conception of the family tree forgoes any claim of historicity in terms of break-up of a proto-speech community. It is merely a convenient abstract representation of the “genetic” relationships of the languages being described, with a number of unreal elements consciously built in.

While I have not yet found a diagram of this version of the family tree in a Continental publication, there is no doubt that Pedersen and Eichner share this conception in advocating model I.A. I quote Eichner (1975:72): “Eine Abweichung von diesem Alternativgefüge [i.e., I.A vs. I.B—HCM] verbieten die sprachlichen Fakten, einen Kompromiß zwischen den beiden Alternativen verbietet die Logik”. I believe that a number of colleagues would vigorously dispute both parts of this extraordinary statement. The first claim is true only if one thinks exclusively in family-tree terms. The second applies only if one adheres to the version of the family tree in I.A. I will never forget the shock I experienced when I first found August Schleicher’s original family tree (Schleicher 1871:9), with its much more naturalistic appearance, produced by the (historically plausible) assumption that

first one language group and then another separated from the common speech community. This conception of the family tree has persisted, both in the U.S. and abroad (see by way of example Hockett 1958:519, Lehmann 1962:139, Cejador y Frauca 1911:323, Tagliavini 1969:408). Obviously, approach I.D. to the issue of the position of Anatolian implies such a viewpoint.

Contrary to my expectations when I began research for this paper, I (re)discovered that approach I.D. was actually the *first* popular response to the new evidence of Anatolian! Emil Forrer (1922:26f) proposed such a schema, but with the unique twist that he viewed Luvian as the first subgroup to branch off, followed by Hittite, and then the rest of Indo-European (see also the approval by Kretschmer 1925:301, who threw in Etruscan for good measure). A similar view was presented by Ungnad (1923:3-4), but with more emphasis on Lycian. To return to more mainstream ideas, Meillet (1931:2-5), in a paper famous mostly for its description of the rise of the feminine gender in PIE, proposed that what he termed “marginal” languages (Hittite, Tocharian, Italo-Celtic, and Armenian) separated from the others before the development of the feminine gender and the loss of the “r”-middle. Finally, Petersen (1933) argued that Hittite and Tocharian were the first branches to “leave” the proto-speech community—a position now espoused by several scholars.

Based on a number of published works, including those by Jasanoff, Oettinger, and Strunk already cited above, and on the tenor of discussions at the meetings of the *Indogermanische Gesellschaft* in Copenhagen and Zürich in 1992, I now conclude that there is a growing consensus among many in favor of model I.D. The remaining debate seems to center on the question of just how radically we should revise our reconstruction of the proto-language at the top node of the diagram (whether we still term this “Indo-European” or something else), in order to account for the archaisms of Anatolian (and Tocharian). This debate involves quite major and substantive points, but it is taking place largely in a context where two things are now taken as more or less established: (1) model I.D. best represents the relationship of Anatolian and Tocharian to the rest of the family, not I.A. or I.B; (2) as intimated above, the innovations shared by the languages other than Anatolian and Tocharian involve mostly formal innovations and readjustments of existing grammatical categories, not development of entirely new ones. Once again I am making gross generalizations and do not mean to suggest that other viewpoints (including I.A. and I.B.) do not exist in the field.

I view the development just described as generally quite positive for the field. Whereas there was little hope for any kind of definitive decision between the irreconcilably opposing views of I.A. and I.B. (and none for a compromise, as per Eichner), the consensus for some form of I.D. permits useful debate on the

specifics of its realization. I personally think that some version of I.D is the likeliest solution to the problem of integrating Anatolian (and Tocharian) into Indo-European. Although I played devil's advocate in raising another possibility, it seems clear that my own recent analysis of the feminine gender in Anatolian (Melchert 1994), if accepted, is most easily explained in terms of a common non-Anatolian innovation in which the already existing feminine gender underwent a major formal adjustment.

I am concerned, however, that a “bandwagon” effect may lead to a rush to judgment. In particular, the *presumptive* “early separation” of Anatolian and Tocharian begs the important question of whether *all* features shared by Anatolian (and/or Tocharian) with western Indo-European dialects can be explained as archaisms. The growing dominance of the model I.D has apparently led some to dismiss out of hand any possibility that such shared features might be innovations, since this idea would be incompatible with the now favored view of early separation of Anatolian and Tocharian. I emphasize that I know of no “smoking gun”—a shared feature that *must* be interpreted as an innovation. However, several do seem to me arguable (e.g., the “r”-middle, the use of the interrogative stem *k^o/i- as a relative). I refer the reader to the works of Pedersen (1925:43f, 51f), Kammenhuber (1961:69ff), Melchert (1994:242), and especially Puhvel (1994:passim), whose arguments on the basis of shared *structured sets* of lexical isoglosses have not received the attention I feel they deserve. In sum, I would like to see a genuine debate on this issue, not a summary dismissal based on the prejudice that I.D makes shared innovations unthinkable. I note finally that shared innovations between Anatolian (or Tocharian) and the western dialects only stand in absolute contradiction to early separation if one thinks solely in family-tree terms. If one regards the issue in terms of dialect geography, nothing precludes some shared innovations with the west followed by relatively early isolation (cf. the remarks of Puhvel 1994:317).

With the indulgence of colleagues who have heard my rantings on the topic before, I must also repeat in this forum my insistence that the debate about the dialectal position of Anatolian *must* henceforth be informed by all the available evidence from the subfamily, not just that from Hittite. We must never forget the fact that attested (Old) Hittite is the product of a considerable historical development from the common pre-stage we may call Proto-Anatolian, during which it underwent an undetermined number of innovations. Yet it is Proto-Anatolian that must be taken as the point of comparison in considering the relationship of the subgroup to the rest of Indo-European. To address this issue, and to ensure that you take away from my presentation at least some useful new data if nothing else, I close with mention of two features whose status in Anatolian has been underappreciated or totally overlooked.

First, as already pointed out by Mittelberger (1966:104) and reiterated by Bader (1991:139f et aliter), there exists in Anatolian beside the thematic genitive singular in *-as* < **-os* (Hittite, Palaic and Hieroglyphic Luvian) also an ending that surely reflects **-e/oso* (Hieroglyphic Luvian and Lycian). I also make bold to suggest that the Carian ending *-s*, with the synchronic function of a dative (perhaps also still of a genitive) belongs here as well (on the function of the ending see Schürr: forthcoming). Much less certain but also worthy of consideration is the possibility that Hieroglyphic Luvian genitive singular */-asi/* continues apocopated **-osyo*.

Second, I have recently assembled evidence (Melchert: forthcoming) that there are remnants in Hittite and Cuneiform Luvian showing that Anatolian did inherit the aspectual contrast of imperfective vs. perfective (“present” vs. “aorist”), not merely the formal markers of these categories in the pre-aspectual guise of aktionsarten (as argued by Lehrman 1985:62ff or Strunk 1994). The evidence is sparse, and it remains to be seen whether my analysis wins acceptance. The potential ramifications of my results for the dialectal position of Anatolian are self-evident.

I hope to have persuaded you that the issue of the dialectal position of Anatolian within Indo-European remains a vital one, both informing and being informed by the more general debate about the nature of linguistic reconstruction and the status of genetic relationship of languages and the various models we employ to elucidate it. We may hope that additional data and more successful exploitation of existing knowledge will lead to further refinements both in our vision of inner Indo-European relationships and in our practice of the methods on which our discipline depends.

Notes

¹ Although the existence of an Anatolian subfamily has been recognized for more than sixty years, most discussions of the problem have framed it in terms of Hittite. In order to avoid needless repetition, I will use either Hittite or Anatolian interchangeably according to the usage of the scholar cited, except where explicitly noted.

² Unfortunately, a written version of his oral presentation was not published in the proceedings of the conference.

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