Indo-European Practice and Historical Methodology
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1. Introduction

One element of the recent controversy over historical methodology set off by Greenberg (1987)'s classification of American Indian languages has been his reliance on superficial lexical resemblances, with no attempt to establish phonological correspondences and no evidence from submerged morphology. Proponents of this methodology argue (Greenberg 1949, 1987, 1990, 1991, Ruhlen 1987) that this is the methodology used to establish the Indo-European language family, and that the success of these methods in the Indo-European case shows them to be reliable.

We argue that this view of the history of Indo-European studies is seriously flawed, in two ways:

(a) for the most part, neither the recognition of languages as IE nor their internal classification have been based primarily on superficial lexical resemblances;

(b) where such methods were employed, they frequently led to erroneous results.

The history of Indo-European studies thus provides no support for superficial lexical comparison, nor, more generally, for the methods advocated by Greenberg and Ruhlen. Indeed, the true history of Indo-European studies provides important object lessons on how to establish genetic affiliation, and how easy it is to go astray.

2. Methods for Establishing Genetic Affiliation

A persuasive argument for genetic affiliation will generally contain two elements:

Regular Phonological Correspondences

Regular phonological correspondences between items including a significant amount of basic vocabulary are necessary. Unsystematic superficial phonetic similarities do not exclude chance resemblance. Correspondences in basic vocabulary are necessary to reduce the possibility that the corresponding items are loans.

An eloquent statement of this point is given by Sapir (1931/1949;74):

> Inasmuch as all sound change in language tends to be regular, the linguist is not satisfied with random resemblances in languages that are suspected of being related but insists on working out as best he can the phonetic formulas which tie up related words. Until such formulas are discovered, there may be some evidence for considering distinct languages related — for example, the general form of their grammar may seem to provide such evidence — but the final demonstration can never be said to be given until comparable words can be shown to be but reflexes of one and the same prototype by the operation of dialectic phonetic laws.

Submerged Morphology

> "It is necessary to show not only that the resemblances are so numerous and detailed as to exclude the possibility of chance as an explanation but also that they are so tightly woven into the basic fabric of the languages that they cannot be explained simply as borrowings." (Goddard 1975;259)

The great Indo-Europeanist Meillet held that correspondences in vocabulary alone were insufficient to establish genetic affiliation (Meillet 1914/1926;91):

> Les concordances grammaticales prouvent, et elles seules prouvent rigoureusement, mais à condition qu'on se serve du détail matériel des formes et qu'on établisse que certaines formes grammaticales particulières employées dans les langues considérées remontent à une origine commune. Les concordances de vocabulaire ne prouvent jamais d'une manière absolue, parce qu'on ne peut jamais affirmer qu'elles ne s'expliquent pas par des emprunts.
>
> Grammatical correspondences provide proof, and they alone prove rigorously, but only if one makes use of the details of the forms and if one establishes that certain particular grammatical forms used in the languages considered go back to a common origin. Correspondences in vocabulary never provide absolute proof, because one can never be sure that they are not due to loans.

Moreover, he held that the strongest evidence involves irregular forms (Meillet 1925/1954;27):

> Plus sont singuliers les faits dont on constate entre deux langues la concordance, et plus grand est la force probante de la concordance. Les formes anomales sont donc celles qui sont les plus propres à établir une "langue commune".
>
> The more singular the facts observed to correspond in two languages, the greater is the probative force of the correspondence. Irregular

forms are therefore those most suited to establishing a "common language".

The importance of morphological evidence was recognized also by Sapir, who considered purely lexical evidence inadequate, as shown by this passage from his letter of 27 February 1913 to Alfred Kroeber (Golla 1984;89):

> Your material is certainly suggestive, but I cannot feel that I have any right to adopt a definite stand in the matter until I know far more about Shastan morphology than I do. As you may remember, I pointed out in my review of Dixon's Chimariko paper that it is difficult to know how to weight lexical correspondences without a definite knowledge of grammatical features as well.

In the review to which he refers (Sapir 1911;143), after citing a table of 57 lexical correspondance plus "a few general morphological resemblances", Sapir wrote:

> In the absence as yet of extended grammatical studies of the Shastan dialects, it is difficult for the student to express a definite opinion.

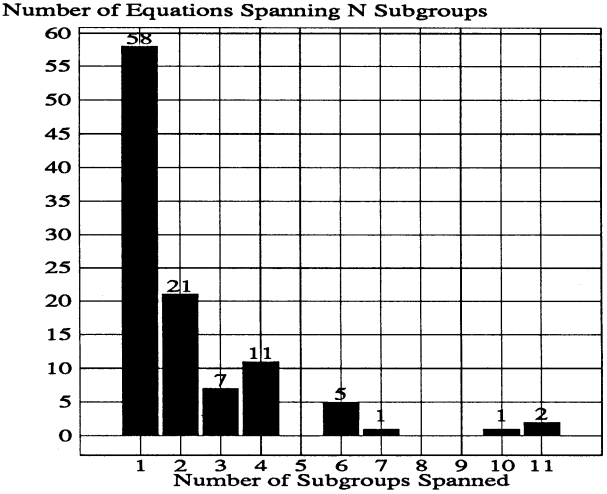
Indeed, Greenberg himself has pointed out the value of such evidence (1957;37-38):

> The presence of similar morph alternants in similar environments is of very great significance as an indication of historical connection, normally genetic relationship. This is particularly so if the alternation is irregular, especially if suppletive, that is, entirely different.

3. Greenberg's Methods

Most of Greenberg's evidence consists of lists of words taken to be similar in form and meaning, with no attempt to establish phonological correspondences. He also presents what he calls "grammatical evidence", which is not, however, the sort of submerged morphology that other scholars consider probative.

"grammatical" evidence of any kind is adduced only in a minority of cases. This can easily be seen by inspection of the following plot of the distribution of grammatical equations in LIA. We see that there are very few grammatical equations that span many subgroups. Indeed, more than half are restricted to a single subgroup.



Such "grammatical" evidence as is presented is not very convincing. Many of Greenberg's examples involve independent words, especially pronouns, which he considers to be "grammatical", in spite of the fact that pronouns have long been recognized as a weak source of evidence (Meillet 1914/1926;89-90). Indeed, entries #22 and #23 are based exclusively on independent words. Even where true morphology enters the picture, it is almost always of the most superficial sort. There are few examples of ablaut or other idiosyncratic alternations, and comparisons are almost all of isolated morphemes, not substantial portions of paradigms.

In many cases the semantic relationship between the comparanda is extremely speculative, and the phonological resemblance is vague. In many cases (e.g. #103) the resemblance is between only a single segment in each language. Indeed, almost all of the morphemes discussed are extremely short, typically a single segment.

Finally, many of the morphological analyses on which his examples are based are extremely speculative if not completely unjustified (Goddard 1987, Campbell 1988, Adelaar 1989, Poser 1992).

As a revealing example of what Greenberg considers to be convincing evidence of genetic affiliation, consider his evidence for the membership of Waicuri in the Hokan family. Greenberg (1987;132) says:

> Waicuri is an extinct language of Lower California known only from a few forms, but these appear to be decisive for its Hokan affiliation.

The sum total of Waicuri evidence in LIA is found in the following four entries in the Hokan section of Chapter 3. No Waicuri data is cited in the "Amerind Dictionary" or in the chapter on grammatical evidence.

2 ALL

Jicaque *pʰu*. Subtiaba *ba:*. **Waicuri** *pu*.

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Chimariko *po*, *poi*. Chumash: Santa Cruz *k-opok* 'dead'. Esselen *poko*. Salinan: San Miguel *p-apa* 'copulate'. Subtiaba *g-ap* 'lie sleeping'. **Waicuri** *pibikiri* 'he died', *tibikiu* 'dead ones'. Yuman: Cocopa *patχ* 'lie down', Kiliwa *pi* 'die', Maricopa *epuik* 'dead', etc.

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Achomawi: Achomawi *ipʰle*, *iplaʔtaj* 'lick', Atsugewi *apʰli*. Chimariko *hipen*, *pen* 'lick'. Comecrudo *expen*. Jicaque *berang*, *pelam*. Karok *aprih*. Pomo: East, Southeast *bal*. Salinan: San Antonio *epa:l*, San Miguel *ipał*. Seri *ʔapł*. Tequistlatec *-apał*, *beł* 'lick'. **Waicuri** *ma-bela*. Yuman: Maricopa *hipañ*, Walapai *ipaal*, etc.

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Achomawi *wina* 'top'. Pomo: East *wina:* 'on top of', Northeast *wi:nal* 'straight up'. **Waicuri** *aena* 'above, heaven'.

In other words, Greenberg considers that he has made a "decisive" case for the Hokan affiliation of Waicuri on the basis of FOUR lexical resemblances and no morphological evidence whatsoever.

In sum, Greenberg's evidence consists primarily of superficial comparison of lexical items, with a limited amount of morphological evidence, none of it submerged, and much of it based on speculative analysis of the languages in question.

4. How the Indo-European Family Was Constructed

In contrast, a survey of Indo-Europeanists' claims about methods and their actual practice shows both that the recognition of languages as IE and the subgrouping of languages within the IE family have been based primarily on submerged morphology, and, especially in the case of subgrouping, secondarily on phonological isoglosses, not on superficial lexical comparison and isolated bits of superficial morphology.

Greenberg himself acknowledges the dominant role played by morphology in Indo-European (1987;36):

> ... in Indo-European it was the numerous points of specific contact in *morphological systems* that played the major role at an early stage ...

However, he and Ruhlen deny that phonological correspondences were considered of any importance on the grounds that regular sound laws were not

recognized until the Neogrammarians in the last quarter of the 19th century (Greenberg 1990;2-7, 1991;127-128, Ruhlen 1987;40-41,122). In point of fact, the use of sound laws to establish genetic affiliation goes back at least to Hadrianus Relandus who in his *Dissertationes Miscellanae* (1706-1708) used them to relate Malay and Malagasy (von der Gabelentz 1891;26). Such early Indo-Europeanists as Rask and Grimm were familiar with sound laws. After all, both of them discovered Grimm's Law.[1]

Greenberg and Ruhlen's claim confuses regularity with exceptionlessness. As Wells (1979;41) points out, the Neo-Grammarian controversy was not about the existence of regular sound laws; it was about whether the regular sound laws everyone acknowledged were exceptionless.

> Grimm, Pott, Diez, and Schleicher all taught the doctrine of the regularity of sound-change; but not until the next stage, the Neogrammarians, was regularity taken to mean exceptionlessness.

We turn now to a consideration of several examples, two of them discussed by Greenberg, but as a review of the history shows, wrongly interpreted.

4.1. Venetic

Venetic, the language spoken in the vicinity of Venice prior to the spread of Latin, known to us only from about 300 short inscriptions, mostly in the Etruscan alphabet, was recognized as a distinct language by Pauli (1885), who argued that Venetic was Indo-European on the basis of the case morphology and derivational affixes (1885;116-117). Pauli (1891;233) added an argument based on a weak/strong grade alternation in the same root, while saying that the entirety of his monograph would confirm his view that Venetic is IE. The explicit arguments in favor of an Indo-European affiliation were strictly morphological, although he also gave interpretations of words with obvious IE counterparts.

The next comprehensive work on Venetic was the 1949 monograph by Beeler, in which he gave the following summary of the evidence that Venetic is Indo-European (Beeler 1949;13), quoted by Greenberg (1990;13):

> Venetic is an Indo-European language. Some of the evidence which proves this point is the following: a) The contrast between the inflectional endings of two series of names, one with *-os, -oi,* and *-on* (like the nominative, dative, and accusative singulars respectively of IE *ŏ* stems), and the other with *a, as* and *ai* (like the nominative, genitive, and dative singulars of IE *ā* stems). b) The verbal ending *-to*,presumably that of the third person singular of the secondary indicative middle, Greek *το*, Sanskrit *-ta.* c) A large number of derivative suffixes, e.g. *-i̯o-, -no-,-so-,-tor-,* which can be abundantly paralleled in the languages of the IE family. d) Many striking lexical

correspondences, such as *·e·χo* = Lat. *ego*, *meχo* = Gothic *mik*, *zoto* = Greek ἕ-δοτο, *lo·u·zera·i·* = Latin *Libera*. e) The characteristically Indo-European nature of the vowel alternation in *vho·u·χo·n·tah* and *vhuχiia* (Pauli).[2]

Observe that four of the five pieces of evidence cited by Beeler are morphological, including facts about ablaut, not merely correspondences in affixes. Moreover, the evidence cited by Beeler in this passage is by no means all that he was aware of, as he explicitly indicates. In particular, Beeler established phonological correspondences between Venetic and Proto-Indo-European, and discussed them at some length (pp. 16-42).

In sum, the evidence offered for the IE affinity of Venetic was at first morphological and then extended to sound laws. Superficial lexical comparison played no role whatever.

The evidence adduced for the subgrouping of Venetic is also instructive. Pauli (1885;117) argued for a subclassification with Messapic as Illyrian, on the grounds that both languages had a genitive singular in *-h*,[3] and that in both languages the nominative singular of the present active participle retains the final /t/ while losing the nominative singular suffix /s/ (1885;117-118). Beeler's classification of Venetic as Italic (as opposed to Illyrian, the then current alternative) is based partly on morphology and partly on phonology, e.g. the fact that PIE **bh* yields *f*, as in Italic, in contrast to the *b* it yields in Messapic (p. 51).

Subsequent discussions of the subgrouping of Venetic, such as Krahe (1950) and Hamp (1959), have again concentrated on phonological and morphological isoglosses. At present the standard reference on Venetic is Lejeune (1974). Of the 54 isoglosses discussed, nine are morphological and 21 are phonological.

4.2. Hittite

The first substantive claim as to the affiliation of the Hittite language was made by Knudtzon (1902), Bugge (1902) and Torp (1902) in a book devoted to two letters between the king of Egypt and a Hittite ruler, found at Tell-El-Amarna in Egypt. Knudtzon, Bugge, and Torp argued that Hittite was Indo-European, largely on the basis of the morphology. An example is the following passage from Torp (1902;108):

> Die Annahme, dass hier eine indogermanische Sprache vorliege, scheint mir durch Knudtzon's Entdeckung von *eštu*, Imp. 3. Sing. des Verbs "sein", und von *mi* und *ti* als enklitischen Possessiven resp. der 1. und der 2. Pers. sehr nahe gelegt.

> The proposal that here we have an Indo-European language seems to me to be strongly suggested by Knudtzon's discovery of *eštu*, the

> third person singular imperative of the verb "to be", and of *mi* and *ti*, the enclitic possessives of the first and second persons respectively.

They pointed to a variety of other affixes, such as the accusative singular *-an* and the first person singular preterite active in *-n*.

Although Knudtzon, Bugge, and Torp were right, their proposal that Hittite was IE was generally rejected and it was not until the work of Friedrich Hrozný (1915,1917) that Hittite was generally acknowledged to be an Indo-European language.

Hrozný had at his disposal the vast quantity of Hittite tablets discovered at Boğaz-Köi, and as a result was able to produce a comprehensive grammar of the language and to justify his decipherment and analysis with numerous examples. He announced his results in Hrozný (1915), a paper that was soon followed by a book (Hrozný 1917). While the book constitutes a grammar of the Hittite language, the paper concentrates on his evidence for the Indo-European affinity of Hittite.

The evidence that Hrozný presented was largely morphological, including the form of the present active participle (p.23), the case morphology (p.24), the existence of r/n-stems (pp. 24-25), the pronouns (pp.25-26), the verbal paradigm (p. 27), and the adverbs (pp. 27-28). When he discussed the case morphology he did not present isolated affixes, but rather a full set of six case-endings. When he discussed pronouns, he did not present an isolated pronoun or two, but a set of 23, including multiple case forms of the same pronoun, some involving irregular alternations. When he discussed the verbal paradigm, he did not present isolated forms but rather the complete paradigm of six person/number forms, which are explicitly compared with their Vedic and Greek counterparts.

Indeed, it is clear that Hrozný did not consider isolated morphological resemblances probative. The first case to strike him was the present active participle. Nonetheless, this did not convince him immediately of genetic affiliation. He notes (1915;24,fn.1):

> Als ich die ersten Übereinstimmungen des Hethitischen mit dem indogermanischen fand, erwog ich auch die Möglichkeit dass das Hethitische vom indogermanischen vielleicht bloss beeinflusst worden sei.
>
> When I noticed the first correspondences of Hittite with Indoeuropean, I also considered the possibility that Hittite might just have been influenced by Indo-European.

Only after all of this morphological evidence did Hrozný tack on thirteen lexical comparisons.

We can now see why Knudtzon's argument had little impact while Hrozný's a mere decade later soon overcame all opposition. Not only was Hrozný's argument based on a much surer analysis of the language itself, but while

Knudtzon could offer only isolated affixes, Hrozný offered complete paradigms and idiosyncratic alternations. In Hrozný's paper there is not a hint of Greenbergian methodology: lexical comparison plays virtually no role, and the morphology invoked does not consist of isolated affixes.

The evidence offered in Hrozný's book included that presented in his paper and added to it. Since the book presented a grammar, however, the evidence was diffused throughout the book. There is no part of the book devoted solely to the argument for the IE affinity of the language. We therefore disagree with Greenberg's presentation of Hrozný's argument.

Greenberg (1990a;11-12,1991;129) quotes Hrozný (1917;vii) as follows:

> Everyone who wishes to interpret the Boghazköi texts, from the moment of their publication, will, like the author, come to the same conclusion on the basis of instances like the fact that *wadar* means "water", that its genitive is not **wadaras* but, remarkably enough, *wedenas*, that the Hittites have a participle in *-nt-*, that "what" (masc.) is *kuis* and in the neuter *kuid*, that "I" is *ug* (cf. Latin *ego*), "to me" *ammug* (cf. Greek *emoige*), "thou" *zig* (cf. Greek *suge*), "to thee" *tug* (Gothic *thuk* etc.), that the Hittite present is inflected *jami, jasi, jazi, jaweni, jatteni, janzi*, etc., etc.

On this Greenberg (1991;129) comments:[4]

> Hrozný does not present a table of correspondences of the kind that have become *de rigueur* in the pages of *IJAL*, nor has anyone since...Note also that the resemblances adduced by Hrozný as decisive are with various Indo-European languages or with none in particular as with the verb paradigm he cites.

However, it is only from Greenberg's English translation that comparanda for the verbal forms are absent. In the original German text (Hrozný 1917;vii), reproduced below followed by our own translation, Hrozný gives a Greek comparandum for every Hittite form.

> Jeder, der die Boghazköi-Texte, sobald sie veröffentlicht sind, wird deuten wollen, wird gleich dem Verfasser zu dem Ergebnis kommen, dass *wâdar* "Wasser" bedeutet; dass der Genitiv hierzu nicht etwa **wâdaraš*, sondern merkwürdigerweise *wedenaš* lautet, dass die Hethiter ein Partizipium Präs. auf *-nt-* haben, dass "welcher" bei ihnen *kuiš*, "welches" *kuit/d* heisst, dass "ich" hethitisch *ug* (vgl. lat. *ego*), "mir" *ammug* (vgl. griech. ἔμοιγε), "du" *zig* (vgl. griech. σύγε), "dir" *tug* (vgl. got. *þuk* usw.) lautet, dass das hethitische Präsens folgendermassen flektiert wird: *jami* (vgl. griech. τίθημι), *jaši* (vgl. τίθης), *jazi* (vgl. τίθησι), *jawéni* (vgl. τίθεμεν), *jatténi* (vgl. τίθητε), *janzi* (vgl. τιθέασι), usw. usw.

> Whoever wishes to interpret the Boghazköi texts, as soon as they are published, will, like the author, arrive at the result that *wâdar* means "water", that its genitive is not pronounced **wâdaraš* but, remarkably, *wedenaš*, that the Hittites have a present participle in *-nt-*, that "which" (masculine) was for them *kuiš*, "which" (neuter) *kuit/d*, that "I" took the form *ug* in Hittite (compare Latin *ego*), "me (dative)" *ammug* (compare Greek ἔμοιγε), "thou" *zig* (compare Greek σύγε), "thou (dative)" *tug* (compare Gothic *þuk* etc.), that the Hittite present was inflected as follows: *jami* (compare Greek. τίθημι), *jaši* (compare τίθης), *jazi* (compare τίθησι), *jawéni* (compare τίθεμεν), *jatténi* (compare τίθητε), *janzi* (compare τιθέασι), etc., etc.

More importantly, in this passage Hrozný is not, as Greenberg suggests, presenting evidence for the Indo-European affiliation of Hittite. As inspection of the German text will reveal, the various facts cited are not evidence for anything — they are what he considers to be firm conclusions about Hittite. Greenberg's mistranslation has transformed Hrozný's list of conclusions into a list of evidence for a conclusion. This interpretation is confirmed by the immediately following lines:

> Diese und die übrigen Ergebnisse des Verfassers stehen felsenfest da, man wird um sie nicht herumkommen können. Jeder neue Text, den der Verfasser neuerdings erhält, legt wiederum diese Deutungen nahe, fordert und bestätigt sie. So wird das hier angeführte hethitische Präsensschema durch viele Hunderte von Stellen gestützt. Dasselbe gilt z. B. auch von den für die Sprachvergleichung so wichtigen hethitischen Pronomina, deren Bedeutung durch eine unübersehbare Reihe von Stellen gesichert ist.
>
> These and the author's other results are so secure that they cannot be evaded. Every new text which the author obtains again suggests these interpretations, demands and confirms them. Thus the Hittite present tense conjugation proposed here is supported by many hundreds of facts. The same is true for example also of the Hittite pronouns, so important for linguistic comparison, whose meaning is assured by an unignorable series of facts.

In sum, Greenberg's discussion of this passage is entirely inaccurate: his translation is incorrect, he is wrong about the Hittite verb forms not being compared explicitly with forms from other IE languages, and the passage is not an argument for the IE affinity of Hittite. *Ex uno disce omnia*, as Greenberg (1990b;660) would say.

The passage in which Hrozný actually summarizes his reasons for believing Hittite to be Indo-European is the following (Hrozný 1917;v):

> Eine systematische, nüchterne und vorsichtige Prüfung eines grossen Teiles der in dem Konstantinopeler Kaiserlich Ottomanischen Mu-

seum aufbewahrten Boghazköi-Texte brachte indes den Verfasser bereits in wenigen Monaten zu der festen Überzeugung, dass das Hethitische eine im wesentlichen indogermanische Sprache ist. Wörter wie *wâdar* "Wasser" (vgl. altsächs. *watar* "Wasser" usw.), Gen. *wedenaš* (vgl. griech. ὕδατος aus ὕδητος), Partizipien wie *dân* "gebend" (vgl. lat. *dans*), Pl. *danteš* (vgl. lat. Pl. *dantēs*), Pronomina wie *kuiš* "welcher" (vgl. lat. *quis*), Neutr. *kuit/d* (vgl. lat. *quid*) usw. usw., wie auch, und zwar vor allem, der ganze im Laufe der Untersuchung sich allmählich ergebende Bau der hethitischen Sprache konnten keinen Zweifel darüber übrig lassen.

A systematic, sober, and careful examination of a large part of the Boghazköi texts kept in the Imperial Ottoman Museum in Constantinople however led the author readily in a few months to the firm conviction that Hittite is essentially an Indo-European language. Words like *wâdar* "water" (compare Old Saxon *watar* "water" etc.), genitive *wedenaš* (compare Greek ὕδατος from ὕδητος), participles like *dân* "giving" (compare Latin *dans*), plural *danteš* (compare the Latin plural *dantēs*), pronouns like *kuiš* "which (masc.)" (compare Latin *quis*), neuter *kuit/d* (compare Latin *quid*) etc., etc., and also, and indeed, above all, the entire form of the Hittite language which gradually reveals itself in the course of the investigation, can leave no doubt.

Here it is clear that Hrozný's emphasis is on the morphology, not the individual lexical items. That is why he cites pairs of related forms, including such distinctive items as an r/n-stem. In the chapter on *Formenlehre des Nomens* (The Morphology of the Noun) he refers again to the importance of the r/n stems for establishing the IE affinity of Hittite (p.61), and again, after a discussion of the declension of r/n stems like *watar*, he says (p.64):

Wir haben schon Mitteilungen d. deutsch. Orient-Ges. Nr. 56, S. 24f. bemerkt, dass diese Übereinstimmung in einer so auffälligen Deklinationsart — nebst vielem anderen — als ein zwingender Beweis für unsere These zu bewerten ist, dass das Hethitische eine indogermanische Sprache ist.

We have already remarked in the *Communications of the German Oriental Society*, Number 56, pp.24sqq, that this agreement in such a striking type of declension — among many others — is to be regarded as convincing proof for our thesis that Hittite is an Indo-European language.

Nor was Hrozný alone in his evaluation of the morphological evidence as crucial. Marstrander (1919;63) pointed specifically to the argument from r/n-stems:

M. Hrozný a réussi à établir une série de thèmes hittites anomaux en *r/n* et à fournir ainsi une des preuves les plus positives du caractère indo-européen da la langue hittite.

Mr. Hrozný has succeeded in establishing a series of anomalous Hittite stems in *r/n* and thus in furnishing one of the most positive proofs of the Indo-European character of the Hittite language.

And here is Gusmani (1968;7)'s comment on Hrozný's evidence fifty years later:

...accanto a tutta una serie di concordanze di carattero morfologica (desinenze ecc.), anche diverse coincidenze lessicali tra l'ittito e le altra lingue indoeuropee che dovevano corroborare la sua teoria del carattere indoeuropeo della lingua di recente scoperta.

...near a whole series of morphological correspondences (suffixes etc.), as well as various lexical correspondences between Hittite and the other Indo-European languages which should corroborate his theory of the Indo-European character of the recently discovered language.

Marstrander (1919;7) also emphasized the peculiar pronominal paradigm:

Sur l'origine indo-européenne de ces formes il ne peut y avoir aucun doute. Leur flexion montre la mê alternance particulière de thèmes que nous retrouvons dans presques toutes les languages indo-européennes. Que *u-ga* et *am-mu-ga* proviennent de la même source que ἐγώ : ἐμέγε, *ik : mik*, *ego : me*, cela saute aux yeux.

As to the Indo-European origin of these forms there can be no doubt. Their inflection shows the same peculiar alternation in the stem that we find in practically all of the Indo-European languages. That *u-ga* and *am-mu-ga* derive from the same source as ἐγώ:ἐμέγε, *ik:mik*, *ego:me*, that leaps to the eyes.

What it is essential to understand about Hrozný's book is that the argument that Hittite is Indo-European is not restricted to the facts mentioned in the single passage quoted from the Foreword. Hrozný's entire monograph is an argument for the IE affinity of Hittite — that is why its full title is "The Language of the Hittites: its Form and its Membership in the Indo-European Language Family", and why, in the passage quoted above, Hrozný cites as evidence "...the entire form of the Hittite language which gradually reveals itself in the course of the investigation...". The evidence is found throughout, in the many places in which he points out the relationship between some aspect of Hittite morphology and that of Indo-European. Among

many examples we may cite the two tables in Chapter 3 (pp.153, 162-3) in which Hittite verb forms are given along with their Vedic counterparts.

Let us now consider the matter of phonological correspondences. If one reads past the Foreword to chapter 5, pp. 186-190, entitled "Der Lautbestand des Hethitischen" (The Inventory of Sounds of Hittite), one finds a table of correspondences between Hittite and Proto-Indo-European. It is true that Hrozný did not offer this table of sound correspondences as the primary evidence for the IE character of Hittite — he clearly felt that the morphological evidence he cited was the most striking evidence of the relationship — but he did indeed work out and present phonological correspondences. Moreover, as the five exclamations "Centum-Sprache!" (*centum*-language) and the discussion on pp. 29-30 of his 1915 paper show, he used these correspondences to determine its place within the Indo-European family. It is just not true that Hrozný did not work out and make use of phonological correspondences, despite Greenberg's claims to the contrary.

Nor is it true that subsequent authors have not given such tables of correspondances. Marstrander (1919) gives a table showing the relationship between Proto-Indo-European and Hittite on page 169, and Sturtevant (1933) devotes much of Chapter III *Phonology*, pp. 87-143, to the sound correspondences between Proto-Indo-Hittite[5] and Hittite, at every point citing numerous comparanda in other Indo-European languages.

4.3. Armenian

We turn now to an example involving subgrouping rather than affiliation *per se*. Armenian was recognized as an Indo-European language by Petermann in 1837, and soon thereafter, in 1846, was classified as Iranian by Windischmann on the basis of the many obviously Iranian words in its lexicon. This remained the dominant view, accepted, among others, by Bopp, in spite of doubts expressed by Pott and the suggestion of DeLagarde that the Iranian words represented loans, until the publication of a classic paper by Hübschmann in 1875. Hübschmann demonstrated, to the satisfaction of virtually all scholars since, that Armenian belongs to a distinct subgroup of IE, not Iranian.

Hübschmann's discovery of the correct position of Armenian within the IE family was due to his recognition that words are so easily borrowed as to be poor indicators of genetic affiliation, vastly inferior to morphology (Hübschmann 1875;10):

> Sind wir nun gegen das lexicon misstrauisch geworden, so dürfen wir uns vertrauensvoller an die grammatik wenden: ist diese doch bei allen lebenden sprachen das palladium, das fremder einfluss nicht berühren kann. Wie wüst ist das lexicon im afghanischen und neupersischen, oder im englischen, und wie klar lehrt die grammatik, dass wir dort iranisch, hier germanisch vor uns haben!

> As we have now become distrustful of the lexicon, we must turn trustfully to the grammar: it is the palladium of all living languages, which is not subject to foreign influence. How confused is the lexicon in Afghan and Modern Persian, or in English, and how clearly the grammar teaches us that we have before us there Iranian, here Germanic!

He concluded that in its morphology Armenian exhibts no specifically Iranian features, differs in an important point with Indo-Iranian, and corresponds most closely to Balto-Slavic (p. 13).

The remainder of the paper is devoted to a detailed examination of the sound laws and the demonstration, on the basis of the sound laws, that two strata of Persian loans must be distinguished from the truly Armenian stratum, which exhibits very different correspondences. His ultimate conclusion is that Armenian is an independent subgroup of Indo-European, most closely related to Iranian and Balto-Slavic.

The first lesson that we draw from this example is that reliance on the lexicon is dangerous for we run the risk of being misled by loans. The second lesson is that phonological correspondences play a crucial role in distinguishing loans from native vocabulary. Note, moreover, that Hübschmann's appeal to sound laws preceded the Neogrammarians. Indeed, his paper appeared in the same issue of the *Zeitschrift für Vergleichende Sprachforschung* as the paper of Karl Verner's that set off the Neogrammarian revolution.

5. The Reliability of Superficial Lexical Comparison

We turn now to the second part of Greenberg's claim, namely the proposition that superficial lexical comparison produced reliable results when applied by the early Indo-Europeanists. We submit that those early Indo-Europeanists who did make use of such techniques were frequently led into error.

5.1. Sir William Jones

Sir William Jones is known to most linguists solely from the famous passage below in which he proposed the nucleus of the Indo-European language family (Jones 1798;422-423).

> The *Sanscrit* language, whatever be its antiquity, is of a wonderful structure; more perfect than the *Greek*, more copious than the *Latin*, and more exquisitely refined than either; yet bearing to both of them a stronger affinity, both in the roots of verbs, and in the forms of grammar, than could possibly have been produced by accident; so

> strong, indeed, that no philologer could examine them all three without believing them to have sprung from some common source, which perhaps no longer exists. There is a similar reason, though not quite so forcible, for supposing that both the *Gothick* and the *Celtick*, though blended with a very different idiom, had the same origin with the *Sanscrit*; and the old *Persian* might be added to the same family, if this were the place for discussing any question concerning the antiquities of *Persia*.

Greenberg (1949;79, 1990a;3) cites Jones as an example of a successful user of methods like his own. Jones' published work provides only the skimpiest evidence as to his methods, for he generally gave only his conclusions, not detailed arguments and data, but a careful examination of his work suggests that in one important respect his methods did indeed resemble Greenberg's.

Jones was aware of the possibility of borrowing, and that borrowing is especially likely in cultural and technological vocabulary. However, like Greenberg and some other modern scholars, he also did not recognize that massive borrowing was possible, or that even relatively basic vocabulary can be borrowed (Jones 1799a;54-55):

> I close this head with observing, that no supposition of a mere political or commercial intercourse between the different nations, will account for the *Sanscrit* and *Chaldaic* words, which we find in the old *Persian* tongues; because they are, in the first place, too numerous to have been introduced by such means; and secondly, are not the names of exotic animals, commodities, or arts, but those of material elements, parts of the body, natural objects and relations, affections of the mind, and other ideas common to the whole race of man.

As a result, he was ready to postulate genetic affiliation on the basis of large numbers of similar words. This was the basis for his conclusion that Romani is descended from Sanskrit, as it happens, a correct conclusion (Jones 1799c;8):

> It seems agreed that the singular people, called *Egyptians*, and by corruption, *Gypsies*, passed the *Mediterranean* immediately from *Egypt*; and their motley language, of which Mr. Grellmann exhibits a copious vocabulary, contains so many *Sanscrit* words, that their *Indian* origin can hardly be doubted.

Jones was also aware that grammatical correspondences provide stronger evidence of genetic affiliation than lexical correspondences (Jones 1799c;4):

> That the written *Abyssinian* language, which we call *Ethiopick*, is a dialect of old *Chaldean*, and sister of *Arabick* and *Hebrew*; we know

> with certainty, not only from the great multitude of identical words, but (which is a far stronger proof) from the similar grammatical arrangement of the several idioms.

In other words, Jones understood some principles of valid comparison, including the necessity of excluding loanwords and the value of grammatical evidence, but underestimated the possibility of borrowing.

Since, in spite of his recognition of the problem, Jones was not careful about excluding loans, since he did not establish phonological correspondences, and since in general he based his conclusions on fairly superficial comparison of languages, his methods led him astray in many cases. A particularly striking case is his misidentification of Pahlavi, an IE language of the Iranian branch, as Semitic (Jones 1799a;52):

> This examination gave me perfect conviction, that the *Pahlavi* was a dialect of the *Chaldaic*;[6] and of this curious fact I will exhibit a short proof. By the nature of the *Chaldean* tongue most words ended in the first long vowel, like *shemia*, heaven; and that very word, unaltered in a single letter, we find in the *Pazend*, together with *lailia*, night; *meyd*, water; *nira*, fire; *matra*, rain; and a multitude of others, all *Arabic* or *Hebrew*, with a *Chaldean* termination; so *zamar*, by a beautiful metaphor, from *pruning trees*, means in *Hebrew* to *compose verses*, and thence, by an easy transition, to *sing* them; and in *Pahlavi* we see the verb *zamruniten*, to *sing*, with its forms *zamrunemi*, I *sing*, and *zamrunid*, he *sang*; the verbal terminations of the *Persian* being added to the *Chaldaic* root. Now all those words are integral parts of the language, not adventitious to it like the *Arabic* nouns and verbals engrafted on modern *Persian*; and this distinction convinces me, that the dialect of the *Gabrs*, which they pretend to be that of *Zeratusht*, and of which *Bahman* gave me a variety of written specimens, is a late invention of their priests, or subsequent at least to the *Muselman* invasion.

Similarly, Jones mistakenly classified other Iranian languages as Semitic (Jones 1799c;7-8):[7]

> ...there is very solid ground for believing, that the *Afghans* descended from the *Jews*; ...and, principally, because their language is evidently a dialect of the scriptural *Chaldaick*.

Another language mistakenly identified as Semitic by Jones is Malay (Jones 1799c;10):

> As to the *Moplas*, in the Western parts of the *Indian* empire, I have seen their books in *Arabick*, and am persuaded, that, like the people

called *Malays*, they descended from *Arabian* traders and mariners after the age of Muhammed.

Jones apparently did not recognize that Malay was an Austronesian language, for he mistakenly regarded the Austronesian languages as Indo-European, specifically Indic (Jones 1799c;12):

> From the very accurate and interesting account of it by a learned and ingenious member of our own body, we discover, without any recourse to etymological conjecture that multitudes of pure *Sanscrit* words occur in the principal dialects of the *Sumatrans*. ...If Mr. Marsen has proved (as he firmly believes, and as we, from our knowledge of his accuracy, may fairly presume) that clear vestiges of one ancient language are discernible in all the insular dialects of the southern seas from *Madagascar* to the *Phillipines*, and even to the remotest islands, lately discovered, we may infer from the specimens in his account of *Sumatra*, that the parent of them all was no other than the *Sanscrit*.

Yet another non-Indo-European language wrongly regarded as Indo-European by Jones is Tibetan (Jones 1799c;13):

> ...for, although it [Tibetan] was anciently *Sanscrit*, and polysyllabick, it seems at present, from the influence of *Chinese* manners, to consist of monosyllables, to form which, with some regard to grammatical derivation, it has become necessary to suppress in common discourse many letters, which we see in their books, and thus we are enabled to trace in their writing a number of *Sanscrit* words and phrases, which, in their spoken dialect are quite undistinguishable.

Another case in which Jones failed to recognize a real relationship is that of Hindi, which he denied could be related to Sanskrit on the grounds that its grammar was typologically so different (Robins 1990;93).

To summarize, Jones mistakenly regarded Pahlavi, "Afghan", and Malay as Semitic, and Tibetan and the Austronesian languages as Indo-European while failing to recognize that Malay is Austronesian, and that Hindi is Indo-European.[8]

5.2. Franz Bopp

Our second example is Franz Bopp, also cited as a model by Greenberg. Deservedly famous for his work on IE comparative grammar, he was less successful in his judgments as to affiliation and classification. As we have already pointed out, he, like many others, was deceived by the large proportion of Iranian loans into classifying Armenian as an Iranian language. Here he fell victim to a failure to be sufficiently wary of loans.

Bopp also argued for the IE affiliation of the Malayo-Polynesian languages (Bopp 1840ab) and of Georgian (Bopp 1846). The consensus was and is that he was wrong about both. In these two cases his error was his ready acceptance of idiosyncratic relations between comparanda. For example, he proposed (1840a;172) that Malayo-Polynesian *po* "night" is to be related to Sanskrit *kṣapas, kṣapo*, with loss of the initial syllable. He did not, however, propose that this loss of initial syllables is of any generality.

Far from representing the Indo-Europeanist norm, Bopp's work on Malayo-Polynesian and Georgian was rejected by other scholars of his day and came in for severe criticism by the Neo-Grammarians. After praising Bopp's contributions to comparative grammar, Delbrück (1884;23-24) cited Bopp's work on Malayo-Polynesian as an example of his lack of a rigorous method and specifically criticized his failure to require regular phonological correspondences.

Similarly, in a passage extremely critical of methods like Greenberg's, von der Gabelentz (1901;164-168 — emphasis ours) specifically condemned the failure to require phonological correspondences and cited Bopp's work as an example of the sort of error to which it led. We cite here the beginning and end of a four-page diatribe on the question:

> Es ist schrecklich verführerisch in der Sprachenwelt umherzuschwärmen, drauf los Vocabeln zu vergleichen und dann die Wissenschaft mit einer Reihe neu entdeckter Verwandschaften zu beglücken. Es kommen auch schrecklich viele Dummheiten dabei heraus; denn allerwaerts sind unmethodische Köpfe die vordringlichsten Entdecker. Wer mit einem guten Wortgedächtnisse begabt ein paar Dutzend Sprachen verschiedener Erdtheile durchgenommen hat, — studirt braucht er sie gar nicht zu haben, — der findet überall Anklänge. Und wenn er sie aufzeichnet, ihnen nachgeht, verstaendig ausprobirt, ob sich die Anzeichen bewähren: so thut er nur was recht ist. Allein dazu gehört folgerichrichtiges Denken, und wo das nicht von Hause aus fehlt, da kommt es gern im Taumel der Entdeckungslust abhanden. So ging es, wie wir sahen, dem grossen Bopp, da er es versuchte, kaukasische und malaische Sprachen dem indogermanischen Verwandtschaftskreise zu zuweisen. Das Schicksal hatte es merkwürdig gefügt. Es war, als hätte er die Richtigkeit seiner Grundsätze doppelt beweisen sollen, erst positiv durch sein grossartiges Hauptwerk, das auf ihnen beruht, — dann negative, indem er zu Schaden kam, sobald er ihnen untreu wurde... Die Sprachen sind verschieden, denn die Lautentwickelung hat verschiedene Wege eingeschlagen. Hüben und drüben aber ist sie ihre Wege folgerichtig gegangen; darum herrscht in den Verschiedenheiten Ordnung, nicht Willkür. **Sprachvergleichung ohne Lautvergleichung ist gedankenlose Spielerei.**

> It is terribly seductive to roam the world of languages comparing words from them at random and then to bestow upon scholarship a

series of newly discovered relationships. Very many stupidities also result from this; for the most urgent discoverers have unmethodical minds. He who, endowed with a good memory for words, has gone through a couple of dozen languages from different parts of the Earth, — he need not at all have studied them —, finds familiar forms everywhere. And if he records them, investigates them, tests intelligently whether the indications pan out, he does only what is right. Only logically correct thought belongs here, and where it is not absent from the outset then he gladly gets lost in the giddiness of the mania of discovery. Thus it went, as we saw, with the great Bopp, when he sought to assign Caucasian and Malayan languages to the Indo-European language family. Fortune had decreed him a curious fate. It was, to have to prove the correctness of his principles twice, first positively through his magnificent main work, which is based on them, then, negatively, by coming to grief as soon as he was unfaithful to them... Languages are different because sound change has taken different paths. But it has gone its way consistently hither and thither; therefore Order reigns in differentiation, not Chaos. **Language comparison without comparison of sounds is irresponsible game-playing.**

Both Jones and Bopp were led astray by their failure to take sufficiently seriously the possibility of diffusion, and Bopp fell into error through his failure to require regular phonological correspondences. Interestingly, Jones did not suffer from this latter malady. While he exhibits no awareness of the existence or role of sound laws, he was well aware that idiosyncratic resemblances were unreliable, as he explained in a plea perhaps more deserving of immortality than the passage for which he is famous (Jones 1799d;431):

> ...I beg leave, as a philologer, to enter my protest against conjectural etymology in historical researches, and principally against the licentiousness of etymologists in transposing and inserting letters, in substituting, at pleasure, any consonant for another of the same order, and in totally disregarding the vowels ...I contend, that almost any word or nation, might be derived from any other, if such licenses as I am opposing, were permitted in etymological histories.

6. Conclusion

In sum, the classification of the Indo-European languages was accomplished by the techniques advocated by critics of *Language in the Americas* and other similar work, namely sound correspondences between items of basic vocabulary and grammatical correspondences, especially those involving submerged morphology. The methods used bear no resemblance to Greenberg's; in the rare cases in which such methods were used, they led to serious

error. Thus, Indo-European practice offers no support for methods like those advocated by Greenberg, Ruhlen, and other recent proponents of controversial language groupings, but rather a caution against their use.

Notes

[1]See Hoenigswald (1990) for a general discussion of early discoveries of sound laws and their use in establishing genetic affiliation.

[2]The raised dots in the tranliteration of Venetic reflect the practice in Venetic, as well as later Etruscan, of marking syllable-initial vowels and coda consonants and glides with one or two raised dots. The letters corresponding to the Greek aspirates are believed to reflect voiced stops (Sommer 1924), but the conventional transliteration of Venetic reflects Greek usage.

[3]On this point Pauli was wrong. As Sommer (1924) demonstrated, Pauli's understanding of the writing system was imperfect, and what he took to be genitives in *-h* are actually datives in *-i*.

[4]Similar comments are to be found in Greenberg, Turner & Zegura (1986;493), who give the quotation in abbreviated form, and in Greenberg (1990a;12).

[5]Sturtevant considered Hittite to be a sister of the remainder of the Indo-European family, rather than a daughter language, and referred to the parent of Hittite and IE as "Indo-Hittite".

[6]*Chaldaic* refers to the Semitic family, especially to Aramaic.

[7]It is not clear which language Jones refers to as Afghan, but the main languages of Afghanistan, Dari and Pashto are Iranian, as are nearly all of the others. No Semitic language is, or was in Jones' day, spoken in Afghanistan.

[8]While in some cases one might attribute Jones' error to his limited knowledge of the relevant languages, this cannot possibly explain his misclassification of Pahlavi. Jones was extremely well versed in Pahlavi and in the other forms of Persian, as well as Arabic, and devoted much of his career to the study and translation of Persian and Arabic literature (Cannon 1990).

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