Indo-Hittite Revisited

The classic family-tree diagram of genetic relationships among languages[1] was never meant to describe the chronological and dialectal intricacies of subgrouping, and should not be reproached for failing to do so. Just like its prototype, the manuscript-descendance stemma, the language-family tree is only meant to show *which genetically related complexes share innovations not known in the rest of their "kin"* – no more, and no less. The genetic relationship among three languages, to choose the minimally relevant case, is established by demonstrating *a common inheritance* – the material that has been retained by all three – on the basis of regular sound correspondences. If two of the three languages *share a number of innovations not likely to be coincidental,* then the two must have come from a common source separate from the third language and thus constitute a separate subgroup. The fact that languages develop through time and show dialect variation, i.e., are not uniform, is taken for granted, as is the fact that each set of phonetic and grammatical phenomena at any given time belongs, whether exclusively or not, to a specific register of the language (dialectal, social, etc.). The relevant details, chronological and dialectal in the broadest sense, can be and usually are spelled out in any competent comparative work, depending on its scope; the family-tree diagram, however, is only designed to show the relationships of subgrouping. The important thing, all too easy to lose track of, is that subgrouping cannot be done on the basis of shared archaisms but only on the basis of shared innovations. The so-called "archaisms" are only discernible by contrast with either their more productive counterparts or else with the absence of anything corresponding to them; in other words, archaisms are no more than the obverse of the corresponding innovations. For example, the OCS. 1.sg.prs. -ѣ found only in вѣдѣ

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[1] The principles on which the discussion in the first paragraph of this essay and *passim* is based are stated in Cowgill 1986: 15–17 (with reference to Brugmann 1884: 253).

'I know' is recognized as an archaism only against the background of the younger form (innovation) вѣмь and against the background of the productive 1.sg.prs. -ж (also an innovation) found almost everywhere else; Anatolian "laryngeals" are recognized as an archaism through the contrast with their absence, or loss – also an innovation – in the non-Anatolian Indo-European languages. In any case, archaisms are a part of what is inherited and cannot be used for the purposes of subgrouping.

The reconstruction of Proto-Indo-European was thought to be virtually complete when Hittite was discovered. The discovery proved deeply disturbing to scholars in the field. While Hittite was clearly akin to Proto-Indo-European, it displayed a glaring absence of a number of significant grammatical patterns and categories that had been taken for granted in Proto-Indo-European, in other words, that constituted the Proto-Indo-European inheritance: absent, incredibly, was the feminine gender in the noun[2]; absent were the familiar *so-/to-* demonstrative pronouns; absent were the imperfect, aorist, and perfect tenses, as well as the subjunctive and optative moods in the verb. Hittite also displayed a number of unfamiliar phenomena, such as the "laryngeal" *h*, unknown in Proto-Indo-European, and a system of consonants in which the Proto-Indo-European three-way contrast of voiceless, voiced, and voiced aspirated corresponded, on the face of it, to a two-way contrast of non-geminate and geminate consonants, whose phonetic nature was not clear.

These striking differences were either explained away as losses and other types of innovations within Hittite (with the exception of the "laryngeal," on which see below), or else they were simply ignored, so that Hittite and the rest of the Anatolian languages came to be regarded as Indo-European languages like any others; the Anatolian family was viewed as just another filial branch of Proto-Indo-European, like Indo-Iranian, Greek, or Celtic.

The cost of this seemingly conservative "explanation", however, proved to be very high. The most pernicious consequence of it was the erosion of the firm logical ground on which the theory of language subgrouping had rested. But that was not the only revolution-

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[2] On the fictitious traces of feminine in Anatolian see below.

ary consequence of this pseudo-conservatism. If the classic family-tree diagram was to be preserved at all costs, then the one Anatolian phenomenon that could not in good conscience be dismissed as an innovation, namely the "laryngeal" – actually, two "laryngeals" as it turned out – had to be acknowledged as an archaism and admitted into the reconstruction of the Indo-European protolanguage. The absence of the feminine and all the rest could still be viewed as Anatolian losses rather than as shared innovations of Indo-European separate from Anatolian.

The obvious problem with this view was the wide chronological gap separating the attestation of Anatolian from the oldest non-Anatolian Indo-European languages. Hittite texts, in a form that points to a well-established and well-developed tradition, are attested beginning in the 17th century B.C. and continue until the 13th, while the earliest indirect testimony for non-Anatolian Indo-European comes from 14th-century B.C. Indo-Iranian names preserved in Hurrian texts from Mitanni; original texts in an Indo-European language, represented by Mycenaean Greek tablets, hail from the 12th century B.C., that is, a full century after the fall of the Hittite empire. "But how can a language attested so early, 1500 B.C. and indeed earlier, have 'lost' everything which is characteristic of Greek and Sanskrit, have lost it much before their time, lost it without a trace? It is hardly credible," says Wolfgang Meid (1990 [1986]: 15), and I could not agree more.

I shall yet have cause to return to Wolfgang Meid and his influential revision of the classical family-tree model. But first I have to mention the alternative view of subgrouping, the view that is firmly grounded in the well-tested classical theory of subgrouping – namely, the Indo-Hittite theory. In this theory, the significant features and categories found in Proto-Indo-European but absent in Anatolian are correctly regarded as *shared innovations* of the Indo-European family; the Anatolian protolanguage (which has its own innovations, not shared by Indo-European) constitutes in the family-tree diagram a separate branch, *laterally – not filially* – related to Proto-Indo-European. These "sister" families must thus descend from a single parent language which Edgar H. Sturtevant dubbed "Proto-Indo-Hittite." While one may argue about the felicity of his coinage, his theory of the subgrouping of Anatolian and Indo-European is the only one consistent with the known facts.

Yet scholars in the field have been unwilling to accept Sturtevant's theory. Wolfgang Meid is a classic and influential example. While recognizing that "to subjugate Hittite under the traditional concept of Indo-European, considering it on par with Greek, Iranian, and the rest, is not a realistic [alternative]," (op. cit., p. 16) he is nonetheless dismayed by the division of the Indo-Hittite protolanguage into the collateral Proto-Anatolian and Proto-Indo-European branches. "There is no justification for such a dichotomy," he says (ibid.). But what can justify just such a dichotomy if not the presence of significant innovations in Proto-Indo-European, which Meid himself willingly acknowledges? He writes: "There are many indications that Hittite branched off earlier from the mainstream of Indo-European though perhaps not as early as to justify a modification of the traditional 'Stammbaum' model to the extent that Proto-Hittite [= Proto-Anatolian] would be on equal terms with Indo-European, both deriving from a higher node in the stemma, labelled 'Indo-Hittite' by E. H. Sturtevant." Meid apparently believes that the lines which connect the language nodes in a family tree have something to do – or should! – with *how early* innovations occurred and not with the fact that they occurred. He says further: "(...) modern dialectology has taught us that there are not clearcut breaks between dialects, but rather a number of gradual transitions, and that the emergence of different dialects, and their separation into languages, is a slow process" – as if the speed with which languages change were relevant to subgrouping! W. Meid's first charge against the classical family-tree model, then, is that its lines represent neither the exact temporal point of change nor the speed with which change occurs. Yet, may I remind the reader, the classical family tree was not designed for that. It was intended only as a diagram of relationships among languages, based on shared innovations, and nothing more. A discussion of exactly when and at what rate particular changes occur is irrelevant to subgrouping and belongs in a different place.

W. Meid's second charge against the tree model is that it disregards *archaisms*. In his influential 1975 essay he writes: "Eine Sprache ist niemals völlig uniform. Das sprachliche System enthält zu jeder Zeit neben die Regel oder Norm darstellenden allgemeinen Sprachgebrauch sowohl einerseits Archaismen, d. h. Überreste früheren Sprachgebrauchs, als auch erste Indikationen von Neuerun-

gen, d. h. Entwicklungstendenzen, die später zum Durchbruch kommen können. (...) All dies macht uns zur Pflicht, die Systeme sämtlicher indogermanischer Einzelsprachen, auch der spätbezeugten, auf ihre Archaismen hin zu untersuchen und diese gegeneinander abzuwägen und in Relation zueinander bringen. (...)" (1975: 210, 211 f.; reiterated in English in 1990 [1986]: 17 f.). "Ein Archaismus ist immer ein kostbares Relikt, und östliche Archaismen und wieder andersartige westliche ergänzen sich gegenseitig zum Bild des frühen Indogermanischen. Mittelindogermanisch sind die etwas rezenteren, aber vergleichsweise immer noch altertümlichen Strukturen, die normalerweise durch Übereinstimmung in Ost und West erwiesen werden (...) Spätindogermanisch sind Strukturen, die nur regionale Verbreitung und Produktivität hatten (...)"[3] The main trouble with this approach is that archaisms cannot be used in subgrouping *because subgrouping is possible only in reference to shared changes, never in reference to shared retentions,* which is precisely what archaisms are; *the retentions (archaisms, relics) constitute, by definition, the shared inheritance on the basis of which affinity only, not divergence, can be established.* Furthermore, Meid's chronocentric proposal to divide the protolanguage into Early, Middle, and Late phases is completely irrelevant to subgrouping.

Wolfgang Meid opts to replace the family tree (Stammbaum) with something like a family river ("Stammfluß" one could call it). Con-

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[3] Meid 1975: 213. It becomes clear from these and other statements as well as from W. Meid's illustrative diagrams that the word *archaism* (*Archaismus*) has two different meanings in Meid's texts, and that he is not aware of it. The first meaning comes to light when W. Meid describes his view of language as consisting at every period of "*archaisms,* norm, and innovatory tendencies" (1990 [1986]: 18; "Archaismen, Norm, Neuerungstendenzen" 1975: 210). In this schema, archaisms and "innovatory tendencies" contrast with the norm and coexist parallel to it. This meaning is quite different from the meaning normally assigned to the term. But when W. Meid gives examples of these archaisms (the Vedic injunctive, the medial 3. sg. ending *-o-* [vs. the innovative *-to-*] and Vedic *śaye* [vs. Sanskrit *śete*], the medial extension *-r*, etc.), he unconsciously reverts to the traditional meaning: in the attested language systems of Vedic, Greek, Slavic, etc., these archaisms do not contrast with any simultaneously existing norm but *constitute the normal form of their words* within their systems. They are called "archaisms" not by virtue of their contrast with competing forms of the same words but by virtue of their contrast with the forms proved as *innovative* and belonging to *other language systems,* be it later stages of the same language (such as *śaye* and *śete*) or other languages.

sider this description of his revised model (1990 [1986]: 19f.): "Thus, if we envisage a staggered differentiation, like
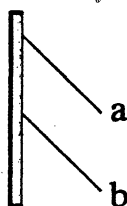
a may reflect earlier traits of the parent language which had been abandoned when b started off, but fail to reflect the most recent developments which, on the other hand, are reflected by b:

parent language

a			b
———————— 1	———————— 1		
———————— 2	———————— 2	————————	
———————— 3	———————— 3	————————	
4	———————— 4	————————	

If we assume that Hittite is comparable to *a* in this model, and other Indo-European languages behave like *b*, and assume also possible derivations *c*, *d*, etc. which remained even longer in the Indo-European mainstream, it will be easier to account for the different patterning of the existing Indo-European languages and the different distribution of diachronic elements of the parent language reflected in them" (1990 [1986]: 19f.).

In the first diagram, the "stream" of Proto-Indo-European flows through time. At the end of its Early course, *a*, the Anatolian rivulet, we might say, flows out and breaks up into Hittite, Palaic, Luwian, and other language brooks (not shown). The Indo-European mainstream, however, flows on through time, and, at the end of its Middle course (sic?) the rest of the Indo-European languages, *b*, break out into a new river bed. What, however, are the "possible derivations *c*, *d*, etc.", assumed by Meid? I wonder. And does the mighty stream of the Indo-European protolanguage still flow on, at the time of this writing? The continuing line of the diagram certainly seems to suggest that. If the center line continued no further than the branching-off of *b*, we could think that the diagram represented the same graph as Sturtevant's Indo-Hittite tree, placed vertically. But the line does not stop at *b*.

The second diagram is explained most coherently in Meid's earlier presentation (1975: 211): "In der früher aus der Grundsprache ausgeschiedenen Sprache [a] ist ein Zustand 1 der Grundsprache noch reflektiert, der z. Z. des Ausscheidens von [b] schon aufgegeben war. Andererseits reflektiert die später ausgeschiedene Sprache [b] einen Zustand 4, der z. Z. des Ausscheidens von [a] noch nicht erreicht war. Beispiel für einen erhaltenen Archaismus die phonetischen Laryngalreflexe im Hethitischen (...), Beispiel für noch nicht vorhandene Neuerungen in derselben Sprache das Fehlen jüngerer Entwicklungsstufen der Perfektformation." In other words, the loss of the laryngeals and the evolution of the perfect exemplify shared innovations in the non-Anatolian Indo-European languages. This is not at all different from the well-known description of the basis for the Indo-Hittite subgrouping, *pace* Neu 1976: 253 f. and many others who accept W. Meid's rhetoric uncritically (except that "Zustand 4" cannot belong in the protolanguage, as the diagram seems to show: that would simply make no sense). Yet even these scholars intuitively hesitate to sign on to the "new" model. H. Craig Melchert, for example, is "not yet persuaded that a temporal-spatial model such as that advocated by Meid (1975) can be realized in a meaningful way" (1994a: 3) – and he is right, because, as should be amply clear by now, Meid's model is meaningless. If it is stripped of irrelevance and error, it becomes essentially identical to the Indo-Hittite model.

If the extra-Anatolian Indo-European languages show several significant shared innovations, then the Indo-Hittite model has to be accepted as the only correct one. The already mentioned characteristics of Anatolian – the presence of the "laryngeals," the absence of feminine, the absence of perfect (the ḫḫi-conjugation being a very different formation indeed, see Cowgill 1975 and 1979), the absence of simple thematic presents (Lehrman 1985), the absence of the aorist, the absence of the subjunctive and optative moods – constitute some of the most significant Anatolian "archaisms," i.e., they are matched by corresponding shared innovations in extra-Anatolian Indo-European. These characteristics, however, with the obvious exception of the "laryngeals" (on which see below), have been at different times claimed to be Anatolian losses. The absence of the feminine in Anatolian has been challenged most recently by scholars claiming to have found traces of the feminine in Luwian (Starke 1982, Oettinger 1987) and Lycian (Melchert 1992b, 1994b).

H. Craig Melchert has even gone so far as to state (1994c: 129): "It is now certain that Common Anatolian inherited three genders from PIE.: masculine, feminine, and neuter. The strongest evidence for the feminine is provided by Lycian animate nouns with *a*-vocalism such as *lada-* 'wife' (Melchert 1992b) and by the phenomenon of "*i*-motion" in at least Luvian, Lycian, and Lydian (Oettinger 1987, after Starke)." Let us examine this alleged evidence closely, starting with the "strongest."

The "strongest evidence," upon examination, turns out to be incredibly weak. Lycian is attested very late – only from about the 5th through about the end of the 4th century B.C. This means that some twelve centuries of language development elapsed between the earliest attestation of Hittite and that of Lycian. To rely on Lycian in building inferences for the purposes of "pre-Anatolian" reconstruction would be about as justifiable as relying on Old Prussian for the reconstruction of Proto-Indo-European – with the important difference that Lycian is known and understood so meagerly that Old Prussian, by comparison, appears problem-free. Maximum caution should be exercised in any comparative inferences drawn with the help of Lycian, a language scantily attested and fraught with basic problems, whose philological processing, in spite of a century-old effort, is still in its infancy. The origin of many of the forms that Melchert uses as comparative evidence cannot be said to have been determined with the certainty required for comparative use. Eager to prove that certain Lycian nouns in -*a*- continue PIE. feminines in *-$\bar{a}$ ("< *-eh_2"), Melchert compares Lycian directly with Proto-Indo-European, as if Lycian could rival Hittite either in seniority or in quality of attestation. Melchert first sets out to prove that, unlike Hittite and Luwian, Lycian preserved the contrast of PIE. *o and *a; this would allow him to "demonstrate" that Lycian clearly differentiated between *o-stems and *$\bar{a}$-stems.

The quality of Melchert's examples (1992b: 46f.) matches the erroneous nature of his premise. Here is a representative sampling. The Lyc. "infinitive" in -*āne/-ēne/-ēni* is split into two: the forms ending in -*e* are taken back to "*-$\bar{a}tn\bar{o}$, matching exactly Hittite -*anna*" (p.47), while the ending -*i* "probably continues a locative form" (ibid.). But PIE. "*-$\bar{a}tn\bar{o}$" is itself problematic, as is the origin of the Hittite directive case in -*a*. Is the continuation of PIE. short *-o in Lycian supposed to be the same as that of the long *-$\bar{o}$

($<$ *eh_3??)? Melchert says nothing on the subject, nor does he elaborate on the origin of the directive case (beside referring the reader to Gk. ἄνω, κάτω and Lat. *quō*, see ibid., p.47). To consider Melchert's next potentially promising morphological example: it is extremely unlikely that Lyc. prt. 3rd sg. ending *-te/-tẽ* (if cognate with Luw. *-tta*) continues PIE. middle *-to*; the Luwian final vowel is far more likely to be analogical and not go back directly to PIE. *-o*; the same concerns Lyc. pl. *-ñte/-ñtẽ*. Lyc. *ẽke* 'when' (: Luw. *a-ah-ha*?) need not continue PIE. *en-h_2o*, with precisely *o* rather than *a*. Lyc. *ñte/ñta* (if from *endo*) changes its vowel under "vowel harmony" and can by no stretch of the imagination be probative. Lyc. *ebe-* 'this' (: Hitt. and Luw. *a-pa-a=*) cannot be remanded to any "virtual *obho-*" (p.46): "vowel harmony," by Melchert's own rules (p.44), disqualifies Lyc. *ebe-* from consideration because all of its attested forms contain front vowels in their final syllables. The Lycian particle *me* (and *mẽ*) is also problematic. This particle which, Melchert says, "[d]espite its partially divergent syntax certainly [sic! – A.L.] equals Hitt. *-ma*," and which also, according to Melchert, appears in "*im-ma* = Lat. *im-mō* 'indeed' $<$ PIE. *id-mō*" (p.46), should likewise be disqualified because it always occurs either before the particle *ne/nẽ* or before the pronominal particle *ti*, with front vowels.[4] Melchert's best example is Lycian *pddẽ* 'place', possibly but *not necessarily* interpreted as a neuter nominative singular corresponding to Old Hittite *pí-e-da-an* 'place'. Even if *pddẽ* really is a nominative/accusative singular cognate with *pí-e-da-an* – and I cannot emphasize the uncertainty of both assumptions strongly enough – there is no need whatsoever to make the extremely risky inference that its vowel directly continues PIE. *o* instead of Proto-Anatolian and even Proto-Luwian *a*. Lycian had certainly had enough time – at least 1,200 years and probably quite a bit more – to innovate in ways difficult to determine at this stage of our knowledge of it. The

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[4] Note in this connection "a development of special feminine forms in the [Lycian] adjective" (ibid. p.48), one of which Melchert finds in TL 100 *ebe xupa me tibeija* (*tibeija* acc. to him is a "fem. nom. singular of a relational adjective in *-iyo-*"). Whatever it is (the inscription is clearly incomplete), the *ti* occurring right after *me* probably does not belong with "*tibeija*" but with the preceding *me*. And I heartily welcome Melchert's admission (1994b: 236) that his "examples (...) for feminine *-a-* in adjectives in Lycian must be regarded as uncertain." They should be abandoned altogether.

phenomenon of Lycian vowel harmony, familiar but, *pace* Melchert 1992b: 44, opaque in details (like everything else in Lycian) – the regressive change of *a* to *e* or *e* to *a* depending on the front or back quality of the final syllable and possibly on some other unclear factors – should, failing everything else, introduce an ethos of deep humility into any speculation on the origins of Lycian *a* and *e*.[5] The possibility of Lyc. *lada-* 'wife' and the rest of Melchert's Lycian "animate class"[6] directly going back to PIE. feminines in *-ā* has to be rejected. There is no compelling reason to regard the final *-a* of this group of nouns as a direct descendant of PIE. *-ā*, bypassing the far more compelling evidence of Hittite and Luwian. To be sure, the striking similarity of Lyc. *lada-* to Russian лада is tempting, but nothing more; the Lycian-Russian lexical "isogloss" *lada* : лада[7] may well be compared with Meillet's classic example of Modern English *bad* : Modern Persian *bad* (both 'bad'). In both cases, the best thing to do is to leave well enough alone, at least for the time being: far too many links are missing. A real explanation for the Lycian nouns in *a* may be found after the basic philological work has been satisfactorily completed. But we would do well to keep in mind that a phenomenon in need of an explanation may not be used to explain another, still unexplained, phenomenon; such an "explanation" merely misleads those – and they are a majority – who have no first-hand familiarity with this poorly mapped-out terrain.

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[5] Melchert is forced to disqualify some of his examples (Lyc. *pede-* 'foot', *esbe-* 'horse', *epri-* 'rear', *epirije-* 'sell' (: "Hitt. *ḫappiriya-*") due to the "umlaut rule" (1992b: 47), which does not prevent him from using some of them as probative on other occasions. More of his examples must be disqualified for the same reason, see above.

[6] The term "animate" in relation to this group seems unsuitable in view of the fact that at least half of the nouns in the group are not particularly animate ('tomb', 'building', 'altar', 'chamber'); it might just as well be called the "repository" or "container" class. Beside *lada-*, Melchert lists the following as original members of the class: *χñna-* 'grandmother', *Malija-* 'Athena', *χupa-* 'tomb', *ñtata-* 'chamber', *prñnawa-** 'building', *χaha-** 'altar' (only inst./abl. *χahadi* attested).

[7] Closer to home, Lyc. *lada-* 'wife' is likely to be related to OHitt. *lazzi-* 'good, pleasant' (whence Old Hittite *lāzziia-ᵗᵗᵃ* 'to be well, recover'), Hier. Luw. (*ar-ha*) *la-ta-/la-rà/i-* (with "rhotacism") 'to put in order, arrange'; also the factitive *la-ra/i-nu-*. Hittite shows that the Proto-Anatolian form and the source of Lyc. *lada-* was **lat-*, whatever the source of the stem vowel. Russian ладъ, лада, etc., may somehow be related, but that is an unrelated question.

There is, then, no basis whatsoever for the claims made in Melchert's main essay on the subject (Melchert 1992b), to wit: "I cannot pursue here all the consequences of the *demonstration* [italics mine – A.L.] that Lycian distinguishes PIE. *o from *a. (...) The Lycian evidence for animate(!) stems with *a* vocalism distinct both from the reflexes of *o*-stems and from neuter collectives *answers in the affirmative* [italics mine – A.L.] the long debated question as to the existence of the feminine gender in Anatolian. *Evidence for Indo-Hittite or for an early separation of the Anatolian branch will have to be sought elsewhere* [italics mine – A.L.]" (ibid. p.52). Cf. also Melchert 1992d: "Now that feminine *ā*-stems *have been demonstrated* [italics mine – A.L.] for Anatolian (...), I see no reason not to take Lydian anim. acc. singulars in *-ãv* from *-ām (...)" (p.37). Nothing at all has been *demonstrated*; a new mildly sensational δόξα, in which *explananda* masquerade as *explanantia*, has been put into circulation, through the unfortunate neglect of some of the most fundamental methodological principles of comparative philology – and that is all.

I shall now turn to the evidence for the Anatolian loss of the feminine that is not quite as "strong" as the one just discussed but perhaps somewhat better known, namely the "*i*-motion" suffix discovered in Luwian by F. Starke (1982) and further developed in Oettinger 1987 and Melchert 1994b.

F. Starke noticed that in the nominative and accusative of both numbers of *common-gender* nouns in Cuneiform Luwian the vowel *i* unexpectedly appears where one would expect either the thematic *a* or no vowel at all (as in *-n-*, *-nt-*, and other athematic stems). In the oblique cases the vowel disappears, yielding to the expected forms. Starke proceeded to explain this phenomenon as a suffix cognate with PIE. "Motionsfemininum" in *-ih₂* (Starke 1982: 408 footnote 3 et passim), overlooking the much more likely – and, alas, homely – event of a Common Luwian innovation: the postaccentual Proto-Anatolian *-a- before final *-s becomes "raised" to a vowel written with *-i-* or occasionally with *-e-*,[8] phonetically probably [ə].

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[8] Unlike Old Hittite, Cuneiform Luwian in the attested period did not seem to distinguish /e/ and /i/, cf. the variant spellings *ti-ia-am-mi-eš* and *ti-ia-am-mi-iš* 'earth', *el-ḫa-ú-i* (1.sg. prs.) and *il-ḫa-ti* (3.sg. prs.), *ap-pa-ra-an-ti-en* and *ap-pa-ra-an-ti-in* (acc. sg. com.), *ḫal-li-eš-ša* and *ḫal-li-iš-ša*. Hieroglyphic Luvian does not seem to have had this distinction either.

It is also very likely that this vowel was phonetically distinct from the continuation of the inherited Proto-Anatolian *-i-: notably, Hittite borrowings from Luwian render this Luwian -i- < *-a- as -a-, see e.g. the examples given in Starke (op. cit., p. 410, footnote 13). Note also that the change did not occur in non-final position, as shown by first members of compounds such as *Walwa-ziti-*, *Arma-ziti-* (but cf. *Piha-walwi-*, pl. nom. ᴰEN.ZU-*in-zi* [arminzi]). Analogically, common-gender accusative singular also received the vowel -i- before -n, to differentiate it from the neuter: (neu. sg. nom.) -an : (com. sg. nom.) -iš = (neu. sg. acc.) -an : (com. sg. acc.) *-an → -in. From this new accusative singular the vowel spread to the common-gender nominative and accusative plural: (neu. sg. acc.) -an : (com. sg. acc.) -in = (neu. pl. nom./acc.) -a : (com. pl. nom.) *-anzi → -inzi, (com. pl. acc.) -anza → -inza. The innovation does not seem to have taken place in accented syllables, cf. e.g. Cun. Luwian nom. sg. com. EN-aš 'lord, master' (not *EN-iš!): Old Hitt. com. nom. sg. *iš-ḫa-a-aš* (with plene).[9]

Many details of the Common Luwian sound change described above remain to be worked out, but it seems to me, all in all, a far more reasonable explanation than the fanciful "i-Motion suffix" which mysteriously appears in the nominative and accusative and just as mysteriously disappears in the rest of the common-gender paradigm. A stranger suffix has never been witnessed! Certain scholars have gone to great lengths to justify its unprecedented, unsuffixlike behavior. N. Oettinger even mobilizes for this purpose the dread armor of structuralist theory.[10] H. Craig Melchert (1994b) sees

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[9] Cf. also Old Hittite *ḫa-aš-ša-a-aš* 'hearth' (with the plene discussed in Hart 1980: 14) vs. Lyc. *χaha-** 'altar' (if the latter really belongs here). The Lycian a-nouns may at least in part go back to Luwian stem-vowel accented nouns. But the Lycian facts are likely to retain their "local" significance and to continue to be irrelevant for pre-Anatolian reconstruction. In any case, the relationship of Lyc. *χaha-** with Lat. *āra* is an explanandum, not an explanans; Lat. *āra* cannot be used, as done by Melchert with *petitio principii*, to show that Lyc. abl./instr. *χahati* contains an a-class stem. Cf. Hart (ibid.): "A direct morphological equation with Latin *āra* is (...) not assured, although there can be little doubt that the words are ultimately related."

[10] "Diese Beschränkung innerhalb der Kasus ist m. E. als sekundär erklärbar. Es dürfte sich um eine Reduktion auf das Wichtigste handeln, denn Nom. und Akk. sind die am stärksten "grammatischen" Kasus' und bilden daher den Kern des 'Deklinationsparadigmas'." (Oettinger 1987: 41 with footnote 26: "Zum Begriff der 'grammatischen Kasus' s. J. Kuryłowicz, Esquisses linguistiques, p. 131–150" and

the weakness of Oettinger's reasoning but needs the "suffix" as evidence for his alleged pre-Anatolian feminine. Therefore he comes up with the following hypothesis: "[T]he development of the feminine gender took place in not one but two steps. In the first, the motion suffixes were added only in the nominative and accusative where gender was already marked. In the second, the suffixes were generalized throughout the paradigm. Anatolian reflects only the first stage. This hypothesis obviously has implications for PIE. relative chronology and the position of Anatolian" (1994b: 236). In other words, even if observed darkly through the glass of methodological error, Anatolian facts present a shape that simply refuses to fit the general Indo-European mould. Melchert proceeds to fall back on his Lycian invention, but as I have shown, the murky Lycian evidence cannot be used to clarify anything whatsoever. On the other hand, some Lycian facts become more intelligible in terms of the Luwian sound-change tack outlined above. Lycian final -i of e. g. ẽni 'mother' (Cun. Luwian an-ni-, an i-stem [abl. an-ni-ia-ti]), tedi (Cun. Luwian ta-ti-, an i-stem [abl. ta-ti-ia-ti]) 'father', the ablative in -adi (Luwian = a-ti), represents the inherited -i- of Proto-Luwian, while Lycian final -e in pddẽ 'place' as well as in the likes of esbe- 'horse' and izre- 'hand' simply continues the reduced Luwian vowel written as -i-, mistaken by Starke et al. for the "i-Motionssuffix."[11]

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footnote 27: "Vgl. zur Stellung von Nom. und Akk. im Deklinationsparadigma allgemein H. Seiler, Kratylos 11, 1966, p. 204 f."

[11] There is no reason to think that the Hittite i-stems parkui- 'clean' and dankui- 'dark' are anything other than Hittite remakes of older u-stems. (Lindeman in his review of Starke 1990 gives an unconvincing explanation of these i-stem transfers by appealing to "inanimate abstract nouns" in $*-iH_2$, see Lindeman 1994: 354. In the case of parkui- the innovation is more likely to have come from the denominative parku-ie- 'to cleanse', see my forthcoming book The Case for Indo-Hittite. Naturally, I agree with Lindeman [1994: 355] that "the whole idea of a Proto-Anatolian motion suffix -i- should be abandoned.") Pace Oettinger 1987: 38, fn. 14, there is no reason to think that Hittite denominatives alpuẽšš- and dampuẽšš- point to *alpui- and *dampui-. And I believe (cf. Starke 1990: 78) that Luwian wasui- and the like are Common Luwian thematizations of formerly athematic stems (such as e. g. Luw. suwani- 'dog') that subsequently underwent the phonetic change which I describe above. There is no ground here for the IE. $*-ih_2$-suffix (or any other similar suffix, whether ablauting or not), pace Melchert 1994b: 239–42 (and Oettinger 1987 with lit.).

Those who have tried to prove that the Anatolian languages had inherited the feminine gender of Proto-Indo-European and then proceeded to transform it or lose it, have failed to hit their mark. It does still hold, then, that it was rather Proto-Indo-European that innovated by creating the feminine gender. This innovation alone is significant enough to demonstrate the necessity of the Indo-Hittite model.[12] Yet there were also other significant innovations, already mentioned above.

There is, then, no viable alternative to the Indo-Hittite model. It alone is consistent with the known facts and with the corroborated "shared-innovations" principle of language subgrouping. Whether the protolanguage be called "Indo-Hittite" or something else is a different matter. I do, however, object to the Meidian "Mittelindo-germanisch" as it clings to the name "Indogermanisch." I propose that Sturtevant's time-weathered term be retained, as long as its obvious shortcomings are clearly explained to beginners.

Once the Indo-Hittite model is accepted, the need to accomodate in Proto-Indo-European the Anatolian "laryngeals" (exemplified by Hittite h and hh which variously reflect "$*h_2$" and "$*h_3$") falls away.[13] Contrary to well-known claims, there is not the slightest necessity, other than the habitual itch to use the rusty machinery of structuralist morphophonemics, to import into Proto-Indo-European the algebraic *coefficients sonantiques A, Q* and the neo-Saussurean *E* or anything like them – barring the subjective (nominalist) requirements of "symmetry" and "simplicity" or "economy" which, throughout history,

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[12] See Stempel 1994: 208: "Es bleibt die Frage, wie das Fehlen des Genus femininum im Anatolischen letzten Endes zu bewerten ist. Mir scheint es (...) weniger wichtig zu sein, daß darin ein Archaismus dieses Zweiges liegt, als vielmehr, daß alle übrigen Zweige des Idg. das neue Genus im wesentlichen mit denselben Morphemen bilden und denselben Kategorien zuordnen, eine tiefgreifende Innovation (...), die wohl einen noch recht engen Zusammenhang der nichtanatolischen idg. Sprachen voraussetzt, so eng vielleicht, daß wir zum Ansatz des Sturtevant'schen "Indo-Hittite" genötigt werden." Reinhard Stempel reaches his conclusion after an analysis of the *i*-motionists' arguments, without considering the Anatolian evidence that makes their play with reconstructed suffix forms superfluous. While I cannot agree with some of the specifics of Stempel's essay, I applaud his boldness in plainly stating the logical consequences of this shared Indo-European innovation.

[13] See Melchert 1994a: 21 f., 68–75. The "first" laryngeal, $*h_1$, which is not "preserv[ed] (...) in any position" (ibid. 65) should not be reconstructed at all.

have done nothing but distort the inconveniently unruly complexity of facts. There is no reason (except "symmetry" and "economy") why *all* Proto-Indo-European lexical roots need to conform to the "$(S)(T)(R) e (R)(T/S)$" pattern – in accordance with the Bolshevik principle that "the minority shall be subordinate to the majority" (note that there are no such Procrustean "rules" for suffixes and endings). There is no reason (except the same "symmetry" and "economy") why disyllabic roots[14] of Proto-Indo-European such as *pe,lə- / *plē- / *pl̥- (for *plə-) 'fill / full' should necessarily be cooked down to a monosyllabic skeleton, with *h_1 (E) or other laryngeals in place of vowel timbre plus length or the *shwa*, just to "simplify" things morphophonemically; *plə,no- being realized as *pl̥no- furnishes no evidence at all for treating ə as a consonant, *pace* Cowgill (1965: 144), for whom that was "the most powerful evidence" for "laryngeals." The "laryngeals" should be reconstructed – but for Proto-Indo-Hittite, *not* for Proto-Indo-European – and they should be reconstructed only when they are *actually attested* in Anatolian. While Proto-Indo-European proper should be left largely in the shape familiar from the second edition of Brugmann's *Grundriß*, Proto-Indo-Hittite needs to be reconstructed afresh – strictly on the merits of each cognate as it is actually attested.

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[14] "[T]he ablaut of the disyllabic bases (…) furnishes the most powerful evidence for the laryngeal theory, – evidence that I believe would be sufficient to establish the theory without help from Hittite *h*'s or any other sound effects that with more or less plausibility have been attributed to laryngeals" (Cowgill 1965: 144).

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