

JENS ELMEGÅRD RASMUSSEN

HAERETICA INDOGERMANICA

A Selection of
Indo-European and Pre-Indo-European Studies

Det Kongelige Danske Videnskabernes Selskab
Historisk-filosofiske Meddelelser 47, 3



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To the memory of my teacher
KAJ BARR

Prefatory Note

The Indo-European studies contained in this collection are sprung from thoughts and theories that took shape in my mind during my years as a student of Comparative Linguistics at the University of Copenhagen through 1971. Their present form crystallized during my subsequent teaching at the Institute of Linguistics in the same city which forced me to weigh the pros and cons in a considerable number of theoretical issues. On many occasions I was led to seek the justification of a reconstructed paradoxal structure by engaging in the difficult pursuit of analyzing or re-analyzing stages of the linguistic history prior to the period commonly labelled Proto-Indo-European. In these endeavours the linguist must beware of a two-fold danger. On one hand, the advanced age of these linguistic strata makes it very difficult to submit them to methodical scrutiny, given the complete absence of comparative information. This entails, on the other hand, that studies of this kind are often looked down upon in not always quite (if partly) justified contempt, being dismissed as “fantastic”, “glottogonic”, or the like. It has been my privilege that the latter of these impediments has had little influence on my work. Copenhagen linguists have never been dogmatic, and I hope they never will be. Thus, even if I tentatively adopt the six-laryngeal system of Professor F. O. Lindeman, it merely means that I regard it as a very sober theory accounting for our present knowledge, and not that I ascribe any canonical status of “God’s Truth” to it, an attitude that I know I share with Prof. Lindeman himself. *Sans comparaison*, my theories of Pre-Indo-European linguistic phenomena are likewise to be regarded merely as attempts at assessing what our solutions to a number of problems would be like if we are justified in assuming that the hints we have for our hunches

are pertinent. Tentative research of this kind is no novelty in scientific studies. Nuclear physics and Freudian psychology are other examples of fields of study where the proportions of the conclusions often far exceed the precious material they were based on. The scope of such theories is of course to establish the highest possible amount of simplicity and coherence in the material studied, as is quite beautifully instanced by one of the most famous linguistic theories of this nature, Benveniste's theory of the structure of the IE root.

The kind help and the keen criticism of Professor Hans Hendriksen with whom I discussed the content of study No. I, has saved me many a pitfall. Professors L. L. Hammerich and Jes P. Asmussen of the Editorial Board kindly read the present work in its original heterogenous (Danish, English, and German) version and recommended it for publication in this tighter shape. My English was kept free of the worst barbarisms through the valuable assistance of my friend and pupil Mr. Gerard Muller. To all of these I wish to tender my sincere thanks also in this place.

The work is inscribed to the memory of Kaj Barr, whose pupil I consider myself. My firm rooting in the tradition of Holger Pedersen is due first and foremost to the guidance of Kaj Barr who represented this ideal of philological linguistics so well. It is my hope that the present studies will momentarily prove worthy of paying tribute to this tradition.

Roskilde, August 27th, 1973.

J. E. R.

I

Some Linguistic Universals Applied to Indo-European

It has been a matter of growing concern for linguists working in the field of Indo-European to see how often a structural pattern doomed impossible by the evidence of the “universals in language” was exactly congruent with the solution generally arrived at for Proto-Indo-European. The present paper will discuss a few of these strange cases where IE apparently shows features of a kind that languages are just not found to have. The exposé will aim at demonstrating that one such structural monstrosity need not worry us all the same, while a number of others can be done away with by a certain amount of analyzing.

1. The One-Vowel System and the Sanskrit Evidence

The most remarkable feature of IE as reconstructed by the more daring adherents to the “laryngeal” theories is the one-vowel system. One of Uspenskij’s rules¹ goes when read in words: “It is probably universally valid that languages do not have less than two different vowel phonemes”. Uspenskij cites three languages as possible exceptions, viz. Aranta, Abaza, and this very reconstructed Indo-European. For none of these this has remained undisputed, and so the non-validity of the rule is far from proven.

However, I think we may add one well-known language to the list, as I see no other way of analyzing the vowels of Sanskrit² than

¹ B. A. Uspenskij, *Strukturnaja tipologija jazykov*, Moskva 1965, p. 187 at the top. Uspenskij’s rules are given in a formulaic notation here transposed into plain words.

² Or perhaps rather of Old Indo-Iranian on the whole, the Avestan and Old Persian facts being, to the extent that they are known, in perfect agreement with the analysis here proposed for Sanskrit. I will not exclude the possibility that also the Luwian branch of Anatolian may comply with this analysis, the basic requirement being the coalescence of IE *e, *o, and *a.

to reduce the whole list to one vocalic element, plus a variety of consonants that the language has anyway.

Sanskrit has the following vowel phones: [a], [e], [o], [i], [u], [ā], [ī], [ū], [ṛ], [ṝ], [ḷ], plus the diphthongs [ai] and [au]. It can be shown that none of these contains any other vowel phoneme than /a/, as is seen from the phonemic interpretation of the vocalic graphs given below.

[a]	may be rewritten	/a/
[e]	→	/ay/, being the antec consonantal allo- phone of the [ay] found before vowels
[o]	→	/av/ as the antec consonantal alternant of [av]
[i]	→	/y/ as the antec consonantal alternant of [y]
[u]	→	/v/ as the antec consonantal alternant of [v]
[ā]	→	/aa/, cf. -a + a- > -ā- by synchronically active rule of sandhi and word- composition
[ī]	→	/yy/ since never opposed to [yi], [iy], or [yy]
[ū]	→	/vv/ since never opposed to [vu], [uv], or [vv]
[ṛ]	→	/r/ vocalized between consonants
[ṝ]	→	/rr/ never opposed to [rṛ], [ṛr], or [rr] ³
[ḷ]	→	/l/ vocalized between consonants
[ai]	→	/aay/, being the antec consonantal alter- nant of the [āy] occurring before vowels; furthermore, the rule -a + e- > -ai- of sandhi and word-com- position is then phonemically /-a/ + /ay-/ > /-aay-/
[au]	→	/aav/, being the antec consonantal alter- nant of antevocalic [āv]; again, -a + o- > -au- in sandhi and word- composition is phonemically /-a/ + /av-/ > /-aav/.

³ The famous counter-example *nīr-ṛī-* "Verderben, Todesgenie, Abgrund" has -s + r- with external sandhi treatment, not underlying /-rṛ-/.

In all of the above formulae the term consonant includes word-boundary. The vocalization of /r/ in preconsonantal initial position (*ṛtá-*) is thus perfectly regular, as are vocalized sonants in word-final position (neut. *śúci*, *mádhu* and the like).⁴

As is seen from the table, no Sanskrit vowel demands the acceptance of another purely vocalic element. Nor does this interpretation demand any consonantal phoneme not found in the language beforehand: Sanskrit has /y/, /v/, /r/, and /l/ as indisputable consonants anyway. What is more, with a few seeming exceptions to which I shall revert below, it appears that no matter how any of these ‘sonants’ are grouped together with or without the vowel /a/, there is always only one possible phonetic outcome. Thus for an underlying form /vyvyday/ the only possible reading is [vívide] (3rd sg.pf.mid. of *vid-* ‘find’) arrived at in the following way: The second /y/ is clearly anteconsonantal and so vocalized to [i]; this makes the second /v/ antevocalic and therefore consonantal; the resulting [v] vocalizes the first /y/, in front of which the initial /v/ is kept consonantal. No other solution would meet the requirements of the reading conventions of the underlying phonemes grouped in this way.

It is perhaps just a matter of taste how far the alternations within the language should be taken into account in phonemic interpretations like these. In the present paper this will be done in one further instance where it will prove useful. It must be maintained that the [ā] alternating with [i] and zero as it does in the verbal forms *á-dhā-t* : *dhi-ṣvá* : *da-dh-úr* (radical aorist 3rd sg.act. ind. and 2nd sg.mid.ipv., and pf. 3rd pl.act.ind., all from *dhā-* ‘put’) is a vowel different from the [ā] arising of, say, *a + a* in composition and sandhi. Now this alternation is open to further analysis: [ā] is the full grade (guṇa), [i] and zero are the anteconsonantal and antevocalic variants respectively of the zero-grade, just as *á-kar* : *kṛ-ṣvá* : *ca-kr-úr* (the same forms of *kṛ-* ‘do’, full grade *kar-*) alternate. As is seen (and it is a well-known fact in IE linguistics), the root *dhā-* is analyzable into the same structure as *kar-*, i.e. *CaC* alternating with *CC*. This analysis is achieved in rewriting [dhā] as /dhaX/, where /X/ is a consonant invented for

⁴ Such surface refinements as loss of syllabicity in word-final before vocalic initial (posterior to the period evidenced by the RV metrics, but prior to the written versions of the same text) fall outside the scope of the present paper.

a start just to account for this, but it will be argued below that we would need it anyway. About $/X/$ we know this: $/aX/$ is phonetically realized as a long vowel $[\bar{a}]$ before consonants, $/X/$ is $[i]$ between consonants and zero before a vowel. Thus we have two long vowels $[\bar{a}]$, one from $/aa/$, the other from $/aX/$.

An apparent opposition between $[u]$ and $[v]$ is seen in *uras* 'breast' and *uróh*, gen. of *urú-* 'broad', vs. *vrajati* 'he wanders'. All of these should be rewritten $/vra-/$ according to the above rules, and for one of the solutions $[ur-]$ and $[vr-]$, then, this analysis is wrong.

This riddle is easily solved if we assume that $[vra-]$ is the normal realization of initial $/vra-/$. Then $[ura-]$ must have had its $/v/$ vocalized between consonants, and we will have to assume an initial consonant in front of the $[u]$: $/Xvra-/$. The pronunciation is now fully in accordance with our rules concerning $/X/$: 1. It behaves structurally like a consonant, hence $/v/$ is vocalized between it and the consonantal allophone of $/r/$. 2. It goes to zero before a vowel, including vocalized sonants as in *da-dh-úr*, and so $[Xura-]$ yields $[ura-]$ in the end. This is much simpler than assuming something special for the case of *vrajati*, since we merely have to apply the rules accepted for the alternating root forms of *dhā-* above. Into the bargain we get an initial consonant in $/XvrvS/$, gen. $/XvravS/$ ⁵ ($>$ *urúh*, *uróh*) matching the prothetic vowel of Greek *εὐρύς* 'broad'.

Instances of a seeming opposition between $[i]$ and $[y]$ can be accounted for in the same way. The desiderative stem of the verb *yā-* 'go' forms an abstract noun *yīyāsā* 'desire to go', while the verb *i-* of like meaning has a perfect 3rd pl. *īyúr*. Here we clearly have antevocalic $[yiy-]$ opposed to antevocalic $[īy-]$ and it would be wrong to rewrite both as $/yyy-/$. This is readily solved if we consider a few other forms of the verbs in question. Of *i-* the imperfect has the 3rd pl. *áyān* and the perfect the quoted *īyúr*, both exhibiting a lengthening of a vocalic element prefixed to the radical (augment and reduplicative vowel). No such lengthening is seen in the corresponding forms of *yā-*, cf. 1st sg. ipf. *á-yām* (3rd pl. *á-yur*) and 3rd pl. pf. *ya-yúr*. Now we can account for this difference by writing in the $/X/$: *áyān* is then

⁵ $/S/$ is an archiphoneme covering the neutralization of $/s/$ and $/r/$ (in word-final position when not preceded by $/a/$).

$/a-Xy-an/$, and $\bar{y}úr$ is $/Xy-Xy-vS/$, while $\acute{a}y\bar{a}m$, $\acute{a}yur$, $yayúr$ are $/a-yaX-m/$, $/a-yX-vS/$, $/ya-yX-vS/$. Hereby we get the normal root structures $/Xay-/$ (for $i-$) and $/yaX-/$ (for $y\bar{a}-$), which in turn allows us to rewrite $yiyāsā$ as $/yy-yaX-saa/$. Thus, no two forms presuppose different pronunciations of the same underlying phoneme sequence.

With this in mind it will be tempting to analyze the problematic middle forms $\acute{y}ase$ and $\acute{y}ate$ (from $i-$) as thematized variants of reduplicated presents comparable to forms like $píbati$ (from $pā-$ 'drink') and $tīṣṭhati$ (from $sthā-$ 'stand'). The meaning of these forms circulates around 'go regularly', frequently used of Agni as the messenger to the gods, and so it seems justified to see an iterative or intensive force in the reduplication, as it has been pointed out by Elizarenkova⁶ for a number of verbs of this type. The underlying forms are then $/Xy-Xya-say/$ and $/Xy-Xya-tay/$ respectively.

This is of course merely the laryngeal theory stated synchronically. However, Sanskrit itself does here and there demand the positing of a non-pronounced consonantal element to account for such vocalizations as $súar$ $/sXvar/$ 'sun' (the initial cluster accounting for the disyllabic pronunciation in pursuance of the Sievers-Edgerton Law) and $tanúam$ $/tanvXam/$, acc. of $tanú-s$ $/tanvXS/$ 'body'.⁷ Some seeming anomalies thus become regular forms.

Hereby the last objections to the one-vowel system of Sanskrit are in my opinion ruled out. This in turn has its implications for Proto-Indo-European. It will be seen that it is methodically wrong to reject in IE pre-ablaut vowel system comprising nothing but the one element $*e$ as a sheer impossibility, claiming that the world does not know of any such language. We do have such a language, and it has been right here before our very eyes all the time.

⁶ T. Ja. Elizarenkova, 'Značenie osnov prezensa v Rigvede', *Jazyki Indii. Sbornik statej*, Moskva 1961, p. 91-165, especially p. 149. – The Rigvedic instances of $\acute{y}ase$ and $\acute{y}ate$ used of Agni as the messenger to the gods are the following: I.141.8, 145.1 (hardly passive as in Geldner's translation), II.6.7, III.3.2, 3.6, IV.2.2, 2.3, 7.8, 8.4, V.3.8, VI.15.9, 59.5, VII.3.3.

⁷ To my judgment, the dual terminations and individual words whose finals do not contract in sandhi have nothing to do with an underlying laryngeal, but rather represent special juncture phenomena due to certain emphatic particles attached to the endings of these forms, as described at some greater length in my paper on Gothic nam : $nēmum$, p. 39f. below.

2. The Indo-European System of Plosives

The IE system of stops as reconstructed in the traditional fashion with special attention to the Sanskrit evidence contains four modes of articulation, viz. $*t : *d : *th$. Now laryngeal theory has made it clear that $*th$ is in fact nothing but the $*t$ followed by a laryngeal. This leaves us with only three independent articulations: $*t : *d : *dh$. However, as pointed out by Roman Jakobson, no language is found to possess aspirated mediae without having at the same time the aspirated tenues of the respective articulations, $*th$ in the case of the dentals.⁸ Therefore, something is wrong with the series $*t : *d : *dh$, and its elements cry out for reinterpretation.

In 1957 Andreev tried to periodize the troubles away⁹ and posited the following successive stages in the development of IE itself:

1. Early IE	$TT : T : t$ (geminated emphatic: emphatic: plain tenuis) + laryngeal x
2. Intermediate IE	$T : t : th + x$
3. Late IE	$T : t : th + h$
4. Traditional IE	$t : d : dh + \text{ə} \sim \emptyset$

This only made things worse. No language is known to manage the two-fold opposition of intensity and of aspiration in its stops.¹⁰ This rules out stages 2 and 3. Equally unheard-of are three degrees of stop intensity as demanded by stage 1¹¹, nor does any language have an opposition of aspiration without possessing an h at the same time, which is just another reason for rejecting stage 2.¹²

⁸ Roman Jakobson, 'Typological Studies and their Contribution to Historical Comparative Linguistics', *Proceedings of the 8th International Congress of Linguists*, Oslo 1958, p. 23 = *Selected Writings* vol. I, 's-Gravenhage 1962, p. 528; Uspenskij, op. cit., p. 196 middle. See also the attempt undertaken by Christian Peeters (*KZ* 85, p. 164) at redefining IE $*bh *dh *gh$ in terms of distinctive features as "neither voiced nor voiceless and non stop". I think this may be right in so far as it means that the fundamental characteristic of $*d$ and $*dh$ was that of being lenes in contradistinction to the fortis $*t$.

⁹ N. D. Andreev, "Periodizacija istorii indoevropskogo prajazyka", *Voprosy Jazykoznanija* 1957, N° 2, p. 8.

¹⁰ Uspenskij, op. cit., p. 196 (at the top).

¹¹ This is the way I read Uspenskij's rule, op. cit. p. 195 at the bottom of the page, stating that if a given language has "an opposition of tenseness in its consonants" then it does not have "an opposition of consonant intensity".

¹² Uspenskij, op. cit., p. 191, middle.

One of the last writings from the hand of Holger Pedersen contained thoughts on previous stages of the IE system of plosives (1951).¹³ Pedersen drew attention to the blank in the system where a **b* is lacking. As *b*'s are not known to vanish more readily than other consonants, whereas many languages are known to have lost or weakened a **p*, Pedersen inferred that the phoneme lost was not an IE **b*, but rather a Pre-IE **p*. His argumentation is a sober one, and one cannot but accept his theory of a sound shift whereby Pre-IE (***p*) ***t **k . . .* were shifted to \emptyset **d *g . . .* with an empty space for **b*, because the older ***p* had already vanished before the shifting.

Pedersen went further in his argumentation and posited two more sound shifts: ***b **d **g . . . > *p *t *k . . .*, and ***ph **th **kh . . . > *bh *dh *gh . . .* But this is unacceptable, and the weightiest objection comes from Pedersen himself. Writing in 1904 on the relation between the West Armenian dialects and those of East Armenian, where one group has *b d g* corresponding to *p t k* of the other and vice versa, he stops to wonder, "Wie ist es möglich, dass tenuis zu media und media zu tenuis wird, ohne dass die laute unterwegs zusammengefallen wären? Meiner ansicht nach ist dies einfach unmöglich".¹⁴ This observation is ingenious and self-evident, and his attempts at getting around it in 1951 are very far from convincing. Therefore, if \emptyset **d *g* go back to (***p*) ***t **k* as they probably do, then IE **p *t *k* cannot be from older ***b **d **g*, nor can they of course have been ***p **t **k* all the time.

I then regard it as the simplest solution to derive **t *d *dh* from Pre-IE ***T **t **d* respectively, *T* being a cover-symbol for any emphatic stop however phonetically realized (glottalized, pharyngealized, or just stronger). The shifting can then be described as one single slide towards a weaker and looser articulation and does not presuppose simultaneous tendencies of incompatible natures. The opposition between emphatic and plain articulation becomes one between the voiceless fortes and the voiced lenes, while the contrast between voiced and voiceless becomes one of

¹³ Holger Pedersen, *Die gemeinindoeuropäischen und die vorindoeuropäischen Verschlusslaute*, Det Kongelige Videnskabernes Selskab, Historisk-filologiske Meddelelser, bind 32, nr. 5 (1951), p. 12-16, especially p. 16.

¹⁴ Holger Pedersen, 'Armenisch und die Nachbarsprachen', *KZ* 39 (1904), p. 337.

aspirated and non-aspirated articulation. This has the advantage of avoiding aspiration at the oldest stage, where we can then do without the /h/. Likewise we avoid the absurd situation of coalescence that would arise if we developed **d* from ***t* and **t* from **d*.

This leads us to positing the following Pre-Indo-European system of plosives, here presented together with their resultant Late IE counterparts:

$$\left. \begin{array}{l} P \ (p) \ b \\ T \ t \ d \\ \hat{K} \ \hat{k} \ \hat{g} \\ K \ k \ g \\ K^w \ k^w \ g^w \end{array} \right\} > \left\{ \begin{array}{l} p \quad bh \ (+ \ ph < PH) \\ t \quad d \quad dh \ (+ \ th < TH) \\ \hat{k} \ \hat{g} \ \hat{g}h \ (+ \ \hat{k}h < \hat{K}H) \\ k \quad g \quad gh \ (+ \ kh < KH) \\ k^w \ g^w \ g^wh \ (+ \ k^wh < K^wH) \end{array} \right.$$

Now Milewski has tried to demonstrate a sound shift for Hittite whereby **t* **d* **dh* should have yielded a two-fold opposition between an emphatic member *T* (from **t*) and a plain stop *t* (from **d* and **dh*).¹⁵ This in combination with the fact that no aspirated surds have developed from tenuis + laryngeal in Hittite, looks at first glance like a good argument in favour of the Indo-Hittite hypothesis. It will be seen that it is much easier to derive Hittite *T*: *t* from Pre-IE ***T* and ***t*/***d* respectively than to start from IE **t* vs. **d*/**dh*. Thus Hittite seems to presuppose the older stage of IE and has probably never known the intermediary stage traditionally labelled "Indo-European". However, the details and the implications are probably somewhat different from what is generally accepted by the adherents of this theory.¹⁶

¹⁵ T. Milewski, "La mutation consonantique en hittite et dans les autres langues indoeuropéennes", *Archiv orientální* XVII, pars II, 1949. The same theses are presented in the posthumous article "Die Differenzierung der indoeuropäischen Sprachen" in the *Lingua Posnaniensis*, vol. XII/XIII of 1968. It may be worth while to stress that Milewski was probably not right in his historical evaluation of the Hittite facts. Finding real or postulated sound shifts only in languages on the outskirts of the IE linguistic area, he inferred that these consonant changes must be due to influence from neighbouring non-IE languages. However, the usual relation between center and periphery is the opposite of this, and already on the surface of it it seems more satisfactory to see something old and well-preserved in these peripheral phenomena.

¹⁶ The Hittite sound shift conjectured by T. V. Gamkrelidze in the article 'Peredviženie soglasnyh v chettskom (nesitskom) jazyke', *Peredneaziatskij sbornik*, Moskva 1961, p. 211-291, is untenable. Gamkrelidze takes Hittite *-pp-*/*-bb-*, *-tt-*/*-dd-*, *-kk-*/*-gg-*/*-qq-* to represent aspirated stops: [*p^h*], [*t^h*], [*k^h*], developed from IE **p*, **t*, **k*. His arguments are above all based on spelling inconsistencies such as *tekissar* for the usual *tethessar*, *hameskanza* for *hameshanza*, and *É kilamni* for *É hilamni* (p. 244). These are as weak and inconclusive as are the forms of the paradigm of the word for 'water' presenting an unexpected *-tt-* for IE **d*, which Gamkrelidze rejects

It seems to me that Hittite is not the only language occupying such a lateral relationship, if not to IE then at least to the traditional system of plosives. It will also be simpler in the case of Armenian to derive the historical reality from our Pre-IE than by starting on purely traditional grounds. In that case

$P (p) b$	$h/\emptyset (p) b^h$
$T t d$ yield Armenian	$t^h t d^h$
$K k g$	$k^h k g^h$

while $*PH *TH *KH$ yield $p^h t^h x$.

The difference between the two Armenian series $h/\emptyset t^h k^h$ and $p^h t^h x$ cannot be derived from the traditional series “ $*p$ ” “ $*l$ ” “ $*k$ ” and “ $*ph$ ” “ $*th$ ” “ $*kh$ ”. Between “ $*p$ ” and h/\emptyset we would have to assume the intermediary stages $*p^h > *p^f > *f$, which would be the only natural line of development that I could think of. But stating at the same time that “ $*ph$ ” has remained an aspirated p is the same as asserting that the development of one phone has overtaken and passed another without leading to the coalescence of the two. We must think of a way whereby “ $*p$ ” and “ $*ph$ ” do not go through the same intermediary stage, and since we know that $*ph$ is from older $*pH$ it would seem justifiable to assume that this cluster did not become an aspirated stop until the development of “ $*p$ ” had already left this transitional stage. The development of Arm. h/\emptyset and p^h could then be traced as follows, dots denoting the unbroken retention of the older phonetic values:

(p. 247) because they “are found only in isolated instances in late texts and do not overthrow the picture of a regular spelling”. Secondly, the theory hinges on a few strange-looking etymologies like Hitt. *halzāi-* ‘call’: Greek *καλέω* (p. 243) and *harsanis*, gen. *harsnas* ‘head’: Skr. *śiras*, gen. *śirṣās* (p. 240). As the Hitt. counterpart of Greek *καλέω* is probably *kalles-* ‘call’, the first of these is probably wrong, and the etymologies operating with IE initial laryngeals are presumably normative also for these words. But there is one major reason why the theory of aspiration must be wrong. If the cluster $*tH$ before $*i$ becomes an aspirated $[t^h]$, written $-t/-dd-$, as is presumed by Gamkrelidze (p. 237), then the assibilation of $*ti$ to what is written zi must be older than the spontaneous aspiration of $*t$, which would otherwise have led to the coalescence of $*ti$ and $*tHi$ into either $[t^hi]$ or $[tsi]$. The development $*ti > zi$ is restricted to Hittite, the Luwian group preserving ti unchanged, and so Gamkrelidze is led to assume that the Hittite aspiration is a Hittite innovation not shared by the other Anatolian languages, and only on this assumption is his theory tenable. But the assumption is wrong. Sturtevant’s Law is as valid for Luwian as it is for Hittite: Luw. *happinatt-* ‘riches’: Hitt. *happinant-* ‘rich’ (cf. the p of Lat. *ops*), but *aduna* ‘to eat’: Hitt. *adanna* (cf. d in Lat. *edō*, etc.). Whatever the exact phonetic nature of the sound shift, then, it is of Common Anatolian date.

$$\begin{array}{l}
 h/\emptyset < f < p^f < p^h < \text{"}p\text{"} \\
 \dots\dots\dots p^h < \dots pH < \text{"}ph\text{"}
 \end{array}$$

We have, then, a contrast between p^h and pH , whatever the exact phonetic value of H . This places the retention of laryngeals relatively late in Armenian, and so we must ascribe to Pre-Armenian Indo-European a pronunciation of the laryngeals different from a mere $[h]$. Now, since the laryngeals had a stronger effect on $\text{"}^*k\text{"}$, which was spirantized to $[x]$, than they had on $\text{"}^*p\text{"}$ and $\text{"}^*t\text{"}$, which were merely aspirated, the laryngeal must have been a sound or a group of sounds better suited to influence a velar stop than it was to affect a labial or a dental one. As we were led to assume the stages $\dots [p^f] > [f] \dots$ in the development of $\text{"}^*p\text{"}$ it would seem to me that we are safe to assume the same for $\text{"}^*kH\text{"}$, i.e. $[k^x] > [x]$. This could be summarized in a sound-law stating that at some stage of the Armenian linguistic history homorganic groups of stop + spirant (affricates) were changed into spirants, whereas heterorganic groups show other results: $p^f > f$, $k^x > x$, but $p^x > p^h$ and $t^x > t^h$. This reveals the laryngeal that affected the $\text{"}^*k\text{"}$ as a velar spirant $[x]$. Whether Armenian had only one laryngeal at this early stage or a number of similar phones as well, is impossible to know. All we can say is that no indication exists of more than one such spirant.

Thus, Armenian presupposes the stage of Indo-European with retained velar fricatives, that is to say, the stage before $\text{"}^*bh\text{"}$ $\text{"}^*dh\text{"}$ $\text{"}^*gh\text{"}$ etc. and $\text{"}^*ph\text{"}$ $\text{"}^*th\text{"}$ $\text{"}^*kh\text{"}$ etc. developed out of $\text{"}^*b\text{"}$ $\text{"}^*d\text{"}$ $\text{"}^*g\text{"}$ etc. and $\text{"}^*P\text{"}$ $\text{"}^*T\text{"}$ $\text{"}^*K\text{"}$ etc. respectively. What is more, Armenian did not go through the stage $\text{"}^*bh\text{"}$ $\text{"}^*dh\text{"}$ $\text{"}^*gh\text{"}$ \dots $\text{"}^*ph\text{"}$ $\text{"}^*th\text{"}$ $\text{"}^*kh\text{"}$ \dots , and so this stage only developed in part of the IE languages. This amounts to stating the theory that the "Armenian sound shift" is not a sound shift at all, but rather a marginal retention of the old phonetic values, while the system of plosives required to account for the majority of IE languages represents a common innovation.

Now the similarity between the Armenian and the Germanic sound shifts has long been considered a strange coincidence. It is hard to see which of the two systems of plosives is presupposed by the Germanic facts. When Pre-IE $\text{"}^*T\text{"}$ $\text{"}^*t\text{"}$ $\text{"}^*d\text{"}$ develops into Gmc. p t d (or δ ?) there is little to prevent it from going through the traditional IE $\text{"}^*t\text{"}$ $\text{"}^*d\text{"}$ $\text{"}^*dh\text{"}$. The to-and-fro development of $\text{"}^*t\text{"} > \text{"}^*d\text{"} >$

t* is no less possible than, say, that of Pre-IE *e* > IE **e*: **o*: **a* > Indo-Iranian **a*. However, nothing really compels us to accept the transitional stage of traditional IE. We do not know with certainty that the Gmc. **t* was ever voiced or whether Gmc. **d* was ever aspirated. The assumption that Gmc. **d* was probably a spirant [ð], since the voiced alternant of **p* could come to coincide with it in pursuance of Verner's Law, does not require an aspirated **dh* of IE date any more than Gmc. **p* does an IE **th*. So it seems to me that we are free to choose between ***T* ***t* ***d* and **t* **d* **dh* for the origin of Gmc. *p t d*. Occam's razor then perhaps bids us to assume the unbroken retention of at least *t* if not of both *t* and *d* as a simpler solution than an all-round sound change.

I would then summarize the above ideas as follows. The traditional IE system of plosives, e.g. **t* **d* (**th*) **dh* can be held to represent a dialectal innovation upon the Pre-IE ***T* ***t* ***d*. This appears to be necessary for Anatolian and Armenian and a little bit simpler for Germanic than the traditional system. The old values of the stops were preserved in the marginal regions that did not take part in the innovation common to Indo-Iranian, Greek, Italic, Celtic, Balto-Slavic, and Albanian. Thus Anatolian and Armenian and possibly Germanic do not have any sound shift, being the only well-documented languages (possibly together with Tocharian) that did not participate in the Central Indo-European sound shift.

As I was working out the above I came across the article by V. M. Illič-Svityč on the "Sootvetstvija smyčnych v nostratičeskich jazykach", published in the annual *Étimologija* of 1966 (publ. 1968), and I was happy to see that the late leading figure of Soviet *nostratistika* had, on purely comparative grounds, arrived at the "Nostratic" system of plosives which he exemplified with the dentals **t* **t* **d* as the source of IE **t* **d* **dh*, **t* being an emphatic stop of alleged glottal coarticulation (and the origin of the pharyngealized emphatic *t* of Arabic as well). This is remarkably congruent with the system posited above for Pre-Indo-European, and though I claim no competence in Nostratic matters I gladly welcome this comparative analysis as some affirmation of my own analysis which was based partly on reasons of linguistic universals, and partly on a reconsideration of some seemingly inconsistent developments in Armenian.

II

Glottogonic Reflections on the Indo-European Personal Endings – in the Light of some Arctic Parallels

(Paper read before the Linguistic Circle of Copenhagen
on March 28th 1972)

The lecture that I intend to deliver to you to-night is, despite the bombastic title, in fact quite unpretentious. I readily admit that “glottogonic” speculations have an innate tendency of losing their way into the mists of uncontrollable hypothesis-making. It is to be admitted, too, that outside parallels supply only very weak evidence in matters of linguistic reconstruction. However, if treated with caution they constitute a source of inspiration that is not to be underestimated. It is precisely a manifestation of this caution when I present the case to this forum hoping that the subsequent debate will contribute to throw light on questionable points in what follows.¹

By “glottogonic” reflections I mean such reflections as seek to invent a linguistic system – a language type, if you like – out of which a known, preserved or reconstructed, linguistic stratum can be deduced by application of the common rules for the development of language systems (i.e. above all by sound-law, analogy, and derivation). The pivotal point is in the word “invent”, as we cast our minds back to a stage of the linguistic history which is not reached by comparative reconstruction proper. We find ourselves left with a more or less well-defined reconstructed Indo-European and want to know something about its previous stages. It is quite

¹ Many of the observations included in the following notes were inspired by this discussion, and I gladly acknowledge the help of the contributors, among whom special mention must be made of my distinguished teacher, professor F. O. Lindeman for the keen remarks that provoked notes 10 and 17 below.

possible that a Pre-Indo-European will eventually become accessible to normal comparative reconstruction when the posthumous "Nostratic" dictionary by Illič-Svityč is published in the near future.² The preliminary studies already published must be described as very promising, but are only concerned with the phonology. Although I must declare my incompetence in Nostratic matters I will find it interesting to see how well or how poorly the attitude expressed in the present paper will match the Nostratic findings. Until then Indo-European itself is all we have to work on.

We are, then, dealing with what has been called "internal reconstruction". One might as well speak of "typological" or "structural reconstruction", this approach involving the following clues:

1. The reconstructed proto-language may prove itself to be wrong beyond question by violating some universal law. If an as yet unheard-of phenomenon has been "reconstructed" we have grounds for the suspicion that further analysis centering around this point will be particularly rewarding.

2. Asymmetrical points of structure where a given form is isolated and "irregular" are in the main explainable by the assumption that the anomaly represents the last remnant of an old system given up by the rest of the forms.

3. Forms showing no synchronically functional interdependency may sometimes present mutual morphological correspondences of a nature that calls for an explanation. They may be the ruins of an old construction in which the units now redundantly marked entered into a meaningful network of interrelationships. If, e.g., a given language has more case-endings than case-functions, this may be due to the previous existence of a larger number of functional contrasting cases.

It is in cases like the last-mentioned that the working with parallels may have the greatest importance by demonstrating that the system postulated by this type of reconstruction can in fact be made up of the elements concerned. There is nothing particularly suspicious about linguistic parallels; they should just not be overestimated. Reconstruction of linguistic systems is normally

² The first part of it was published shortly after the present paper was read: *Opyt sravnenija nostratičeskich jazykov. Vvedenie. Sravnitel'nyj slovar' (b-ḫ)* (Moskva 1971). Its implications for the present paper are restricted to a single instance which has been commented upon in note 26 below.

practised without the adducing of parallels. However weak a verification there is to be seen in the possibility of citing languages exhibiting the same behaviour, it will never directly weaken the argumentation.

Previous stages of a given language do not necessarily show any typological harmony with the later historical development of that language, and in the case of Indo-European one must in a large number of cases go outside this linguistic family for inspiration as to stages preceding the immediate proto-language. It has already been demonstrated on several occasions that the linguistic type characterized by "ergative" sentence structure comes very close to the most reliable ideas of Pre-Indo-European. This is where we find the opposition of active and inactive in the noun as well as in the verb, just as it has been postulated for Indo-European³. Specimens of this type are Eskimo and Aleut, and the following is a series of observations done during the study of Greenlandic (together with other Eskimo languages and, on a more limited scale, Aleut), where I was struck by some further quite unexpected structural correspondences between these languages of Arctic America and Indo-European or its predecessor.

1. The pronoun "I" in Indo-European and Eskimo

The IE proto-form of the pronoun "I" may be reconstructed as $*\acute{e}g\acute{e}H_3m$ alternating with $*\acute{e}g\acute{H}_3om$, preserved in Greek $\acute{\epsilon}\gamma\acute{\omega}\nu$ and Sanskrit $ahám$ respectively.⁴ These forms are open to a certain amount of further calculation.

The alternating component is merely $*\acute{g}\acute{e}H_3- \sim *\acute{g}\acute{H}_3-$ which must therefore be the root entering into a base I and a base II respectively. The initial $*e-$ (perhaps to be refined to $*H_1e-$ if

³ C. C. Uhlenbeck, "Agens und Patiens im Kasussystem der indogermanischen Sprachen", *IF* 12 (1901), p. 170f.; the same, "Zur Casuslehre", *KZ* 39, p. 600-03; Holger Pedersen, "Neues und Nachträgliches", *KZ* 40 (1907), p. 129-217, esp. p. 151-3; the same, *Hittitisch und die anderen indoeuropäischen Sprachen* (København 1938), p. 83-5; N. van Wijk, *Der nominale Genetiv Singular im Indogermanischen in seinem Verhältnis zum Nominativ* (Zwolle 1902); *Ērgativnaja konstrukcija predloženiija* (Moskva 1950) (mainly translations of Western contributions); *Ērgativnaja konstrukcija predloženiija v jazykach različnyh tipov* (Leningrad 1967) (Russian contributions), esp. the articles of M. M. Guchman (p. 58-73), A. N. Savčenko (p. 74-90), and I. M. Tronskij (p. 91-4); I. M. Tronskij, *Obščeeindoeuropejskoe jazykovoe sostojanie* (Leningrad 1967), esp. p. 81f.; V. V. Ševoroškin, "K istorii indoeuropejskogo genitiva", *Voprosy jazykoznanija* 1957, No. 6, p. 89f.

⁴ On forms of this pronoun not matching these proto-forms, see note 10 below.

vocalic initial is excluded in the language) is probably the sentence connective known as the augment⁵ and as a fossilized initial element in conjunctions and pronouns like Lat. *e-t*, *e-quidem*, Greek *ἐ-κεῖνος* = Doric *κεῖνος*. The final **(o)m* may safely be identified with the homophonous verbal personal termination. We have, then, the structure *connective particle + root + personal ending*.

To identify the root, it will be natural to look among the pronominal stems for a root of the proper deictic nature. We must here bear in mind that IE **ǵh* – as well as the alternation **ǵ ~ *ǵH* – according to Ivanov⁶ develops into an Anatolian phoneme represented by Hittite */k/* and by zero in the Luwian dialect group. There is in fact an Anatolian pronoun exhibiting this alternation in initial position, viz. Hitt. *kās*, neuter *kī* “this one”, to which Hieroglyphic Luwian has *ī*.⁷ The deixis is that of the first person, so the adverbs Hitt. *kā*, Hier. Luw. *īti* mean “here”, the pronominal root being probably the same as in Lat. *hi-c*, *hī-c* showing the aspiration, while the palatal nature of the plosive is borne out by the particles Skt. *hi*, Avest. *zī* “namely”.⁸

⁵ C. Watkins has on several occasions identified the augment with the Luwian sentence connective *a-*, cf. *Indo-European Origins of the Celtic Verb. I. The Sigmatic Aorist* (Dublin 1962), p. 114; “Preliminaries to a Historical and Comparative Analysis of the Old Irish Verb”, *Celtica* VI (1963), p. 15; “Preliminaries to the Reconstruction of Indo-European Sentence Structure”, *Ninth International Congress of Linguists* (London, The Hague, Paris 1964), p. 1042; V. V. Ivanov, *Obščejindoevropejskaja praslavjanskaja i anatolijskaja jazykovye sistemy* (Moskva 1965), p. 244–9. This is rejected by Friedrich, *Hethitisches Wörterbuch*, 3. *Ergänzungsheft* (Heidelberg 1966), p. 49, but still retained by Watkins in *Idg. Gr.* III, 1 (Heidelberg 1969), p. 40. The theory is supported by the evidence of the Lydian particles *fa-* and *fak-* exemplifying the intermediary stage where the preverb *fa-* (*f-* before vowels) is used only if the sentence does not begin with the conjunction *fa(k)-*, cf. the interchange of *šnslibid* and *fšnslibid* in inscription no. 3 of Gusmani’s *Lydisches Wörterbuch* (Heidelberg 1964): *# a-k qis qisred # fa-k-aš silavad # fa-t nid šnslibid # a-k-m-λ / levš saretas # qis-it f-šnslibid esλ vanaλ | . . .* “and who spares (it, i.e. the burial chamber), and he takes care of (it), and does not damage it, to him / Levs (will be) gracious; but who damages this burial chamber . . . (Levs will destroy)”. The adverbial conjunction *f(a)-* is here treated as a preverb in that it sticks to the verb even when the latter does not occupy the initial position in the sentence.

⁶ *Symbolae . . . Kurylowicz* (Wrocław 1965), p. 130 ff.

⁷ A brief review of the material for this sound-law is given by Laroche, *BSL* 58 (1963), p. 79. To this should be added Lyc. *kbatra* “daughter” < **twatra* < **dhu-* + **-atr-* with zero for the **ǵ(h)* of Skr. *duhīl̥*, Gr. *θυγάτηρ*, cf. as correctly seen by Heubeck, *Die Sprache* VIII (1962), p. 86, and Laroche, *BSL* 62 (1967), p. 48.

⁸ This etymology is preferable to the one involving IE **ki-* (Lith. *šis*, Lat. *cis*, etc.), since the Luw. cases of zero for Hitt. *k* seem to contain IE **ǵh* or **ǵ ~ *ǵH* (the alternation of *ἐγών*: *ahám*: H.-Luw. *ī*; *θυγάτηρ*: *duhīl̥*:- Lyc. *kbatra*; *μέγας*: *máhi*, which is preferable to IE **mek-* as a match to Hitt. *mekki-*: Luw. *mai-*, Lyc. *miñti*; cf. Laroche, *BSL* 58, p. 79. Thus there is no need for the controversy described by Dunaevskaja, *Jazyk chettskich ieroglifov*, Moskva 1969, p. 74.

An exact parallel, i.e. a form of the pronoun "I" that is unquestionably to be interpreted as *pronoun of first-person deixis + personal ending*, occurs in Eskimo. In Greenlandic "I" is *uwanga* /*uaja*/, and from the Western Eskimo linguistic area one may cite Čaplinian⁹ *xwaja*. As far as the phonological history is transparent, one would accept these two forms as the proto-forms of East Eskimo and West Eskimo, respectively. The deixis of nearness inherent in this pronoun stems from the underlying demonstrative which otherwise appears as Greenl. *uwa-* "the one I'm pointing at", e.g. locative *uwane* "there, here"; likewise in Čaplinian we find the demonstrative exclamation *xwa* "look here! come here! now!" with the locative *xwani* "here where I am pointing". The termination is identical with the 1st singular ending of an intransitive verb of a principal clause showing *-ya* in all dialect areas: Greenl. *aki-vu-nga*, Čapl. *aki-qu-ya* "I pay" (*-vu-* // *-qu-* being the mark of the intransitive indicative).

We find, then, in IE and Eskimo a pronoun for the first person singular with the structure *demonstrative stem with deixis of nearness + personal ending of the 1st p. sg.*¹⁰ We shall now look at some further implications of this finding.

2. The First Person Singular in the Indo-European Verb

Besides the ending **-m*, IE possessed two further morphemes of the 1st p. sg., viz. **-ō* of the thematic present and **-a* (in laryngealist terms **-H₂o* or **H₂e*) of the perfect.

Eskimo, too, has at its disposal two more endings of the 1st sg., besides the **-ya* of the intransitive principal clause, viz. **-ka* (> Greenl. *-ga*, Čapl. *-ka*) of the transitive principal clause and

⁹ Main dialect of Siberian Eskimo. Čaplinian forms have been cited from the writings of G. A. Menovščikov, *Grammatika jazyka aziatskich ěskimosov*, I (Moskva-Leningrad 1962) (pronoun "I" p. 249f), II (Leningrad 1967); "Ěskimoskij jazyk", *Jazyki narodov SSSR*, č. V (Moskva-Leningrad 1967), p. 366-385. Rubcova's *Ěskimosko-russkij slovar'* (Moskva 1971) appeared too late to be used in drafting the present paper.

¹⁰ Some Eskimo dialects show irregular truncated by-forms: Naukanian (Siberia) and Kuskokwim (SW-Alaska) *wi* beside *wiŋa* (see Menovščikov, *Grammatika I*, p. 248). This is paralleled by such shortened IE forms as Old Lith. *eš*, Arm. *es* (preconsonantal sandhi variant of **ec* < IE **eĝ(-)*, cf. Meillet, *Esquisse*², p. 57). Greek *ěγώ* and Lat. *ego* (from older **egō* with "iambic" shortening) may be explainable in the same way as the verbal ending **-ō* discussed in the following. Hittite /*uk*/ seems to have inserted an enclitic particle **-u-* between the connective **(H₁)e-* and the truncated pronoun, the form **e-u-ĝH(om)* being parallel to Greek *οὔτος* (**so-u-tos*) and *ěκείνος* (**e-ke-enos*).

-*ma* (preserved in this shape all over the linguistic map) in the verb of dependent clauses. The two last-mentioned morphemes recur with an opposition of case as marks of possession in the noun ("my . . ."), *-*ka* being the inergative (also called "absolute", "intransitive", or "independent") and -*ma* the ergative ("relative", "transitive", or "dependent" in other terminologies). Thus, the relation between the three terminations is the following: -*ŋa* is intransitive and non-possessor, -*ka* is transitive and possessor of the intransitive, -*ma* is possessor of the transitive.

The other persons (discounting the 1st pl.¹¹) present no opposition corresponding to that of -*ŋa* and -*ka*, i.e. between non-possessor on one hand and possessor of the intransitive on the other. This system of three fundamental endings for the 1st person against two for the other persons is common to all Eskimo dialects known to me.

Likewise, IE has three different forms for the first person (*-*m*, *-*ō*, *-*H₂e*), but only two in the rest (*-*s*: *-*tH₂e*, *-*t*: *-*e*, *-*nt*: *-*r*), and it is natural to raise the question whether there exists any parallelism between the IE and the Esk. triads also in regard to *function*.

To investigate this question we shall tentatively accept an answer in the affirmative to see whether this assumption will lead us to further results.

The assumption of a syntactic correspondence must depart from the 1st sg., since only this shows a maximum differentiated picture in both linguistic groups. From the personal pronoun *-*e-ĝH₃o-m* = *xwa-ŋa* it appears that Esk. -*ŋa* is the functional counterpart of IE *-*m*. In that case *-*m* should be in origin the mark of complete intransitivity, used "absolutely" with no involvement of any other person in the verbal process. This appears bewildering at first glance, as it is the Hittite *hi*-conjugation, and not the *mi*-conjugation, that has won a certain amount of half-

¹¹ The forms of the 1st pl. are: Greenl. *aki-vu-gut* "we answered", *aki-va-r-put* "we answered him", *aki-ga-vla* "when/because we answered". The corresponding Čapl. forms end in (mode-sign and termination): -*qu-kut*, -*qa-ŋ-put*, -*ja-mta*. The three endings are analyzed as *-*ku-t*, *-*pu-t*, and *-*pta* (or *-*mta*) by Bergsland, *IJAL* 17 (1951), p. 169, 170, and 168, and by L. L. Hammerich, *Personalendungen und Verbalsystem im Eskimoischen* (København 1936), p. 164, 107, and 108. The details of the interrelationship of these endings are not clear. Moreover, as IE presents no corresponding triad this information would not be useful to our purpose anyway.

hearted acceptance as the true descendant of an old intransitive verbal category.¹² I shall, however, try to demonstrate that it is rewarding to stick to the correspondence with Eskimo and, in so doing, reinterpret the functional system postulated for Indo-European.

There remain now on the IE side the endings **-ō* and **-H₂e* and on the Eskimo side **-ka* and *-ma*. In IE this is tied up with an opposition of present and perfect (Greek *φέρω* and *οἶδα*), probably an old opposition between action and state.¹³ However, the only place in IE grammar where **-ō* is used to the exclusion of all other endings is the (thematic) subjunctive. Behind the opposition seen in the Vedic injunctive *bhāram* vs. the subjunctive *bhārā* we might, then, see a contrast between the verbal form of a principal clause and that of a dependent clause. This is exactly the difference between Eskimo **-ŋa* and *-ka* on one hand and *-ma* on the other, cf., e.g. Greenl. indicative *aki-vu-nga* "I answer, I pay", *ak-va-ra* (from **-R-ka*, the uvular spirant **-R-* marking the singular) "I answer him" of principal clauses as against *aki-ga-ma* (Čapl. *-ja-ma*) "when/ because I answered", *aki-gu-ma* (Čapl. *-ku-ma*) "when/ if I answer", transitive *aki-ga-v-ko* (< **-ka-m-ku*) "when/because I answered him", *aki-gu-v-ko* (< **-ku-m-ku*) "when/ if I answer him", the four last-mentioned forms belonging in dependent clauses. What is here described as the "verb of dependent clauses", is variously designated in Eskimo grammar as "dependent moods", "gerunds", or "causative and conditional" and there is a certain common consent to conceive of them as infinite forms. I will not insist on any one terminology, but merely stress the fact that they are inflected for person, the transitive

¹² Pedersen, *Hittitisch*, p. 83 ff. See also Watkins, *Idg. Gr.* III,1, p. 66.

¹³ The bibliography of this analysis is now considerable, cf. the literature mentioned in notes 3 and 12 above, to which should be added the following: Chr. Stang, *NTS* 6, p. 29–39; J. Kuryłowicz, *BSL* 33, p. 1–4 (both 1932), *Apophonie* (1956), p. 44, *Inflectional Categories* (1964), p. 61; T. Burrow, *The Sanskrit Language* (1955), p. 296; V. V. Ivanov, *Obščeeindoeuropejskaja . . .* (1965), p. 137; A. N. Savčenko "Problema proischoždenija ličnych okončanij glagola v indoeuropejskom jazyke", *Lingua Posnaniensis* VIII (1960), p. 44–56; the same, "Kategorija mediuma v indoeuropejskom jazyke", *BPTJ* XX (1961), p. 99–119; Jan Safarewicz, "Les désinences moyennes primaires de l'indo-européen", *Bull. intern. de l'Acad. Polonaise* (Krakow 1938), p. 149–156; the same, "Razvitie formativov vremeni v indoeuropejskoj glagol'noj sisteme", *Problemy indoeuropejskogo jazykoznanija* (Moskva 1964), p. 13–17; "Le présent indéterminé et le présent déterminé en indo-européen", *Symbolae . . . Kuryłowicz* (Krakow 1965), p. 246–254; I. M. Tronskij, *Obščeeindoeuropejskoe jazykovoe sostojanie* (Leningrad 1967), p. 88–91.

forms even for two persons, just like verbal forms of principal clauses.

Thus having equated IE $*-\bar{o}$ and Esk. $-ma$ we are left with IE $*-H_2e$ and Esk. $*-ka$, and our only possibility for maintaining the parallelism between the two sets of endings is now the assumption that $*-H_2e$ is the functional correspondence of $*-ka$.

The postulated parallelism may be tabulated as follows:

	Proto-Indo-European		Proto-Eskima	
	principal clause	dependent clause	principal clause	dependent clause
Intransitive	$-m$	$-\bar{o}$	$-ŋa$	$-ma$
Transitive	$-H_2e$		$-ka$	

The other persons are phonologically unclear (this is especially true of the 2nd p. sg.), or they have only two endings, one for the principal clause and one for dependent clauses. Specially clear is the 2nd p. pl.: Greenl. princ. cl. $-se$: dep. cl. $-use$, Čapl. $-si$: $-pasi/-fsi$. There can be no doubt that the dependent-clause form consists of the ergative casemarker $*-m-$ + the principal-clause form. In the first place *nuna-v-se* "(of) your country" is the ergative of the possessive form corresponding to the inergative *nuna-r-se* (not marked for case: $-r-$ signalizes the singular). In the second place, the situation is evidently the same in the Aleut 2. pl. possessive inergative $-čix$, ergative $-m-čix$: *ajxasi-čix* "your boat", *ajxasi-m-čix* (*uxasi-ŋis*) "(the oars) of your boat". $-m-$ is the mark of the ergative: *adax* "father", erg. *ada-m*. Instead of $-či-x$ which is in fact a dual form one would expect $-či$ (= Eskimo $-si$), and this is indeed the form found in Iochelson's and Veniaminov's¹⁴ materials, from where the following forms are cited. In gerundial (i.e. dependent-clause) forms we have the expected 2nd pl. $-m-či$, as in the periphrasis constituting the "near future": *su-m-či ari-ku-xtviči* "having taken, you will be" = "you are about to take". In the 1st sg. the forms are: possessive inerg. and principal-clause verb $-ŋ$, and unexpectedly also $-ŋ$ in the ergative of the

¹⁴ V. I. Iochel'son, "Aleutskij jazyk v osveščeni grammatiki Veniaminova", *Izvestija Rossijskoj Akademii Nauk* 1919, p. 144; the same, "Unanganskij (aleutskij) jazyk", *Jazyki i pis'mennost' narodov severa*, č. III (Moskva-Leningrad 1934), p. 135.

possessive and in dependent clauses. The ending *-miŋ* expected by Iochelson¹⁵ is indeed found, but only in the marginal function of personal ending with postpositions: just as the Aleut for “to the house” is *ula-m hada-n* with the ergative of *ula-x*, “to me” is *hadi-miŋ* with the ergative *-m-* well-preserved. Even if some details have become blurred we have sufficient remains to recognize the system with ergative suffixes containing the ergative mark *-m-*, these suffix forms being used also in the verbs of dependent clauses.

Analyzed according to this system the Eskimo 1st sg. ergative *-ma* must now consist of the **-m-* of the ergative + either **-ka* or *-ŋa*. It is somewhat complicated to see what might come out of an original **-m-ŋa* or **-m-ka*. We do know, however, that Esk. **mŋ* became Greenl. *ngm* (now pronounced [m:]) and Čapl. *mŋ* as in **kaməŋ*, pl. **kamŋət* “boot(s)” yielding Greenl. *kamik*, pl. *kangmit*, Čapl. *kamək*, pl. *kamŋət*. We know, too, that **km* gave Greenl. *ngm* ([m:]) and Čapl. *ŋm*, as seen from the 1st sg. poss. erg. of a dual noun, cf. Greenl. *nuna-ŋg-ma* (now obsolete), Čapl. *nuna-ŋ-ma* “(of) my two countries” formed with the **-k-* of the dual + this same *-ma*. In view of this, the alternative assumption that **-m-ka* should have developed to *-ma* is deprived of all probability, as it presupposes a more weakened result of **-mk-* than came out of **-mŋ-*. On the other hand, one must consider it very understandable if **-mŋ-* developed into *-m-*, even though we have no sure etymologies to point to, the group *-mŋ-* being a more homogenous cluster than *-mŋ-* and therefore expected to yield a more assimilated result than the *-ngm-* which the latter gave in Greenlandic (the pronunciation [m:] being a modern development of the [ŋm] attested by the Kleinschmidtian orthography). We know the product of **mn* to be *m* in all Esk. languages, cf. Greenl. *nuna-me*, Čapl. *nuna-mi* “(of) his own country” from **nuna-m-ni*, formed with the ergative **-m-* + the reflexive sg. possessive **-ni*, cf. the inergative Greenl. *nuna-ne*, Čapl. *nuna-ni* “his own country”. Thus, even without any direct attestation we find ourselves fully justified in rejecting **-m-ka* and accepting **-m-ŋa* as the reconstructible basis for the ending *-ma* of the 1st sg. of the dependent-clause verb and of the ergative possessive.

¹⁵ Iochel'son 1919, p. 148, and 1934, p. 137.

The Greenlandic series *-nga*, *-ga*, *-ma* (= Čapl. *-ŋa*, *-ka*, *-ma*) is thus seen to go back to Eskimo **-ŋa*, **-ka*, **-m-ŋa*.

If the functional equivalence between these forms and the IE endings **-m*, **-H₂e*, **-ō* (in that order) is realistic, we must now try to analyze **-ō* in the same way as we did **-ma*, i.e. as composed of the intransitive ending (which was **-m*) + a morpheme for the ergative case.

In this analysis we are helped by the IE morphophonemics. To my mind the phonological relation seen in Sanskrit between the verbal forms subjunctive *bhárā*, indicative *bhárāmi*, and injunctive *bháram* is exactly the same as the one obtaining in the *n*-stem paradigm between the nom. *rájā*, the acc. *rájānam*, and the voc. *rájān*. Just as the three nominal forms are undoubtedly to be derived from IE **rēǵō*, **rēǵon-m* (with **-o- > -ā-* in an open syllable¹⁶), **rēǵon*, one could deduce the verbal forms from IE **bherō*, **bherom-i*, **bherom*. In that case the subj. in **-ō* is seen to bear the same relation to the bare form of the inj. in **-om* as is found to exist between the nom. in **-ō* and the unmarked voc. in **-on*.

IE nominal stems in final sonant have in the nom. a long vowel taking the place of underlying vowel + sonant + **-s*, cf. Skr. *sákhā*, dat. *sákhye*, Greek *Λητώ*, voc. *Λητοῖ* (stems in **-oǵ-*), Skr. *pitā*, dat. *pitré*, Lith. *duktĩ* (stems in **-er-*). Other stems show in the nom. a lengthening that runs counter to the historically known sound-laws: Greek (Doric) *πός*, Lat. *pēs* (stems in short vowel + **d*). As is well-known, the ending of the nom. is merely **-s* (Skr. *śúci-s*, *sūnú-s*), and so the long vowel of the nom. must be the result of the encounter between the stem-final and the **-s*. This is apparently contradicted by such Skr. paradigms as *gáus*, gen. *gós* (**g^wōu-s*, gen. **g^wou-s*) and *dyáus*, gen. *dyós* (**dǵēu-s*, gen. **dǵeu-s*). But all things considered, this is an argument in favour of the analysis. As neither **g^wōu-s* nor **g^wou-s* can be interpreted etymologically in any other way than stem **g^wou-* + case-ending **-s*, their proto-forms cannot be contemporaneous. The nominative must be the older of the two, because its long vowel presupposes the operation of prehistoric sound-laws that have not been operative in the genitive.

¹⁶ A defence of Brugmann's Law which I hold to be right in some form or other, would go far beyond the scope of the present paper.

The most natural explanation of this discrepancy is that the two cases were one in a very old period of the language. This is the Pre-IE ergative, marked with a morpheme that normally became IE *-s, but under certain conditions produced a long vowel. This may be explained as a compensatory lengthening by assuming that the *-s, was at the old stage a more complex entity, e.g. an affricate or a cluster in the order of [ts]. It was not until this sound-law had ceased to operate that the ergative was split up in two cases by the creation of a new form for the novel adnominal possessive form in *-s or *-e/0 s. The old ergative form was then left with only part of its functional domain, namely that of marking the subject. This is the only way in which I can explain this apparent inconsistency in the phonological history allowing forms with long and short vowels without any etymological difference, and I take this as an argument in favour of the assumption that the somewhat peculiar nominatives with long radical vowel and loss of stem-final sonant do in fact continue the Pre-IE ergative case-form. Thus the nom. *rēġō of the nasal stems may in all probability be derived from an ergative *rēġon-s.

Likewise the subj. *bharā*, IE **bherō*, may be traced back to Pre-IE **bherom-s*, i.e. to the same case-form in *-s made from the verbal form **bherom*.¹⁷ The functional domain of the subjunctive

¹⁷ I do not consider the testimony of *m*-stem nominatives like Skr. *kṣās*, Avest. *zā* ‘‘earth’’ and Avest. *zyā* ‘‘winter’’ with preserved Indo-Iranian *-s crucial to the theory. Greek *χθών*, *λίωv* have the same nom. form as the **n*-stems (*κύων* etc.) which must be due to analogical identification of the two paradigms. The point of departure of this process must have been some case-form that incidentally turned out to be common to **n*-stems and **m*-stems. This can only have been the nom. sg., since the two nasals would be kept apart internally (as e.g. gen. sg. Skr. *śūnas* vs. *jmās*). The old **m*-stem nominatives were, then, **dhġhō* and **ġhġō* (or **ġhġē*) which have been variously reshaped in the individual languages: Indo-Iranian seems to have departed from the acc. where *kṣām* // *zqm* is probably the regular phonetic treatment of IE **dhġhom-m* involving the same kind of simplification of final sonant cluster as seen in **g^wow-m* > **g^wōm* and **diew-m* > **diēm*. The coalescence of this acc. formation with that of root-nouns like Skr. *vayo-dhām* ‘‘bestowing strength’’ and Avest. *mazdqm* entailed the analogical nominatives *kṣās* // *zā* on the model of *vayo-dhās* // *mazdā*. In Greek the secondary **n*-stem paradigm restored the nasal in the nominative: **χθω*, gen. **χθομος* → **χθ* ∪ *χθόνος* → *χθών χθόνος*. O Ir. *dú*, gen. *don* ‘‘place, earth’’ presents the same analogy to the **n*-stem type *cú*, gen. *con* ‘‘dog’’. Latin *hiems hiemis* probably developed from an old paradigm **hiē himes* (cf. Avest. *zyā zimō*) through the stages **hiēms* (nasal and *-s reinstated in the nom.) > *hiems* (Osthoff’s Law) with the analogical gen. **hiemes* > *hiemis*. A retention of the regular nom. form with loss of nasal and lengthening of the radical vowel is perhaps seen in Greek *δῶ*, gen. *δώματος*. The word is neuter, but the lengthened vowel appearing in *ῥδωθ* (as against Hitt. *watar*) and reflected in Arm. *awr* (as against Gr. *ἤμαρ*), is probably indicative of the situation that inanimate nouns, contrary to the general belief, could also form the ergative case, though apparently to a much more limited extent than animate nouns.

is a variety of more or less strongly modally shaded dependent clauses of the same type as the Greenlandic dependent moods or the Čaplinian gerunds (deepričastija).

We have thus arrived at the possibility – quite an acceptable one to my mind – that the structural relation between principal clause and dependent clause was the same in IE as it is in Eskimo and a series of other languages with ergative and polysynthesis, i.e. the relation of possessor and possession, the dependent clause being construed as the owner of the principal clause. Indeed, there is the same relation of government between principal clause and dependent clause as between property and owner. The second clause of *if I have time, I shall do it* is not any *I shall do it*, but only that special specimen of *I shall do it* that is characterized and delimited by the condition *if I have time*. It is, as it were, the statement *I shall do it* belonging to *if I have time*.

There remains the ending $*-H_2e$ of the perfect, which was tentatively identified with Esk. $*-ka$, the mark of the possessor of something intransitive. The verbal function of transitive form in principal clauses has not been preserved in IE, but it is easy to imagine how it was given up and replaced by the intransitive value of state as the whole category of possession disappeared from the endings of the IE inflectional system. Hereby the “owner of something intransitive” became merely “something intransitive”. On the analogy of the nominal possessive *nuna-ga* “my (inactive) country” ($*nuna-ka$) the verbal form *takuwara* “I see him” may be analyzed as “(this is) my inactive sight” ($*taku-var-ka$). Now, the semantic shift from “inactive sight” to “inactive seeing” with the emphasis on the process instead of on its object, is a very slight and commonplace development, and so the IE situation with $*-H_2e$ anchored in an intransitive situational category in fact presents no obstacle to our theory. It must be admitted, however, that this ending is no argument in itself; it is merely left over and passively fitted into the system to suit the argument.¹⁸ On the other

¹⁸ Elmar Seebold, in a highly speculative article received in this country immediately after the present paper was read, entitled “Versuch über die Herkunft der indogermanischen Personalendungssysteme”, *KZ* 85 (1972), p. 185–210, reviews the theory (p. 207) that identifies the IE opposition $*-m : *-H_2e$ with the Uralic opposition reflected in Hungarian $-m$ of the “objective conjugation” vs. $-k$ of the “subjective conjugation”. This may very well be correct as far as it goes. I would merely suggest the amendment that the IE triad $*-m : *-H_2e : *-ō$ represents a more original system than the Hungarian dichotomy. If there is any shred of truth behind the theory of “The Eskimo-Uralic Hypothesis” described by *inter alios* Knut

hand, the reconstructed form of the IE perfect (*-o-vocalism and two full grades) indicates that it has undergone post-ablaut reshapings with all the possibilities of semantic change this entails.

3. The IE System of Personal Endings

A sober reconstruction of the IE personal endings includes the series *-m, *-s, *-t, *-mé, *-té, *-nt of the system constituted by the prs. and the aor. and the series *-H₂e, *-tH₂e, *-e, *-mé, *-té, *-r as the basis of the system surviving as perfect and middle. Functionally the opposition is one between action and state or between active and inactive. The endings of the 3rd plural *-nt : *-r reflect the relation of two nominal stem-suffixes exploited in Anatolian to express the opposition ergative : inergative.¹⁹ The plural function is not original; the forms were much rather impersonal, as is known with certainty from the -r-endings of Italic and Celtic.²⁰

The only true verbal plural endings were, then, *-mé and *-té, where the correspondence in consonant content between *-m and *-mé of the two first persons spring to the eye. It seems that the plural form has been derived from the singular form by alteration of the accent resulting in different vocalizations also in the stem, so that **junéǵm* and **junǵmé* are in origin two different syncopation products of one and the same underlying form with fuller vocalization. Accent cannot be very old in the prehistory of IE, as it is not a member of the matrix of elements constituting different lexemes. IE could not form a new root with a new and unpredictable semantic content merely by changing the position of the accent. Prior to the genesis of the IE accent the two forms must, therefore, have been the same, i.e. merely a form for the first person irrespective of number, characterized by a morpheme consisting of the consonant *-m-.

The *-m- of the 1st person is in harmony with the pronoun **me*, plur. Lith. *mēs*, Arm. *mek* "we" from **mes*. The pronoun of the 2nd sg. is **te* (or **twe*) with the same consonant as the ending of the 2nd pl. *-té. To make the system stand out one must then

Bergsland, *Suomalais-ugrilaisen Seura Aikakauskirja* 61,2 (1959), this point of view is supported by the further testimony of the Eskimo three-fold opposition -*ga* : -*ka* : -*ma*.

¹⁹ E. Laroche, "Un 'ergatif' en indo-européen d'Asie Mineure", *BSL* 57 (1962), p. 23-43. See also Ivanov, *Obsčeeindoevropskaja . . .*, p. 51-54.

²⁰ J. Vendryes, *Celtica* 3 (1956), p. 185-197.

surmise an old ending $*-t$ of the 2nd sg. (or of the second person without specification for number).

The sporadic cases of 2nd sg. forms in $*-t(-)$ seen in Hittite and Old Slavic are explainable in different ways, either as transfer forms from the category of state or as pronominal accretions. In Hittite the termination of the *mi*-conjugation is *-s*, except when the verbal stem itself ends in *-s* in which case the 2nd sg. ends in the *-t* transferred from the *hi*-conjugation: *mazzasta* $|mazst|$ (from $*mat-s-t$) “thou stoodst thy ground”, *isparzasta* $|sparzst|$ ($< *spart-s-t$) to *ispart-* “escape”, *punusta* $|punust|$ to *punus-* “ask”. This is merely a commonplace phenomenon of dissimilatory selection without bearing on the IE situation. OChSl. *bystǫ* “thou wert, he was” may be from $*bhūs tu$ and $*bhūt tom$ and need not have anything to do with the postulated IE $*-t$ marking the 2nd sg.

It seems, however, that the ending $*-tH_2e$ of the perfect is itself influenced by the $*-t$ of the active. The Indo-Iranian 2nd pl. of the perfect terminates in *-a* which is isolated among the endings of the attested IE languages and has not been explained away as an analogical formation. This *-a* may indeed be identical with the $*-H_2e$ that was the form of the 2nd sg. ending before the $*-t$ of the action category influenced it to give the contamination $*-tH_2e$. To keep the opposition between the 1st and the 2nd persons, which are now both $*-H_2e$, we must assume the existence of two different phonemes both usually labelled $*H_2$, the most probable opposition being that of voice accepted by Lindeman.²¹ The 1st sg. termination must, then, contain the voiceless member of the opposition, since the laryngeal is retained in Hittite (Old Hitt. *-he* $>$ Hitt. *-hi* is from Anatol. $*-hai$, i.e. IE $*-H_2e$ of the “perfect” + the deictic particle $*-i$ of the present), whereas the 2nd sg. morpheme must contain a voiced laryngeal not retained in Hittite after $*-t-$ (Hitt. *-ti* must be from Anatol. $*-tai$, IE $*-tH_2e-i$; retention of the group *-th-* is seen in *tethessar* “thunder”). Even if this explanation of the $*-t-$ of the 2nd sg. perfect ending should not be correct, the $*-t-$ of the 2nd pl. $*-té$ will suffice to demonstrate the link between the pronoun $*te/ *twe$ and the verbal endings.

The usual 2nd sg. verbal termination of the category of action is, however, $*-s$. There is no 2nd sg. pronoun corresponding to this

²¹ *Einführung in die Laryngaltheorie* (Berlin 1970), p. 100f.

form, but it presents a perfect match to the reflexive pronoun **se/ *swe* (parallel to that of **-t* to **te/ *twe*), and it may indeed have originated in an old reflexive verbal form. In that case we have a remarkable parallel to the situation in Aleut exhibiting a typologically interesting syncretism of second person and reflexive. This does not stand out from the Aleut sketch by Menovščikov of 1967,²² but has been recorded by Iochelson in 1934²³ and by Bergsland in 1951.²⁴ We see this in, e.g., *tana-:n* “thy/ his own country (inergative)”, *tana-č̄i* “your/ their own country”. They even have the pronoun in common: *txin* (or *tin*) “thou, thee/ he himself, himself”, *t(x)idix* “you two / the same two, themselves”, *t(x)iči* or *t(x)ičix* “you (pl.) / they themselves, themselves”. Iochelson cites in 1919²⁵ the reflexive forms *txin-súnaḡ* “he took himself”, *txin-agúnaḡ* “he was born” with a hyphen, apparently to underline what he takes to be a special status of the first element different from the one exhibited by *txin súnaḡ* “he took thee”, *txin agúnaḡ* “she bore thee” which he writes without the hyphen. The forms are etymologically probably those of the 2nd person, judging from the correspondence of the pronoun *txin* with the Esk. termination **-tən / *-kən* (the former used after vowels: Greenl. *aki-vu-tit* “you (sg.) answered”, the latter after consonants: *aki-ga-v-kit* “when/ because I answered thee”). The Eskimo reflexive has the ending **-ni*, ergative **-mi* (underlying form **-m-ni*), and the Aleut syncretism may be due to the phonological merger of the old reflexive and the 2nd sg. in **-n*. But this does not alter the typological situation that the Aleut personal endings present a syncretism of a kind which would have been revolted against by the language system, had it not been supported by some sense of semantic identity. Indeed, the syncretism repeats itself in the dual and the plural where no phonological coalescence can be suspected. Until a better and more detailed philosophical explanation is brought forward I will tend to see the semantic justification in the imperative, where there is full identity in extra-linguistic denotation between the second person and the reflexive. Be this as it may, we

²² “Aleutskij jazyk” in: *Jazyki narodov SSSR* V, p. 386 ff.

²³ Iochel’son 1934 (see note 14 above), p. 137.

²⁴ IJAL 17, p. 170 (no. 174 = no. 172; no. 184 = 182); cf. the translations of morphemes no. 152 “you (sg.), himself”, 154 “you two, themselves”, and 155 “you all, themselves” on p. 169.

²⁵ Iochel’son 1919 (see note 14 above), p. 313.

may at any rate safely take the example of Aleut as a guarantee of the possibility of the assumption that the IE reflexive form took on the function of the 2nd sg., as reflexive forms grew superfluous. This they did, as the system with ergative and special dependent-clause forms gradually fell apart, judging from the Eskimo situation where only the verb of dependent clauses has a "reflexive" (i.e. recurrent third person) form.²⁶

Finally, a few words on the 3rd sg. The general trend in languages of the ergative structure (and in many others as well) is the bare zero. This seems indeed to be attested here and there in IE, cf. e.g. Greek *φέγει* made of the bare stem **bhere-* + the particle **-i* of the present and the Indo-Iranian precative *bhūyās*, whose zero ending is borne out by Avestan forms in *-yā*.²⁷ If Watkins is right in considering the **-s-* of the sigmatic aorist a generalized personal ending,²⁸ and also in seeing in this formation traces of a narrow affinity to the middle diathesis,²⁹ this **-s* may be the old mark of the reflexive, and the sigmatic aorist will then be an old reflexive category. As a dependent-clause form the reflexive would be further characterized by the ergative **-s*, the two **s*'s yielding the same lengthening of the preceding vocalism as they do in the nominative of **s*-stems. This explanation of the lengthened-grade vocalism is, however, tied up with some very complex problems of interlacing analogies which cannot be treated within the scope of the present paper; I hope that I shall have occasion to revert to them in the foreseeable future.

²⁶ Illič-Svityč, *Opyt sravnenija nostratičeskich jazykov* (Moskva 1971), p. 6 (with note 2 by V. A. Dybo) and 227 operates with an allophonic assimilation of Nostratic **ʃ* in the position before Nostr. **i*. However, his reconstructions *ti-* "thou" for Proto-Altaic, Proto-Uralic, and Proto-Dravidian (p. 6), if correct, exclude this as the origin of the IE verbal ending **-s*. Also Seebold (loc.cit., p. 191 f. and 197 f.) proposes to see an old alternation *ʃ ~ s* conditioned by factors that have later become blurred (unstressed *ó, ŋ, oí, zí*: stressed *τό, τόν, τήν* etc.?). It would be unwise to reject this as impossible; it should rather be kept in mind as an alternative solution giving fair competition to the theory expressed in the present paper.

²⁷ T. Burrow "The Sanskrit Precative", *Asiatica, Festschrift Weller* (1954), p. 35-42; the same, *The Sanskrit Language* (London 1955), p. 351 f.; Watkins, *Sigmatic Aorist*, p. 90-3.

²⁸ *Sigmatic Aorist*, p. 96 and *passim*. In Watkins' theory **-s* was a root enlargement before it came to be perceived as the mark of the 3rd sg. which was in its turn reduced to stem mark and extended to all persons. I am more inclined to take it as a *desinence* from the beginning, thereby explaining its lack of ablaut variation. Calling the **-s* an "élargissement" (rather than "suffixe") christens the problem, but it does not solve anything, as correctly seen by Kuiper, *Vedic Noun-Inflection* (Amsterdam 1942), p. 6¹.

²⁹ *Ibid.* p. 52-60.

The replacement of the naked 3rd sg. by a form in **-t* cannot be explained in all details, but the same phenomenon is seen elsewhere in IE grammar. A number of suffixes are renewed by the accretion of a **-t-*, cf. **-ero-* → **-tero-*, **-mmo-* → **-tmmo-*,³⁰ and verbal roots as the second member of compounds with an added **-t* of the type Skr. *iṣu-bhṛt-* “carrying arrows”, the **-t*-entailing in none of these cases any change on the functional level.

Thus, by drawing on our knowledge of ablaut and accent (which at this old stage of the linguistic history amounts to the justified total neglect of what we know of these phenomena for younger linguistic strata) and by adducing the pronouns, it is possible to analyze the IE personal endings in a way that is in somewhat fuller harmony with the situation we expect to find, when we view the matter from the standpoint of Eskimo and Aleut typology. It is on this basis I venture to see an old series of personal endings **-m*, **-t*, **-Ø* (zero), **-s*, marking the 1st p., the 2nd p., the 3rd p., and the reflexive, respectively, underlying the system that is more commonly reconstructed as **-m*, **-s*, **-t*.

Even if I may not have had the luck to convince anyone of anything else, I do hope to have demonstrated that linguistic parallels may occasionally take us further in the analysis of a reconstructed language. However problematic an analysis of this kind may be, the problems it raises are of a sort that is worthy of further investigation.

³⁰ Kuryłowicz, *Inflectional Categories*, p. 236.

III

Gothic *nam* : *nēmum* and the Indo-European Reduplication

From the beginnings of Comparative Indo-European Linguistics the alternation *a* : *ē* of the preterite of Germanic class IV and V strong verbs has been a hard nut to crack. This problem and its further implications will be dealt with below.

The singular forms *nam* and *qap* are of course the unadulterated descendants of the IE *o*-grade perfect. To be able to assess the relation of the plural forms *nēmum* and *qēpum* to this paradigm we are in need of a source of inspiration. Our attention is now first attracted by such Latin perfect forms as *fēcī* and *cēpī*. Here, too, instead of *o*-grade and reduplication we have a form with long *-ē*. We know that an earlier form of *fēcī* was *fhe fhaked* with reduplication and zero-grade root vocalism. This observation turns our thoughts in two directions, to the reduplicated aorist type of Skr. *āvocat* (**e we-uk^we-t*) on one hand, and to weak-stem perfect forms like Skr. *cakrē*, *cakrūr* on the other hand. Of these two comparisons the latter is undoubtedly preferable to the former, since the Germanic preterite is in all essentials based on the IE perfect and this presented zero grade in the forms of the plural.

It is precisely in the zero-grade forms of the perfect that we find a striking, if independent, parallel in Sanskrit. Forms like *tāpati* : *tatāpa* : *tepé*, *tepūr* presenting the structure *tep-* instead of the regular **ta-tp-* enable us to judge the Germanic forms with *-ē*.

It is beyond question that the traditional view on the Skt. forms ascribing the *-e-* to the analogy of such phonetically regular forms as *yemūr*, *yemé*, *sediré*, is correct.¹ Once **sa-sd-* had become *sēd-*

¹ See, e.g., Thumb-Hauschild, *Handbuch des Sanskrit*, II Formenlehre, Dritte . . . Auflage (Heidelberg 1959), p. 286 f (§ 522); L. Renou, *Grammaire de la langue védique* (Lyon-Paris 1952), p. 277; or T. Burrow, *The Sanskrit Language* (London 1955), p. 341 f.

a new type of perfect formation was established. The new structure, synchronically describable as the substitution of long *-e-* for radical *-a-* and lack of reduplication, is only exhibited by those rather few forms of the verbs in question that are otherwise formed by means of reduplication and zero grade. This makes the *-ē-* an unusual – and therefore successful – mark of the weak perfect stem of verbs that do not change their initial in reduplicating (by palatalization, deaspiration or otherwise), the underlying structure $C_1\alpha-C_1C_2-$ being the only one leading to the surface structure $C_1\bar{e}C_2-$ through the substitution described.

Another parallel is furnished by Old Irish where the future formation of the type seen in *·béra* (instead of expected **bebra* from **bi-ber-ā-t*) is clearly due to the analogy of phonetically regular forms like *·géna* (from **gi-gen-ā-t*).²

We revert now to the Germanic forms. Our task is here to invent a phoneme sequence that would result in a long *-ē-*, thus yielding the model for the other verbs. In other words, what is C_1 in the equation

$$\text{IE } *C_1e-C_1C_2\text{-}m\acute{o} \text{ (or } *m\acute{e}) > \text{Germ. } C_1\bar{e}C_2um?$$

We know that the sequence **eH₁* yields a long *ē* in preconsonantal position, and the C_1 of our formula may then be the laryngeal **H₁*. We know, too, that the verb *niman* originally had no initial *n-*, seeing that the correspondence with Lat. *emō*, OIr. *do-n-eim* ‘‘protects him’’, Lith. *imù*, and OChSl. *jьmŕ* can only be retained on this assumption. The *n-* must have been carried over from preverbs, the formation being comparable to that of OChSl. *vъnęti*, *vъnъmŕ* and *sъnęti*, *sъnъmŕ* from *vъ* or *sъ* + *jęti*, *jьmŕ*. In these forms a nasal that is absent before consonant or pause has been retained in prevocalic position: *vъ* is probably IE **on* (ablaut variant of **en*, **en-i* ‘‘in’’), while *sъ* matches a variety of possibilities, the most likely one being perhaps IE **som* (Lith. *sán-dara* ‘‘structure, syn-thesis’’) with **kom* and the etymon of Greek *ξόν* running close behind. *Mutatis mutandis* OChSl. *sъnъmŕ* and Goth. *ganima* are thus congruent. Germanic **nema* ‘‘I take’’ is, then, analyzable as **n-ema*, and in like fashion **nēmum* ‘‘we took’’ may be segmented into **n-ēmum*, the truly verbal part **ēmum* going back to IE **H₁e-H₁m-mó* (or **-mé*).

² Rudolph Thurneysen, *A Grammar of Old Irish* (Dublin 1946), p. 414.

Once the accretion of the nasal had led to the forms **nema*, **nam*, **nēmum*, this verb had acquired the same structure as the majority of other verbs, i.e. CVC-, and could serve as their model. Exactly as in Sanskrit **tatpé* conformed to the pattern of *sedé*, Germanic **kwekwum* was changed to **kwēmum* on the analogy of **nēmum*.

Nothing of the sort happened in the singular. It is absolutely impossible to explain Gothic forms like *nam*, *qam*, or *qap* as reduplicated. Assuming loss of reduplication would perhaps solve the problems of most verbs, but, apart from being a hypothesis concocted solely *ad hoc*, it would fail to account for the form *nam*. One would have to assume that **(n)eman* was made the model of the other verbs only in the plural of the preterite, whereas the singular was changed so as to agree with the normal type seen in *qam*. However, a reduplicated singular form **H₁e-H₁om-H₂e > *eoma* (or the like) would definitely undergo a contraction already in the period of the proto-language and result in a structure no less characteristic (and probably no less successful) than that of **nēmum*. The theory of dereduplication is, therefore, best given up.

The only remaining possibility is, now, that *nam* was never reduplicated. In that case we have in the sg. of the pf. forms like **H₁om-H₂e* which gave Gmc. **am*, whence, with the nasal carried over in sandhi, the attested form *nam*. The corresponding plural form presented reduplication: **H₁e-H₁m-mó > *ēm⁰m > *ēmum*, with sandhi nasal *nēmum*. Correspondingly, all Germanic strong preterites of classes I to V must have been unreduplicated in the singular since IE times, whereas the plural forms of classes IV and V were regulated by a reduplicated pattern, a clear indication of their former truly reduplicated nature.

What, now, from the standpoint of linguistic history, are we to do with a paradigm consisting of an unreduplicated singular and a reduplicated plural? First of all, perhaps, we ought to look for parallels, and in Gothic itself we find an interesting counterpart in the endings of the weak preterite. In forms like *salbō-da : salbō-dēdum* we observe a relation very similar to what we see in *nam : nēmum*. We are now practically forced to test the theory that Gothic, in contradistinction to the other Germanic dialects, preserves an archaism and allows us to see the scant remains of an old system where verbs only reduplicated in the plural.

In the relation between accent and ablaut we do occasionally find peculiarities that may be considered traces of such a system. If we take Skr. *dádhāti* from **dhé-dheH₁-ti* to be a well-preserved form dating back to the creation of ablaut, we do not understand why the unaccented radical vowel is retained with full grade. Conversely, if we assume a shift of accent, i.e. a still older form **dhe-dhéH₁tī*, we are at a loss to explain the retention of the reduplicative vowel. It seems justified to infer from this that reduplication is a secondary feature of this form, and the proto-form to be reconstructed for the 3rd sg. of this verb is merely **dhéH₁-t-i*. In the 3rd pl. form *dádhati* from **dhé-dhH₁-nt-i*, on the other hand, we witness full agreement between full grade and accent in the reduplicative syllable (**dhé-*), and between zero grade and lack of accent in both root (**dhH₁-*) and ending (**-nt-*).

An interesting example is supplied by the Indo-Iranian perfect forms of the root *bhū-*, where I would take the discrepancy in the vocalization of the reduplicative syllables of Ved. *babhūva* and Av. */bubāva/*³ as an argument in favour of the theory that the sg. forms were originally unreduplicated, while the agreement between Ved. *babhūvūr* and Av. */bābuvar/*⁴ (discounting the Ved. accent and the Av. vowel-length that are both obviously secondary) indicates a relatively higher age of the reduplication of plural forms.

The IE paradigm of the period immediately following the earliest ablaut changes must have contained the following forms:

- 1st pers. **bhūH-H₂é*
 2nd pers. **bhūH-(t)H₂é*
 3rd pers. **bhūH-é*
 impers. **bhé-bhūH-ŕ*

This is the ancestor of the oldest form of the middle preserved in such forms as Ved. *á-duha*. The Late IE perfect singular with *o*-vocalism represents an analogical introduction of full grade into the sg. in imitation of the present and aorist forms coupled with the additional mark of the *forme fondée* constituted by the change

³ Written *buuāuua* which covers the Younger Avestan counterpart of a non-attested Gāthic **bubāva*, as correctly seen by Strunk, *KZ* 86 (1972), p. 21. Phonemically, however, I would interpret both forms as */bubāva/*.

⁴ Strunk, *ibid.*, p. 22.

of vowel timbre (whatever the exact nature of the phonetic processes involved in this change).⁵

The resulting pf. paradigm must, then, have contained the following characteristic forms:

1 st sg. *bhóuH-Ha	1 st pl. *bh ₀ -bh <u>u</u> H-mé
3 rd sg. *bhóuH-e	3 rd pl. *bhé-bh <u>u</u> H-ŕ

This is the paradigm presupposed by the Old Indo-Iranian patterns derived from it each in its own way.

In Indic, the reduplicative vowel was generalized in its accented form, the radical vocalism, on the other hand, in its unaccented form, the structure *babhū-* consequently running through the paradigm. In the 3rd pl. the accent was shifted to the ending giving *babhūúr*, probably on the analogy of the present accenting *diviṣ-ánt*. The minor adjustments seen in *babhūvimá* (for expected **babhūmá*) and *babhūúr* (for **babhuvur* from **bhe-bhuH-ŕ*) represent commonplace analogical levellings falling outside the scope of the present paper.

In Avestan, the known forms show no generalization of a special vowel quality of either reduplication or root. In the sg. the radical accent entailed the reduced vocalism of the reduplicative syllable, **bh⁰*- being realized as **bhu-* in the lip-rounded environment. The long radical vowel of *|bubāva|* is due to an analogical extension of Brugmann's Law, whereby the phonetically regular difference between 1st sg. *cakára* (**-k^wor-H₂e*) and 3rd sg. *cakára* (**-k^wor-e*) is reproduced by **|bubava|*, *|bubāva|*, though both of these forms should be expected to have a short radical vowel arisen in an originally closed syllable (**-bhuH-H₂e*, **-bhuH-e*). No satisfactory explanation has been advanced of the lengthened reduplicative vowel of *|bābuvar|*; important to our purpose is only that it may safely be taken as an indication that the reduplicative vowel was accented in this form.

Thus, the Indo-Iranian paradigms presuppose the existence of an unreduplicated sg. which, as is well known, is directly attested in OIr. *boí* "he was".

⁵ Jerzy Kuryłowicz, *L'apophonie en indo-européen* (Wrocław 1956), p. 45. Though this theory beautifully maps the attested facts, I fail to see exactly how the coalescence of **e* and **o* into **o* in weakened syllables could entail the *o*-grade of the perfect.

If all these facts are in any way interrelated they are most easily explained by the assumption that reduplication in Proto-IE expressed some kind of plurality. This plurality could be one of subjects (plural form proper), of objects or of repetition (intensive or iterative).⁶ This is to my mind the only possible solution to the riddle constituted by the fact that the forms of the 1st sg. become identical with those of the 1st pl. if they are projected back to the period preceding the IE ablaut. By this projection we are forced to posit one Proto-IE form **ieweneĝeme* as the point of departure common to the two historical forms, Ved. *yunám* of the 1st sg. injunctive present and *yuñjmá* of the 1st pl. of the same categories. Assuming an IE difference of accent would not help us very much, seeing that the accent (like the ablaut alternations caused by it) is revealed to be of secondary origin by the fact that it does not enter into the matrix of lexeme-distinguishing elements, being operative only on the level of inflection and derivation. In pre-apophonic IE the verbal endings could not distinguish "I" and "we" (or "thou" and "ye" if we are allowed to compare the ending **-te* of the 2nd pl. to the pronoun **tu*, **te*). Assuming, however, that the language was unable to distinguish these basic concepts on any grammatical level would be absurd; if reduplication could express plurality in a way independent of the verbal endings, this stumbling-block has been removed.

It is possible that the plurality expressed by reduplication referred to the object rather than to the subject. This would be in excellent agreement with the ergative sentence structure that is to be assumed for the oldest period reached by our reconstructions. If a verbal process is expressed by the ergative of the subject + the inergative of a verbal noun, as e.g. **H₂enere-se* **g^whene-te* "the man's killing", the plurality that could be expressed by reduplicating the verbal noun would change the meaning to something

⁶ This is the situation found *inter alia* in Sumerian: "Reduplizierte Verba weisen auf einen 'pluralischen' Begriff hin. Mit welchem Satzteil sich dieser verbindet, ist nur aus dem Satzzusammenhang zu ermitteln. So kann damit das Vorliegen eines pluralischen Subjekts oder Objekts bezeichnet sein, wobei dann meist die Kennzeichnung des Plurals beim Nomen entfällt, aber auch wiederholte oder dauernde Handlung, vereinzelt eine 'intensive' Handlung oder ein 'intensiver' Zustand" (Falkenstein, *Das Sumerische, Handbuch der Orientalistik*, Abt. I, Bd. II, Abschn. 1 + 2, Lief. 1, p. 57). On the whole subject of "plurality", see now the *Studien zur verbalen Pluralität* (Österr. Akad. d. Wiss., Phil.-hist. Klasse. Sitzungsberichte, Nr. 259, Abhandlung 1, Wien 1968) by Wolfgang Dressler, especially p. 84f on reduplication.

like "the man's several acts of killing" clearly indicating a plurality of objects.

This is unexpectedly coherent with the fact that some situational perfects, being intransitive, were never reduplicated: Skr. *véda vidmá*, Gr. *οἶδξ ἴδμεν*, Goth. *wait witum*. Germanic must be specially archaic in this respect, since perfects of the structure CaR or CaT like *skal* and *mag* have not had their plural forms reshaped to match the structure of *nēmum*, but present instead the well-preserved forms with zero-grade or its equivalent (full grade with roots ending in plosives) and lack of reduplication: *skulum*, *magum*.

Excursus on the dual. It may not be quite out of place to appendix a remark on the position of the dual within the theory described above. I fail to find any indication that the dual was ever characterized by reduplication. The status of the dual in the earliest reconstructible stratum of IE was much rather quite different from its position in the grammars of the attested languages.

The first impression of the dual forms is that they are very old. This is indicated by the traces of prehistoric sound changes dating back to a layer older than the bulk of our IE reconstructions. There is hardly any doubt that the morpheme for person is ultimately the same in the two Ved. forms *bharāva* and *bharāma*, the alternation recurring in adjectival derivatives in *-vant* and *-mant*. On the surface of it, this alternation reminds us of the Celtic mutations, and the conditioning factors may well be the same in both cases. One would then consider the **-w-* of the 1st du. as the result of lenition of the morpheme otherwise preserved as **-m-*. Extending the Celtic parallel still further we might conceive of the **-w-* as having developed in intervocalic position, in which case the Pre-IE form of the 1st du. ending may be reconstructed as **-V-m-V* as opposed to the 1st person non-dual (pl. if reduplicated, sg. if not) **-V-m*, later development producing such secondary features as lenition, vowel quality, and the split of the non-dual ending into two distinct forms, one sg., the other pl.

Another indication of the relatively advanced age of the dual is the identity of the fem. and the neut. in the case of thematic (**-o-/*-ā-*) stems, and of the masc. and the fem. in the case of other stems. This is the two-gender system of animate vs. inanimate where the fem. in **-ā* was in fact the expression of an inani-

mate collective number on a par with the pl. and the dual. No wonder, then, that the dual of the neut. collective is the same as the dual of the neut. sg.

However, the fact that IE used a special word for “both” (Ved. *ubháu*, Greek *ἄμφω*, Lat. *ambo*, OChSl. *oba*, Lith. *abù*, Goth. *bai*, cf. Pokorny IEW p. 34f) and not the dual of a word for “all” is a strong indication that the dual was in origin nothing but the singular of words denoting pairs. The attested dual paradigms are thus mere imitations of the original singular paradigms of the words “two” and “both” with their variety of particles lending personality to the declension. Thus *priyáu* and *mádhū* contain the same particle *u* (*ūṃ iti*) as the pronoun *asáu*, while fem./neut. *priyé* and *súcī* have been extended by means of the particle *-ī* recurring in the pronoun *amī* (like Greek *ὄτος-ῖ*) as is borne out by the common feature of *pragṛhya*. I would further suggest that the Greek ending *-ε* of *ἀνέχ-ε* etc. is a particle, too, and that the same particle is contained in the final vowel of the 1st du. verbal termination **-we* from older **-m *e* as analyzed above.

If any of these speculations are correct, the dual was absent at the oldest stage we can reach, and later it was sufficiently characterized by the conglomerate endings of the numerals “two” and “both” to make further characterization by reduplication superfluous. It cannot, however, be excluded that there did exist, at some stage or other of the linguistic history, reduplicated forms of the dual denoting a plurality of pairs, much as present-day Breton has at its disposal such doubly quantified declensional forms as *daou-lagad-ou* “pairs of eyes” (being the pl. of *daou-lagad* “a pair of eyes”, “two eyes”, this being in its turn the dual of *lagad* “eye”). There is, however, no indication whatever in the attested facts that there was any grammatically exploitable connection between dual number and the derivational device of reduplication.

IV

Some Remarks on the Old Irish *f*-Future

The aim of this paper is merely to draw attention to a number of facts that have not hitherto been duly regarded in discussions about the Old Irish future formation containing a much-debated morpheme *-f-*, especially as concerns its possible relation to the Latin future and imperfect in *-b-*.

As is well-known, the Irish *f*-future is the regular future stem formation of verbs conjugated on stems in Celtic **-ā-* and **-ī-* (IE **-ā-* and **-ē-*, **-ī-*, or **-eje-* respectively). This fact is in itself a strong indication that the formation is young: the old non-derivative verbs do not have it.

In the same manner the future in *-b^e/₀-* was in Archaic Latin restricted to stems in *-ā-*, *-ē-*, and *-ī-*: *amābō*, *monēbō*, *audībō* (later *audiam*). The same delimitation must have been valid for the imperfect in *-bā-* at a certain period, seeing that only these stems can form this paradigm without recourse to analogy: *amābam*, *monēbam*, and the old type *audībam* retain their stem vowels intact, while the thematic *legēbam* (and the new type *audiēbam*) must be due to secondary restructuring.

If the future and the imperfect were to be made from *amā-* and *monē-* as *erit* and *erat* are made from *es-*, a skew and cumbersome set of syncretisms would arise, as the resulting *amet* and *moneat* have already been used as subjunctives. The *-b-* of *amābit* and *monēbit* thus has the advantages of keeping the vocalic elements apart and of characterizing the stem beyond the slightest risk of confusion with other categories.

Formally the relation between *amābit* and *amābat* is like that of *erit* to *erat* or that of *legit* to *legat*. Thus the imperfect in *-bā-* represents an interesting syncretism in that it may equally well

be described as the past of the future stem (*-ā- as in *erat*), i.e. a conditional, and as a future subjunctive (*-ā- as in *legat*), two functions that are seldom kept apart in the morphology of a language. One may instance this by the Latin irrealty of the perfect periphrasis with the future participle: *quid Philippus, si vixisset, facturus fuit* "what would Philip have done, had he lived?"¹ In fact, Ernout-Thomas do record an instance of Ciceronian *debēbās* translatable as "tu aurais dû"², but even if the semantic affinity to a conditional were completely lost in attested Latin, it would still be recommendable to surmise its earlier existence on purely structural grounds.

Among the other Italic languages Faliscan is the only one to possess the labial future: *pipafo* "I shall drink" and *carefo* "I shall want (something to drink)" testifying to the aspirated nature of the *-bh- involved in this formation.

Oscan has the imperfect *fufans* "they were", the only example outside Latin of *-bam*, *-bās*, etc. The isolated *fufans* is suspect, since there is no way to tell whether the morpheme constituting the imperfect is indeed *-fa-* or just *-a-*. In the former case we have a formation like *amābant* to the root *fu-*, in the latter a reduplicated **bhu-bhw-ā-nt* which, though not directly paralleled in Italic, neatly represents the reduplicated form of the Archaic Latin subjunctive *fuam fuās fuat*.

A reduplicated subjunctive, however, is not necessarily an imperfect. Only one other IE language group knows a formation exactly matching *fufans*, namely Celtic. In Old Irish the normal relation between the future and the subjunctive is that the former is a reduplicated variation of the latter. Thus to a present *guidid* "prays" (< **godīti* < **g^whodhejeti* : Gr. *ποθέω*) the sbj. is *geiss* ~ *·ge* (< **gedsti* ~ **gedst* < **g^whedhsti* ~ **g^whedhst*) and the fut. *gigis* ~ *·gig* (< **gigedsti* ~ **gigedst* < **g^whig^whedhsti* ~ **g^whig^whedhst*) with a sigmatic formation as the derivatory basis. Likewise for the sbj. and fut. in *-ā-* : prs. *canaid* "sings" (< **kanāti*), sbj. *canaid* ~ *·cana* (< **kanāti* ~ **kanāt*) and fut. *cechnaid* ~ *cechna* (< **kikanāti* ~ **kikanāt*). Thus Oscan *fufans* is to Archaic Latin *fuat* what the OIr. fut. 3rd pl. *·cechnat* (< **kikanānt*) is to

¹ I borrow this didactic example from Nils Sjöstrand's *Ny latinsk grammatik*² (Lund 1960), p. 253.

² Alfred Ernout & Francois Thomas: *Syntaxe latine* (Paris 1959), p. 248.

the 3rd sg. subj. *canaid* (**kanāti*), the only formal difference being the quality of the reduplicative vowel which is in OIr. *-i- with all verbs, but *-u- in the Oscan form in compliance with the rule known from Old Indo-Iranian demanding -u-reduplication with roots in -u-, cf. Avestan *buuāua* (/bubāva/) of the same verb.³

Structurally, then, the Oscan imperfect *fufans* most of all looks like an Old Irish future. Then it would be in no way surprising if we were to find a correspondence between the Latin imperfect *amābam* and the OIr. future *rannfa* ~ ·*rannub*. But this equation has been contested from time to time, and it might be wise to look for a while at the pros and cons.⁴

Synchronically, the OIr. *f*-future may be described in the following way. It contains: 1) a present stem in -a- ~ -^a- or -i- ~ -ⁱ- giving neutral quality in ·*rann*- and palatal quality in ·*léic*- + 2) a consonant which is -*f*- after a consonant and -*b* in word-final position after a vowel + 3) endings like the *ā*-future or *ā*-subjunctive. If we adopt φ as a symbol for the alternating labial, we have the 3rd sg. ·*RaN φ a* and ·*L'ēk'ḡa* (written ·*rannfa* and ·*léicfea*). Whatever the origin of the - φ -, these forms may be traced back to some such (doubtless anachronistic) formulae as **prāsnā- φ -āt* and **link^w- φ -āt*.

The only phonological unit known to behave like the - φ - of these forms is IE **sw*, and yet even this presents one minor problem. Thurneysen notes⁵ that the *f* arising from lenited **sw* could be written both *f* and *ph* (*tinfed* or *tinphed*, VN of *do·infet* "inspires", un-compounded *sétid* "blows", thus representing **t(o)eni-swesdom* > **ténihwèzdaN* > *t'íñih'wezdaN* > *t'íñíḡed*). But no *ph*-future is found attested in Old Irish, a fact that makes one suspicious about either the spelling rule or – as we shall see – rather the etymology of the - φ -.

I fail to see any plausible morphological justification for

³ On this form, see E. Benveniste in *Symbolae . . . Kurylowicz* (Krakow 1965), p. 25–33. Also Calvert Watkins: *Geschichte der indogermanischen Verbalflexion* (= *Indogermanische Grammatik*, Band III, erster Teil (Heidelberg 1969)), p. 150.

⁴ The arguments against the equation of Latin -*b*- with OIr. -*f*- can be seen in Thurneysen, *Grammar*, p. 637. Further information in Watkins's exposé in *Ériu* XX (1966), p. 69–72. Valuable is also the compte rendu of the latter by E. Bachellery in *Études celtiques* XII,1 (1968–9), p. 322–5.

⁵ Thurneysen, *Grammar*, p. 21. In his *Aspirationen i irsk*, p. 69, Holger Pedersen wrote as long ago as 1897, "I consider it very doubtful whether one may conclude from this that this *f* (scil. the one from **sw*) was different from the usual *f*". Unfortunately, Pedersen did not support this statement by any further argumentation.

*-*sw-ā-* as the future morpheme, even after reading the attempt undertaken by Watkins.⁶ I find it utterly improbable that the derivational source should be desiderative adjectives like the Vedic *didhiṣú-* “wanting to put”, or the unreduplicated *dhákṣu-* “wanting to burn” forming in its turn a denominative verb of the type **dheg^whswāti* “wishes to burn”. As the OIr. counterpart of the Indo-Iranian desiderative is the *s*-future which is found only with non-derivative verbs, the derivative verbs in **-ā-*, **-ē-*, and **-eje-* that are too young to have the sigmatic formation should have instead of this a future derived from a derivation of the *s*-future. This must mean that they once formed desideratives (or *s*-futures) that could generate the adjectival source of the **-swā-* formation. But at that old stage these verbs did not exist at all. The verbs showing *f*-future are the new verbs created in the language after the loss of the derivational capacity earlier displayed by **-s-*, **-ā-*, and reduplication. Watkins’s theory, then, would have to ascribe to these verbs a very long history that they do not have.

The OIr. *-ā-* and *-ī/-ē-* verbs form their subjunctives in *-ā-* : *·léicea*. Otherwise the normal future corresponding to an *-ā-* subjunctive is one with reduplication and *-ā-*, but as these verbs are for the most part derived from unchangeable word stems (like *rannaid* from *rann* “part”) they are incapable of reduplication, and the *ā*-future would be identical with the *ā*-subjunctive if it had not been for the *-φ-*. We here see a very striking parallel to the function of the *-b-* ~ *-f-* of the Italic formations mentioned above. Indeed, the structural similarity of the two oppositions *moneat* : *monēbat* and *L'ēk'a* : *L'ēk'φa* is so clear that it has been quite impossible for sceptics to ease the curiosity of generations of celtologists merely by referring to problems such as the minor difference of function, the difficulties of reconciling Lat. *-b-* with OIr. *-f-* in terms of a sound-law, and the readiness of a forced **-sw-* to take over in case **-bh(w)-* should fail to work. Moreover, as the Oscan imperfect *fufans* is definitely structured like an OIr. future and as the morpheme **-ā-* conveys a modal shade equally prone to change in either direction, there is ample structural support for a tentative equation of OIr. *-φ-* with the Latin *-b-* and the Oscan *-f-*.

⁶ Watkins, *The origin of the f-future*, *Ériu* XX (1966), p. 67–81, with a short *Addendum*, *ibid.* p. 93.

Already the analysis of *fufans* as the reduplicated counterpart of Latin *fuant** limits our choice to IE **bhw* as the source of the hiatus-filling labial. The question is now whether or not this cluster could develop into the OIr. alternation *-f- ~ -β-*, here represented by *-φ-*. Sommerfelt believed it could and analyzed the *-φ-* as the outcome of a geminate **-ww-*, itself the lenition product of Celtic **-bw-* (IE **bhw*).⁷ This is rejected by Watkins for a number of seemingly good reasons: 1) Words with initial **bhw-* are spelt with *b-* in OIr. also in lenition position. 2) Initial *f-* is in fact the product of **-s i* word-final position + initial **w-* > **hw-* > *f-*, and so */f/* never entered into the system of alternating consonant quantities.⁸ These objections are serious; if they can stand criticism the whole edifice of an Italo-Celtic parallel falls to dust.

But there are a few important pieces of evidence pointing to the behaviour of IE **bhw* along the lines of the future morpheme in question. We have an OIr. *f* arising from **bhw* in the initials of proclitic words: *fa*, *fá* beside *ba*, *bá* “or” must be a form of the verb “be” like Old Lat. *fuat* “soit” and go back to **bhwāt*. As a verb, *ba/ bá* is peculiar, presenting as it does a syncretism between the present subjunctive and the past (ipf. and aorist, indiscriminately) of the copula, and even the verbal form is occasionally spelt *fa*. An instance is found readily accessible in the late MS R of the Scéla mucce Meic Dathó, ed. Thurneysen, p. 8, line 10 from the bottom of the page. The more frequent *ro-t·fia* “it will be (so) for thee” of the same MS cannot have *f* from [β] due to the neighbouring *t*, since the latter represents [d]. We rather have to do with instances of generalized sandhi variants, perhaps originating from a dialect or social stratum different from the one exhibiting an unvariable *b-*. Something of the kind must be true of the bewildering *a fáil a mbi* “where it was” of the Book of Lecan = *in bail a mboi* of the Rennes version (both printed as an appendix to

⁷ Alf Sommerfelt, *Le futur irlandais en -f-*, MSL XII (1921), p. 230–2. On the theory of consonant gradation see especially Sommerfelt’s *Consonant quantity in Celtic*, NTS XVII (1954), p. 102–18 (esp. p. 110f). Both articles have been reprinted in A. Sommerfelt, *Synchronic and diachronic aspects of language* (The Hague 1962). A phonemic evaluation of the Celtic reflex of IE **sw* has been made explicit by Eric P. Hamp as a note to his *Consonant allophones of Proto-Keltic*, Lochlann I (1958), p. 209–17 (esp. p. 211 and 217). A laconic rule, “Intervok. *sv, βv* wird zu *f*” has found its way into Julius Pokorny’s *Altirische Grammatik*, Berlin 1925 (new impression 1969), p. 27.

⁸ Watkins, *op. cit.*, p. 70–1.

Thurneysen's text, op. cit. p. 23). This word has a by-form *baile* "place" entered in Pokorny's IEW⁹ as **bh_uə-lijo-*. Throughout the history of Irish the living lenition product of the *b-* reflecting IE **bhw-* is the same as that of other *b-*'s, namely $\beta-$ (in OIr. spelt *b-*), which means that this covers only one living morphophoneme *b*. This is so in the glosses, too: ML. 61^b17 *amal bid* "as if it were", but here again we find remnants of lenited forms in *f-*: ML. 34^b11, 37^b22 *amal fid*.¹⁰ In spite of the limited number of the *f*-forms, their joint testimony weakens the cogency of Watkins's first counter-argument to a very considerable degree. Words with original **bhw-* are extremely scarce, and so a morphophonemic alternation *b- ~ f-* might easily yield to the pressure of any levelling analogy however meager. Indeed the verb "be" itself does have forms with original **bh-* not followed by **-w-*. Thus the 3rd sg. pret. of the substantive verb *boí* (or *baí*) must go back to some such form as **bhowe*,¹¹ which in no way lends itself to an alternation with *f-*. It is perhaps significant that the occasional *f*-forms of "be" do not to my knowledge include any instance of **foí* (or **faí*) for this form.

Watkins's second argument is untenable, too. Even if *f-* of *fer* did represent the joint reflex of **-s + *w-* in **sindos wiros > in fer*, it would be distorting the probability measures to claim the same for, say, *fid* "wood" which is a feminine noun: **sindū wedhus > in fid* (phonetically probably *i'N'id*¹²). Sommerfelt was undoubtedly right in accepting Irish *f-* and British *gw-* (Welsh *gŵr*, *gwŷdd*) as reflexes of the strong member of an underlying alternation *W : w* matching that of *M : w̃*, *N : v*, *R : r*, and *L : l*. If the lenition products of IE **w*, OIr. zero and Welsh *w*, are not weakenings of the respective reflexes of strong *W*, but represent developments of **w* independent of the oppositions of consonant quantity, as is assumed by Watkins, the Welsh facts become difficult to understand. The lenition of proclitic words such as ML.W. and Mod. W. *wrth* "at, against", Mod.W. *wedi* "after", *ar* "on" (Bret. *war*) can only be brought in accordance with the Old Welsh spellings *gurt*, *quetig* / *quotig*, *guor* / *guar* by assuming that

⁹ Julius Pokorny, *Indogermanisches etymologisches Wörterbuch*, p. 148.

¹⁰ This material is of course not new. It can all be read in Thurneysen's *Grammar*, p. 78, but it does not appear ever to have been brought into connexion with the problems of the *f*-future in any of the previous writings on the subject.

¹¹ Thurneysen, *Grammar*, p. 483. Watkins, *Idg. Gr.* III.1. p. 150.

¹² *fid* has (analogical) neutral $[\delta]$, cf. the Modern Scottish Gaelic form $[fioɣ]$ reported in Oftedal's *The Gaelic of Leurbost*, NTS Supp. IV (1956).

[*gw*] was lenited to [*γw*], which in turn went to [*w*], at the same time as [*g*] was lenited to [*γ*] and further to zero. The same representation of IE **w* is found in OW. *petguar* = Mod.W. *pedwar* "four" which proves with all the clarity we could wish that the preliminary strengthening of **w* to *gw* prior to lenition was not restricted to the position after original **s*.

A morphophonemic reinterpretation of the alternations between radical and lenited consonants may lead to some interesting results, if we bear in mind the principles of Sommerfelt's analysis of the degrees of consonant quantity. If we accept the basic equation of *|d|* as a geminated *|δ|*, the alternations *|d|* ~ *|δ|*, *|t|* ~ *|þ|*, *|g|* ~ *|γ|*, *|k|* ~ *|x|*, *|N|* ~ *|v|*, *|M|* ~ *|w̃|*, etc. can all be re-analyzed as *δδ* ~ *δ*, *þþ* ~ *þ*, *γγ* ~ *γ*, *xx* ~ *x*, *vv* ~ *v*, *w̃w̃* ~ *w̃*, etc. This is certainly in keeping with the fact established by David Greene that "gemination" is in fact nothing but the absence of lenition:¹³ "strengthened" merely means "not weakened". The choice of gemination as the basic characteristic of the alternation is certainly arbitrary, but it is preferred to some diacritic sign of "weakening" because it makes the hierarchy stand out quite clearly.

We can then construct the underlying forms of a few alternations that interest us here:

IE	OIr. phonetic shape		Underlying form	
	unlenited	lenited	unlenited	lenited
* <i>m</i>	<i>M</i>	<i>w̃</i>	<i>w̃w̃</i>	<i>w̃</i>
* <i>sm</i>	<i>sM</i>	<i>M</i>	<i>hhw̃</i>	<i>hw̃</i>
* <i>s</i>	<i>s</i>	<i>h</i>	<i>hh</i>	<i>h</i>
* <i>w</i>	<i>f</i>	zero	<i>ww</i>	<i>w</i>
* <i>sw</i>	<i>s</i>	<i>f</i>	<i>hhw</i>	<i>hw</i>
*- <i>N w</i> -		<i>M</i>		<i>w̃w</i>
*- <i>N b(h)</i> -		<i>w</i>		<i>w̃β</i>
* <i>b(h)</i>	<i>b</i>	<i>β</i>	<i>ββ</i>	<i>β</i>
* <i>b(h)w</i>	<i>b</i>	?	<i>ββw</i>	?

The question is now, what would come out of lenited **b(h)w*? It is clear at once that the underlying form should be *βw*, since the lenited morphophonemes are everywhere formed from the unleni-

¹³ In *Celtica* vol. 3 (1956), p. 284-9.

ted by simplification of the geminates. But how should βw be pronounced? It seems to be equally close to $w w$ and to $\beta\beta$. But $\beta\beta$ is pronounced $[b]$ and is identical with the $\beta\beta w$ of the radical, which would require us to assume that nothing happened on the phonemic level, a very unlikely assumption in view of the otherwise global application of lenition. There remain then two possibilities: either βw was realized in the same way as $w w$, i.e. as $[\varphi]$, or else it had a pronunciation of its own. As hinted above, the total lack of ph in the spelling of the f -future is perhaps indicative of a significantly different pronunciation of original $*sw$ and $*bhw$ also in lenition position. If ph does stand for something special it gives to intervocalic $*sw$ a place apart from initial $*w$ and intervocalic $*bhw$. In that case I would think of a labiovelar or labio-laryngal spirant $[x^w]$ or $[h^w]$, in accordance with the underlying form hw . In Latin loanwords ph may well be the notation of the lenited member of an alternation $[p] \sim [h^w]$, replacing earlier $[k^w] \sim [x^w]$. Be this as it may, the analysis of the underlying forms leaves very little room for βw (lenited $*bhw$) to be realized in any other way than $[\varphi]$, i.e. exactly like $w w$ (unlenited $*w$).

It will be seen that we do possess pertinent (if scarce) evidence to support the assumption of $*-bhw\bar{a}-$ as the source of the OIr. future morpheme of derivative verbs.

We now want to know where this morpheme comes from.

A sequence $*-bhw\bar{a}-$ can hardly be anything but a form of the verb "be" furnished with the suffix $*-\bar{a}-$ of the future or the subjunctive. But the future of the substantive verb is $bieid \sim \cdot bia$, and the prs. sbj. is $beid \sim \cdot bé$, representing two different formations only secondarily fitted into the same paradigm. The form $\cdot bia$ is obviously the \bar{a} -derivative of $biid$, i.e. it was made as the sbj. of the habitual present. The substantive verb is the only one to possess a habitual, which may be important in this connexion. The fact that the fut. of $*bhew-$ ($*bhu-bhw\bar{a}-$) could come to be confused with the subjunctive of the habitual $*bhw-i\ddot{x}^e/_{\bar{o}}$ (i.e. with $*bhwi\ddot{q}\bar{a}t$) must mean that the predecessor of the future with reduplication and $*-\bar{a}-$ had close semantic affinities with both of these categories, modally with the subjunctive (therefore $*-\bar{a}-$), and aspectually with the habitual. It must be this aspectual value that is expressed by the reduplication. It follows that the original difference between the two formations was very slight, the latter

being merely an emphatic, perhaps iterative, variant of the former. This in turn means that a form like **bhubhwāt* would lend itself to two different analyses: 1) Reduplicative syllable **bhu-* + root **-bhw-* + modal morpheme **-ā-* + desinence. Or 2) Root **bhu-* + an element **-bhw-* + modal **-ā-* + desinence. At first glance, 1) would be the only natural solution, 2) being excluded by the non-existence of an element **-bhw-*. Nevertheless, analysis 2) must be the way the form was in fact segmented at a certain stage of the language history. The reason for the giving up of analysis 1) must have to do with the taking over of **bhwīāt*, expressing the aspectual shade by stem-formation instead of reduplication. This in turn made the form **bhubhwāt* superfluous, and it became a mere variant of **bhwīāt*.

Now, and only now, is the form **bhubhwāt* likely to be analyzed in a way that singles out an element **-bhwā-* of the same function as the **-iīā-* (i.e. */-yyā-/*) of **bhwīāt* (*/bhwyyāt/*) as contrasted with **bhubhwāt* (*/bhwbhwāt/*). Another factor blurring the analysis of **bhubhwāt* was in all probability the generalization of the initial alternation *[b] ~ [β]* originally restricted to cases where **bh-* was not followed by **-w-* (*boí* above) to cover also cases where it was. Once the intervocalic **-bhw-* had become *-φ-* (or just something different from */b/* or */β/* or a combination of these), the resulting form, */buφāt/* or the like, could no longer be analyzed as a reduplicated form, because now the unreduplicated **bhwāt* had only the alternants */bāt/* and */βāt/*, the old regular lenition variant */φāt/* being restricted to the marginal use preserved in the conjunction *fa, fá* “or”.

The joint operation of all this would lead to the establishing of a new and characteristic (and therefore successful) future morpheme. Thus the non-existence of a form **/buφāt/* is no counter-argument, as its previous existence is demanded by the structure of the Old Irish conjugation, and its ousting rather favours than impedes the analogical almost unlimited use of its elements.

A comparison between the personal forms of the *f*-future and those of the *ā*-subjunctive is not without complications:

<i>f</i> -future	<i>ā</i> -subjunctive
1 st sg. <i>marbfa</i> marβφə - <i>marbub</i> marβəβ ^w or - <i>mairbiub</i> maɪβ'əβ ^w	<i>bera</i> b'erə - <i>ber</i> b'er
2 nd sg. <i>mairbfe</i> maɪβ'φe - <i>mairbfe</i> maɪβ'φe	<i>berae</i> b'ere - <i>berae</i> b'ere
3 rd sg. <i>mairbfid</i> * maɪβ'φəδ' - <i>marbfa</i> marβφə or - <i>mairbfea</i> maɪβ'φə	<i>beraid</i> b'erəδ' - <i>bera</i> b'erə
1 st pl. <i>marbfimmi</i> maɪβ'φəMi - <i>marbfam</i> marβφəw̃ or - <i>mairbfem</i> maɪβ'φəw̃	<i>bermai</i> b'erMi - <i>beram</i> b'erəw̃
2 nd pl. <i>marbfithe</i> * maɪβ'φəp'e - <i>marbfid</i> * maɪβ'φəδ'	<i>berthae</i> b'erpe - <i>beraid</i> b'erəδ'
3 rd pl. <i>marbfait</i> marβφəd' or <i>mairbfīt</i> maɪβ'φəd' - <i>marbfat</i> marβφəd or - <i>mairbfet</i> maɪβ'φəd	<i>berait</i> b'erəd' - <i>berat</i> b'erəd

Note on the phonemic transcription: It may not be quite superfluous to remark that |β| and |φ| are here used of two phonemes, both occasionally entering into the alternation |b| ~ |φ| ~ |β| constituting the morphophoneme φ.

From this table two incongruities spring to the eye: 1) The *u*-quality of -*marbub* vs. the neutral quality of -*ber*, and 2) the spreading palatal quality of the cluster -βφ-, a fact that can hardly be reconciled with the etymological analysis: **mṛwābhawāti* can only lead to *|marβφəδ'|, cf. the neutral quality of the imperfect *marbfad* < **mṛwābhawāto*.

The 1st sg. -*marbub* points to a thematic formation **mṛwābhawō* (like Latin *amābō*). The other persons of such a paradigm would present palatal quality here and there: 3rd sg. **mṛwābhawēti* could very well yield the *mairbfid** of the table. On closer inspection, however, this would meet with insurmountable obstacles: The ending of the 3rd sg. -*mairbfea* would have to be thought of as taken over from the -*ā*-paradigm, and not even this would explain the palatal quality of the 3rd pl. by-forms, where **-bhawonti*, **-bhawont* would be of no avail. One must therefore accept the old theory of analogical influence from the palatal quality of the

-ī-stems: 3rd sg. *lécifid* is the regular reflex of a form in **-ī-bhwāti*. But why *u*-quality in the 1st sg.? One possibility is that it represents a form of the subjunctive ending older than the neutral zero presented by the subjunctive itself. In that case **bherō* was once the form of the subj., just as it is in Vedic *bhárā*. Then at a certain stage a polarization must have taken place, whereby the forms with thematic vowel (whether **-e-*, **-o-*, or **-ō-*) have been restricted to the indicative as a contrast to the **-ā-* used to mark the subjunctive, where it was subsequently demanded in all forms. This led to the symmetry of indicative $|b'ir^wu| : |-b'ir^w|$ (final vowel *-u* vs. *u*-quality of final consonant) and subj. $|b'erə| : |-b'er|$ (neutral final vowel vs. neutral quality). The derivation of the *f*-future must, therefore, have been completed before this polarity worked its influence.

It will be seen that this analysis of the *f*-future as containing a morpheme singled out of an originally reduplicated form of a highly frequent verb constitutes a close parallel to the theory about the Germanic weak preterite advanced in 1963 by G. Bech.¹⁴ It is interesting to note, too, that a similar (and to my mind convincing) theory was presented by Johnny Christensen at a meeting in the Linguistic Circle of Copenhagen in May 1965, concerning Latin *amābam*, *-bās*, *-bat* . . ., seeing in *amābant* a morpheme segmented off by wrong decomposition of the reduplicated formation seen in Oscan *fufans*. The process may indeed have been the same as the one here described for Irish: Once **bhuhwā-* had become *fufā* $||$ **fubā-*, the analysis as a reduplicated form of *fuā-* demanded a living morphophonemic alternation *fu- ~ -f-* $||$ *-b-* of some frequency (i.e. with a worth-while functional load). This alternation being exceedingly rare, the *-f-* $||$ *-b-* came to be perceived as part of the suffix *-fā-* $||$ *-bā-*. Though "do" is not found in Gothic, and **fubant* not in Latin, there can be little doubt that the same kind of analogy worked out a morpheme presenting all the good qualities of a productive suffix, being over-characterized, hiatus-filling, and easily inflectible, and so especially applicable to derivative verbs. This paper merely ascribes the same influence to a Pre-Old-Irish underlying form **|buφāt|*.

¹⁴ Gunnar Bech, *Die Entstehung des schwachen Präteritums*, Hist. Filos. Medd. Dansk. Vid. Selsk. 40, no. 4 (1963). Note especially the identity between the endings of Goth. *salboda* and OHG *teta*, or between Goth. *salbo-dedum* and the whole of OHG *tatum*.

It will be seen, finally, that this paper does not ascribe any "Italo-Celtic" age to the **bhwā*-formations. Latin and Irish merely had the same possibilities of analogical change, and so even a correspondence as close as this need not surprise us very much. It is, however, indicative of a very marked degree of structural similarity between Italic and Celtic testifying to a close cultural contact of a kind likely to produce cases of linguistic convergence typical of an incipient *Sprachbund*.

V

The Labialized Laryngeals of Lycian

The system of laryngeals in the Anatolian cuneiform languages comprises two elements only kept apart in intervocalic position, viz. *-h-* and *-hh-*. The etymological basis of this opposition represents an old issue; the best solution is, however, in all probability that the graphic difference covers a phonological opposition in terms of Sturtevant's Law (whether this is an opposition of voice or one of tenseness¹). It is clear from Crossland's² review of the situation that *-h-* and *-hh-* are not allophones, but represent separate phonemes of separate etymology, a doctrine that has also been accepted in Lindeman's survey³ where we read the reconstructions: **H₁* (voiced palatal fricative) for *-h-* and **H₂* (voiceless velar fricative) or **H₃* (voiceless labiovelar fricative) for *-hh-*.

The facts of the other Anatolian languages are less transparent. The graphic system of Hieroglyphic Luwian probably presents only the one laryngeal */h/* with no further distinction: *huh-* = Hitt. *huhha-* "grandfather". The sign *á* might *a priori* be expected to contain the reflex of a laryngeal,⁴ but the attitude expressed by Laroche seeing in it merely an allophone of initial position (perhaps with a glottal catch) is probably more easily reconciled with the attested facts.⁵

Lydian in a few instances appears to respond with zero, cf. especially *eša-v* "grandson" (acc.sg.) to Hitt. *has-* "beget", *hassa-* "grandson". The old equation *Γύγης = huhhas* ought probably

¹ To this question, cf. my remarks on the alleged Hittite "Lautverschiebung" in Note 16 of my paper "Some Linguistic Universals Applied to Indo-European" published in the present collection, p. 12f. above.

² *Archivum Linguisticum* 10 (1950), p. 79-99, esp. 81.

³ F. O. Lindeman, *Einführung in die Laryngaltheorie* (Berlin 1970), Chapter IV, with the table p. 101.

⁴ The transcription by ' is that of Gelb *Hittite Hieroglyphs* III (Chicago 1942).

⁵ The attitude of different scholars to this question (Gelb, Meriggi, Laroche, Mittelberger, Bossert) has been reviewed by Dunaevskaja in *Jazyk chet'skikh ieroglifov* (Moskva 1967), p. 61f.

not to be bluntly rejected, seeing that we do have evidence for plosive arisen from laryngeal also in Lydian writing, cs. *kofu-λ-k* "and to the water" (dat.-loc.sg.) to Hitt. *hap-* "river".⁶ On the other hand, under the present circumstances it would no doubt be rash to form an overly rigid idea of the development of laryngeals in Lydian.

In the case of Lycian it has long since been recognized that the character transcribed as *χ* by one tradition (Bugge, Torp, Thomesen, Pedersen, Laroche) and as *k* by the other (Kalinka, Friedrich, Meriggi, Gusmani, Ševoroškin) represents a dorsal spirant [x], being the usual correspondence of Hitt. *h*. One may compare, e.g. Lyc. *χñnahi* "of the grandmother" (genitival adjective) to Hitt. *hannas*; *χñtawata* dat.-loc. "reign": Luw. *hantawata-* "commandant";⁷ *χugaha* "to those of the grandfathers" (dat.-loc. of gen. adj.): Hitt. *huhhas*; *χahba* "son-in-law": Hitt. *hassa-* "grandson"; *αχα* "I made (it)", *me-pijαχα* "I reserved (it), I gave (it)", *prñnawαχα* "I built (it)", *αχα* "I made", *se-pijαχα* "and I reserved/gave": Hitt. 1st sg. prt. *-hun*, Luw. *-ha*⁸; *λαχadi* instr. "with the (military) campaign"⁹: Hitt. *lahhai-*; *zχχαte* "they defeated": Hitt. *zahhanzi* "they defeat".

⁶ On the Lydian words, see Gusmani *Lydisches Wörterbuch* (Heidelberg 1964) s. uu. The etymologies have been allotted a fuller treatment by Ševoroškin in *Lidijskij jazyk* (Moskva 1967), p. 50 and 52.

⁷ Laroche BSL 53 (1958), p. 182. Cf. also Gusmani IF 68 (1963), p. 287f. Gusmani translates "Strateg, Strategie" in *Archiv Orientalní* 36 (1968), p. 9.

⁸ It can hardly be doubted that the Lycian forms *prñnawαχα* (40c8) "I built (it)", *prñnawαte* (passim) "he built (it)", *prñnawαte* (6) "they built (it)" contain an enclitic object pronoun which is absent in the corresponding forms without nasalization *prñnawαχα** (cf. *pijαχα* matching *pijαχα*), *prñnawate* (passim), *prñnawαte** (cf. *pijete*: *pijete*). Hittite *-hun* and Luw. *-ha* of the 1st sg. prt. may be different generalizations, *-hun* being the old pronominal form presenting the accusative ending seen in *apun* "eum" and *kun* "hunc". This is a further corroboration of Pedersen's explanation (*Hittitisch und die anderen indoeuropäischen Sprachen*, København 1938, p. 59f) of *asi* and *uni* "the latter" as the old nom. and acc. of the pronoun *a-*, i.e. *as* and **un*, + the deictic particle **i* of *οὐτος·i*, OIr *int·i*. The attested acc. *-an* of the enclitic pronoun *-a-* (Friedrich *Hethitisches Elementarbuch* 2, Heidelberg 1960, § 102) must then be the result of later analogical reshaping. I seize the opportunity to add that the somewhat troublesome present endings of Slavic *ber-ø*, *-ešb* (*-eši* only dialectally), *-etb*, *-emz*, *-ete*, *-otb* may go back to **bherō-(o)m*, **-ešj-om*, **-et-om*, **-emo-(o)m*, **-ete*, **-ont-om*, being a compound of the paradigm that has also been preserved in OIr *do·biur*, *·bir* (phonetically like dat. *nim* < **nemesi*), *·beir*, *·beram*, *·berid*, *·berat* plus an enclitic object pronoun **-om* added to all forms except the 2nd pl. (which is also the only one not to receive any **-r* in the OIr deponent flexion).

⁹ Ševoroškin *Lidijskij jazyk*, p. 62; *Orbis* 17 (1968), p. 477; *Voprosy Jazykoznanija* 1968 No. 6, p. 75 and 79; Ševoroškin and A. Korol'ov *Arch. Or.* 37 (1969), p. 526; Gusmani *Arch. Or.* 36 (1968), p. 4f and 12. The interpretation as a verbal form (Puhvel *Evidence for Laryngeals* p. 84 "he smote" as preterite!) is unacceptable.

Instead of this χ we find in a number of instances its alternant g which is phonetically in all likelihood a voiced spirant [γ], cf. the afore-mentioned $\chi u g a h a$ and the verbal form $a g q$ alternating with $a \chi q$. Further examples are supplied by the hesitation in the spelling of the following proper names: $z a g a b a$ (44a42, and coins) : $z a \chi a b a h a$ (coin no. 192 a); $\chi e z i g a h$ (44a31) : $\chi e z i \chi a$ (65.17); $\chi e r i g a$ (44c37, c50, coins) with the derivatives $\chi e r i g a h e$ (44a10), $\chi e r i g a s a$ (44d8), $\chi e r i g a z : \epsilon$ (44d19),¹⁰ $\chi e r i g a z \tilde{n}$ (44d45, d53f) : $\chi e r [i] \chi e h e$ (43.2); and Mil. $u m r g g a z \tilde{n}$ (44c49) : Lyc.A $h u m r \chi \chi q$ (44a55). In initial position g is the substitute for Iranian g : $g a s a b a l a < *g a n j a b \bar{a} r a$ "tesoriere" (Meriggi).¹¹ As is seen from the examples this g had no phonemic status in genuine Lycian words representing merely the allophonic voicing of intervocalic χ .

The letter traditionally transcribed a q was already before the turn of the century determined by Holger Pedersen as a labiovelar spirant [x^w],¹² True, the etymologies on which Pedersen based this phonetic interpretation, are untenable. We know now that $q l a$ "assembly, precinct" (Pedersen translated "people") has nothing to do with Skt. $k u l a$ - "family", and in the case of the alleged numeral $q a r a z u$ (subtracted from $q a r a z u t a z i$ 44b41) we have no reason for assessing it at precisely "40", nor would this in its turn justify the conclusion of spirantic pronunciation (moreover, it remains completely uncertain whether it is a numeral at all and to be segmented off in this shape). As far as the main content of the hypothesis is concerned, however, Pedersen was undoubtedly right, as we can prove with almost complete certainty that q possessed both distinctive features needed for labelling it a rounded fricative.

The spirantic nature of q has been evident since its correspondence with Hitt. and Luw. h was established by Pedersen in 1945.¹³ One need only cite such well-known etymologies as Lyc. $q a t i$ "he judges, punishes", 3rd pl. $q a \tilde{n} t i =$ Hitt. 3rd pl. $h a n n a n z i$, with the iterative Lyc. $q a s t t i =$ Hitt. $h a s k i z z i$; Lyc. $q l a$ "assembly"

¹⁰ Erroneous use of the word divider, cf. Gusmani *Arch. Or.* 36, p. 17; likewise Ševoroškin *Orbis* 17, p. 484.

¹¹ So interpreted by J. Imbert MSL XIX; cf. also Meriggi *Rendiconti della Reale Accademia dei Lincei*, Classe di scienze . . . VI, IV (1926), p. 449.

¹² Holger Pedersen: *Lykisk, Nordisk Tidsskrift for Filologi*, tredje Række, VII Bind (1898), p. 98.

¹³ *Lykisch und Hittitisch* (København 1945), p. 27.

: Luw. *hila-* “enceinte”¹⁴; and the God’s name Lyc.A *trqqas*, Mil. *trqqiz* = Luw. ^d*Tarhunz*. These and a few further examples will be discussed at some greater length below.

The additional feature of roundedness in the articulation of *q* is clear from its effect on a preceding nasal, which always appears as *m̃* (instead of *ñ*). The following clusters of nasal + stop are permitted in the body of Lycian texts: *ñt* (*ñte* “inside” = Luw. *anda*, *ñtepitās̃nti* “they shall bury”, *s̃nta* probably “a hundred”¹⁵), *ñz* (phonetically [*nts*]: Mil. *q̃ñza* 44d35, according to Ševoroškin¹⁶ dat.pl. “to the families” to Hitt. *hanzassa-*; *χs̃s̃eñ̃zija* 150.1 PN; *ñzzijaha* 29.8 of unknown meaning), *ñk* (probably [*ŋk*], e.g. *ñke* 112.2 = *εke* conj. “after”; *tis̃ñ-ke* 89.3 acc. “whichever”¹⁷), *m̃p* (Mil. *ar̃m̃p̃q* acc., according to Ševoroškin a ritual appellative: Hitt. *arimpa-* “Bronzegerät im Ritual”¹⁸); *m̃parahe* 104b.3 PN gen.). Clusters of two nasal graphs take the form *ñn* (*ar̃ñna* the city of “Xanthos”; *χ̃ñna-* “grandmother”; *kbis̃ñni* a numeral, “20” or “200”; *tris̃ñni* “30” or “300”) or *m̃m* (*tr̃m̃mili* “Lycian”, *k̃m̃mis* 110.2, 124.9 “biers”¹⁹), *m̃mije* 143.2 PN dat.²⁰). Clusters with spirantic second element show the same assimilation of the nasal: *ñχ*[*ŋx*] (*ñχrahidiye* 29.2 of unknown meaning; with morpheme boundary, *ep̃ñ-χuz̃a* 127.1 PN gen.²¹; *ap̃ñ-χahb[ij]a* 18.2, a term of relationship) and, more important, *m̃q* (Mil. *m̃qr̃ε* 44c40,

¹⁴ Laroche BSL 55 (1960), p. 183³.

¹⁵ I note in passing that this numeral is not diagnostic for the assignment of the labels *saem* and *centum*, being in all probability a loan-word from Persian *sada*. The correspondence *-ñt-* = *-d-* and the syncope recur *inter alia* in the example “Lykisch *spp̃nt̃aza* = iranisch **sp̃ad̃āza-*” treated by Rüdiger Schmitt KZ 85, p. 43–48.

¹⁶ *Lidijskij jazyk*, p. 62; *Voprosy Jazykoznanija* 1968 No. 6, p. 79f; *Orbis* 17, p. 487; Šev.-Koro’lov, *Arch. Or.* 37, p. 532.

¹⁷ Acc. of *tise* “whoever” + *ke*, cf. Pedersen, *Lyk. u. Hitt.*, p. 22, and Laroche, BSL 55, p. 177 ff.

¹⁸ *Vop. Jaz.* 1968,6, p. 75, and *Orbis* 17, p. 483, both times rightly against Gusmani, *Arch. Or.* 36, p. 8 (personal name acc. < **Arma-pijama-*).

¹⁹ V. Thomsen, *Études lyciennes* I (København 1899), p. 13, “quelque objet transportable ou l’on peut placer les cadavres”.

²⁰ Dat. of pers. name *Αμμυς*, cf. Houwink ten Cate, *The Luwian Population Groups* (Leiden 1961), p. 103, and G. Neumann, *Handbuch der Orientalistik* I, II, 2 (Leiden 1969), p. 384.

²¹ Without the *-h(ē/e)* of the genitive. Neumann, loc. cit. p. 384, assumes loss of *-h*. In his unpublished MS of a series of lectures held at the University of Copenhagen in 1946, Pedersen analyzed the word as an onomastic use of an appellative meaning “great-grandfather”. Ševoroškin, *Étimologija* 1965 (1967), p. 233, appears to take it as an appellative even in the present text (“pra-ded”), which is obviously a mistake, seeing that the builder introduces himself as “the son of Ep̃ñχuz̃a”.

44dl, d54, perhaps 55.1²², which will be treated below together with its other inflectional forms, and the completely obscure Mil. *qñqi-ke* 55.6). Heterogenous nasal combinations are alien to the language, and there are no instances of *ñp (apart from the fortuitous combination of preverb and verb as *epñ-pijete*, *epñ-pu(we)-*, Mil. *epñ:- predi*), *ñm (discounting scribal errors like *padrñma* 49.1 for regular *padrñma* 48.8, cf. *padrñmah* 11.1, *padrñmahe* 48.6, and, with a different way of noting the open contact between /r/ and /m/, *padrma* 48.2), *ñt (except for the isolated *zeñtija* 44a41), or of *ñk (*tñkre* 55.1 is probably a wrong reading: Ševoroškin reads *tñqrē*²³), nor are *ñn, *ñz, or, more important, *ñq ever met with. Although this proof must be considered cogent in itself it is no less interesting to note that the phonematic rounding of q hereby established is corroborated by defensible etymologies.

In view of the Lycian rounding I do not believe that *qati*, Hitt. *hannāi* “punishes” is connected with Gr. *ἀνάλωμαι* as proposed by Čop.²⁴ Certainly the resemblance between the Greek verb and the Hittite derivative *hanhanijai* “blames” inspires immediate confidence (even if a direct equation is dismissed by Čop himself as “zu kühn”), but there exists another, and to my mind preferable, possibility of etymological connection, whereby the Lycian demand for labialization can be met. I am thinking of *ātau* “damages, deceives” from **ātau*, a secondary thematicization of an old athematic verb.²⁵ To this verb are found the abstract *ātē* “deception”, Lesbian *avata* (both < **α^Fατᾶ*) and the iterative verb *āσκει φθέρει* (Hes.). The last-mentioned form may be identified with the Anatolian iterative: both *āσκε-* and *haski-* may unforcedly be derived from a protoform **Hw_hske-*. The first steps in the development must, then, have lead to **α^Fασκε-* and **h^wanske-* in the individual dialects. As is seen from such instances as Hitt. *huwant-* “wind” ~ Gr. *ἄ(F)ησι* (= Skt. *vāti*) “the wind is blowing”, the group laryngeal + /w/ is preserved in Hittite (provided, of course, the laryngeal is retained). In the initial conso-

²² Meriggi's emendation (*Hirt-Festschrift*, 1936, p. 264) of *wirasajajañkre* to *-ñqrē* is obviously correct, although the meaning of the passage is totally obscure.

²³ *Orbis* 17, p. 482. The *t-* is probably not part of this word, cf. the preceding note.

²⁴ Bojan Čop, *KZ* 85 (1971), p. 26–30.

²⁵ P. Chantraine, *Grammaire homérique* I (Paris 1958), p. 356.

nantism of *qastte* and *haskizzi* we must, therefore, assume a more intimate combination of spirant and lip-rounding than in the case of *huwant-*. The form **H^wnske-* thus arrived at has two advantages: it corresponds structurally to the well-known formation **g^wmske-* (Gr. βάζω and Skt. *gácchati*), and we understand why a sequence */Hwⁿs/* has not resulted in **Huns-*. The Lycian radical verb *qati* is then explainable as from **H^wen-ti* presenting the same structure as the Skt. root-aorist *á-gan* (**g^wem-t*).

The word *qla* “assembly” is trickier; but if we take the underlying form to be **H^w(V)la-* the connection with Doric ἄλιξ “assembly”, Ionic ἄλιξ, Aeolic ἀολίξ “gathered, crowded” becomes evident. The Greek words then go back in the first instance to **ᾱλῖα-* (with the suffixes **-ijā* and **-ēs*), behind which we may safely posit an IE **H^wln-*. There remains, however, the difficulty presented by the vocalism of Luw. *hila-*, Hitt. *Ḫhīla-*, *Ḫhēla-* “courtyard”, whose spellings hardly indicate an initial cluster, but rather reveal an underlying form */h^(w)ela-|*. We are dealing here with one of the cases where Luw. *i* corresponds to Hitt. *e*, a situation seen in e.g. Luw. *issari-* = Hitt. *kessera-* “hand”.²⁶ Further proof that the *-i-* of *hila-* is in fact an old **e* is supplied by the zero-grade alternant seen in Hitt. *Ḫhilamni* “im Torbau” with the variant *Ḫkilamni* presenting an incorrect writing of *k* instead of *h* otherwise only seen in consonant clusters²⁷ and so undoubtedly indicative of the reading */h^lamni|*. In spite of all the difficulties we have, then, no possibility of deriving Lyc. *qla* from anything other than IE **H^welo-*.

The most conclusive piece of evidence is that of the theonym *trqqas*, Mil. *trqiz*, corresponding to Luw. *ḪTarhunz*, Hitt. *ḪIM-unz*. Unless the Luw. form is merely a graphic representation of *[tarx^wnts]* with preserved syllabic nasal, we must operate with two distinct Anatolian proto-forms, **tarH^wants* and **tarHunts* presenting a difference of vocalization of the same underlying phoneme sequence */Cx^wnC|*; the IE proto-form is probably to be posited as **terH^w-nt-*. The word is undoubtedly connected with Hitt. *(-za)*

²⁶ Since both *e*'s of *kessera-* cannot possibly have been accented, one may ask whether or not the Luwian two-fold representation of Anatolian */e/* (*a* and *i*) is associated with an accentual difference.

²⁷ Friedrich, *Heth. El.* I², p. 32f.

²⁸ The forms with *-u-* of this verb have been collected by Polomé, *Evidence for Laryngeals*, p. 43f.

tarhzi “conquers”. The graphic notation of this verb presents some very interesting variations, thus the three-fold shape of the 3rd sg.: *tar-ah-zi*, *tar-hu-uz-zi*, *ta-ru-uh-zi*. This variety of forms may safely be boiled down to the common denominator $[tarh^{wz}i]$,²⁸ especially if we compare the writings *e-ku-uz-zi* and *e-uk-zi* both noting $[ek^{wz}i]$ “drinks”.²⁹ We probably have a cognate of this group of words in Gr. *τιτρώσκω* “pierce, damage”. Judging from the further testimony of the abstract *τρωμα*, Attic *τρωμα*, we must accept Martinet’s³⁰ labialized laryngeal and posit the IE proto-forms $*ti-t_rH^w-ske$ and $*t_rH^w-m\eta$. The Skt. forms *tarute* (prs.ind. middle of the semi-modal *táratī*) and *táruṭr-* “conqueror”³¹ have vocalized this laryngeal as *-u-*: IE $*terH^w-t-$.

To the list of words containing *q* to which I venture to propose an IE etymology, may be added the Milyan (“Lycian B”) nominal stem *m̃qre-*. This word is taken by Ševoroškin as a designation for “ancestors”. It occurs repeatedly in conjunction with a word *abura* which must mean something like “(living) people” as appears from its contrast with *eke* dat.pl. “to the dead”. This would give to the word *m̃qre-* itself a meaning like “dead, deceased”, which suits the contexts well enough, though not accepted with this meaning by Ševoroškin himself. He translates³² 44c40f *m̃qre*: *muri*: *tupleleimi* [- - -] $[az$: *sebe sbirtę pzziti*: *lelebedi*: *χñtabasi*:) - - as “(Hier . . .) wirft der kriegerische Sieger/ Kämpfer den *m̃qre*, die *-as* und das anführerische Denkmal durch Angriffe nieder” with a note explaining *m̃qre* as “irgendeine Personenklasse (viell. Priesterschaft bzw. Adel . . .), hier der gegnerische *m̃qre*, der niedergeschlagen wurde.” If the general sense of Ševoroškin’s translation is correct, one would rather tend to understand *m̃qre* . . . *χñtabasi* as “the body of the (fallen enemy) leader”, who, together with the memorial of his ancestors, is removed or otherwise treated with contempt (Hitt. *pessijazzi*) by the victorious Xeriga. This appears to agree with 44d1 [*me-*]

²⁹ Lindeman, RHA fasc. 76 (1965), p. 29-32. Probably right despite the doubts expressed by V. V. Ivanov, *Chet’skij jazyk* (Moskva 1963), p. 82f.

³⁰ A. Martinet, *Economie des changements phonétiques* (Berne 1955), p. 225f (already published 1953 as “Non-Apophonic O-Vocalism in Indo-European” in *Word* 9). Martinet is probably right in considering the vocalism of *τρωμα* as a generalization of the prevocalic alternant.

³¹ Thus in Geldner’s translation of RV I.129.2. The Skr. *-u-* is also compared to the Hitt. *-u-* forms by Polomé, loc. cit.

³² *Orbis* 17, p. 473.

ne-d(e) m̃qre : etrqqi tuwiji : qrbli : [z]ireimedi which is translated by Ševoroškin,³³ “Nun ehre³⁴ den *m̃*. in *q*. durch verzierte ‘Weihung’”. If *qrbli* is connected with Hitt. *harpali-* as assumed by Š., it means “on the altar”. Š. makes the annotation that *m̃qre* denotes “eine Gruppe von Menschen (Ahnenschaft??)”. The characteristics of this group would, however, consist in their capability of being the object for religious “consecration”.³⁵ We are now practically forced to think of the translation “manes”. This meaning is almost completely certain at 44d65ff *zumala-de neñijeti : masx̃x̃m̃ ti (i)je : qzze | mirẽñe : xinasi-ke : sesi : m̃qri-ke (a)bura seb(e) e|nesi-ke ledesi-ke : xugasi : x̃ñtawaza :*). Ševoroškin’s translation reads,³⁶ “Kumala leitet die Anordnung an die Nachkommen des *mire* (wohl des Königs Keriga), an seiner und seiner Grossmutter *m̃qre* und *abura* (Ahnen und Verwandte . . . ?), sowie an den mütterlichen, väterlichen und grossväterlichen *k̃ñtawaza*”. The subject worships his relatives, whether these be still alive (*abura*) or only accessible in the manifestation of the *manes* (*m̃qre*). To this may be added 44d53ff *me-x̃eri[ga]/zñ : m̃qre : . . . | . . . lqte : (e)ripsse* “den *m̃qre* des Keriga . . . entsühnte/ verehrte (?) aber der Übermächtige (wohl = Keriga)”.³⁷ Whoever is the subject, *m̃qre* must in any case be the object of *lqte* which Š. very convincingly derives from the Lycian correspondence of Hitt. *lā-* “untie, release”. We read then something like “they released the *manes* of Xeriga”, which can hardly be understood in any other way than as referring to Xeriga’s heroic deeds that have to a certain extent spared him the hardships of the Underworld.

At 44d58 there appears a form *mq̃m̃re*. Though the context is obscure, the parallelism between this *mq̃m̃re-ke (a)bure* and 44d66 *m̃qri-ke (a)bura* is, however, so clear that Ševoroškin remarks, “In d66 kommt statt *mq̃m̃re* D. Sg. *m̃qri* vor”.³⁸ It is not altogether clear whether he considers *mq̃m̃re* and *m̃qri* two case-forms of the same stem. 44d27f *tasñ . . . meñmezñ* is translated by Ševoroškin³⁹

³³ Ibid., p. 482.

³⁴ Likewise *Voprosy jazykoznanija* 1968, 6, p. 73.

³⁵ *tuwiji* derived from *tuwi* “Weihung, Weihgeschenk” (*Gusmani Arch. Or.* 36, p. 3⁹ and 8⁴⁰).

³⁶ *Orbis* 17, p. 490.

³⁷ Ševoroškin, *ibid.* p. 489.

³⁸ Ibid., p. 481¹.

³⁹ Ibid., p. 485.

as "das Denkmal der Ahnen", but the morphological details are not entered into.⁴⁰ There exists, no doubt, a serious possibility that the forms acc. *m̃qr̃e*, dat.-loc. *m̃qri*, and dat.-loc.pl. *mq̃m̃re* belong together in one paradigm. First we must remember that *m̃* is before *q* merely a nasal archiphoneme (as stated above, no instance of **ñq* occurs in the inscriptions); phonemically, then, we have $/Nx^{w}r-/\mathit{.}$ A vowel must have disappeared between the nasal and the laryngeal, and we have now merely to look for an IE root **neH^w-* or **meH^w-*. The Indo-Europeanist need not thumb through Pokorny's IEW very long, till he finds the old acquaintance **nāu-* "Tod, Leiche". Of this root ORuss. *navb* "dead body" presents the same pre-vocalic alternant as *pravb* "right" of *preH^w-*.⁴¹

How may this now be reconciled with *mq̃m̃re*? I have the impression that the difference between *m̃qr̃e* and *mq̃m̃re* is connected with a shifting accent. In an IE mobile paradigm the acc.sg. (*m̃qr̃e*) and the loc. sg. (*m̃qri*) belong to the cases with stem-accentuation, while the dat.-abl. pl. (*mq̃m̃re*) exhibits oxytony, as seen in e.g. Skt. *pitāram pitāri* and *pādādam padbhyās*, cf. Lith. acc.sg. *dùkterj*, dat.pl. *dukterims*. We must, therefore, depart from:

acc.sg.	<i>*néH^wr-m̃</i>	>	Anatolian	<i>*náx^wran</i>
loc.sg.	<i>*néH^wr-i</i>	>	—	<i>*náx^wri</i>
dat.-abl.pl.	<i>*neH^wr-ós</i>	>	—	<i>*nax^wrás</i>

At this period a kind of Verner's-Law alternation has come about, whereby **x^w* kept its voicelessness only after accented vowel, but succumbed to the pressure of the voiced surroundings after unaccented vowel:

<i>*náx^wran</i>	>	<i>*nax^wran</i>
<i>*náx^wri</i>	>	<i>*nax^wri</i>
<i>*nax^wrás</i>	>	<i>*nay^wras</i>

Subsequently, the great loss of vowels that has characterized the Lycian words so strongly, takes place according to rules not sufficiently clear:

⁴⁰ The article "Lykische Wörter und Namen" by Korol'ov and Ševoroškin (Arch. Or. 37, p. 523–542) does not touch upon this question either (as would be natural on p. 530 or 542).

⁴¹ Martinet, *Economie*, p. 226.

<i>*nax^wran</i>	>	<i>*ηx^wran</i>	>	<i>*ηx^wrϵ</i>
<i>*nax^wri</i>	>	<i>*ηx^wri</i>	>	<i>*ηx^wri</i>
<i>*naγ^wras</i>	>	<i>*$\eta$$\gamma$^wras</i>	>	<i>*$\eta$$\gamma$^wre</i>

The nasal is now assimilated to the following spirant:

<i>*ηx^wrϵ</i>	>	<i>ηx^wrϵ</i> , written <i>m̃qrϵ</i>
<i>*ηx^wri</i>	>	<i>ηx^wri</i> , written <i>m̃qri</i>
<i>*$\eta$$\gamma$^wre</i>	>	<i>*$\eta$$\gamma$^wre</i>

In the last-mentioned form there occurred the further assimilation nasalizing the spirant:

<i>*$\eta$$\gamma$^wre</i>	>	<i>*ηmre</i>
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The development of ** η mre* to *m̃qm̃re* is parallel to that of **nanijati* (Hitt. *nannijazzi* ‘‘leads’’) to *n ϵ nijeti* 44d65, where we would reckon with a transitional stage with loss of vowel: **nanijati* > ** η nijati* > *n ϵ nijeti*. In both of these cases a vocalic nasal followed by a (homorganic) consonant nasal develops into nasal consonant + nasal vowel.

Of the Lycian, as on the whole of the Anatolian, accent we know practically nothing. Lycian does, however, appear to offer some very precious examples of accentual shifts. When we find *adi* for ‘‘he does’’ and *pijeti* for ‘‘he reserves/gives’’, the distribution of *-di* and *-ti* is seen to match that of *-gq* and *- χ q* of the 1st sg. forms *agq* and *pija χ q* (both in the same line of the same inscription 149.13), and the thought of an accentual difference between the two verbal structures imposes itself. Luwian has *ajatti* and *pijatti* which we may easily conjecture to be accented *| \acute{a} jati|* and *|pij \acute{a} ti|*. We may compare, on one hand, Hitt. *ijazzi ijanzi* ‘‘do’’, probably a reduplicated verb of the type of Gr. *ἴσθημι* or, if thematicized, Skt. *tīṣṭhati*, accented in either case on the reduplicative syllable, and, on the other hand, Hitt. *pāi pijanzi* ‘‘give’’, an old athematic verb secondarily thematicized to *|pij \acute{a} ti|* in Luwian through a process comparable to that of Vedic *y \acute{a} uti yuv \acute{a} nti* ‘‘unite’’ → *yuv \acute{a} ti yuv \acute{a} nti*, i.e. with the accent on the thematic vowel. A development ** \acute{a} jati* > ** \acute{a} iti* belongs to the most natural of its kind, especially in a language presenting syncopation, and the monophthongization of *ai* to *a* is known from Mil. *pinati* 44c57 ‘‘gives’’

: Hitt. *pijanāizzi* “beschenkt”.⁴² The different treatment of *-ja-* in *pijeti* and *pinati* may also be ascribed to the effect of the accent. Hitt. *pijanāizzi* belongs to the laryngealistically interesting type I 3 of Friedrich’s classification, presenting a suffix alternation between (originally accented) *-āi-* before consonants and zero before (originally accented) vowels (*handaizzi*: *handanzi*, cf. Goth. *habaiþ*: *haband*), which must be connected with the accent of the IE early ablaut period. The verb *tadi* “lays, buries” is probably to be judged in the same way as *adi* and to be derived from older **tājati* (based on the paradigm seen in Hitt. *dāi tijanzi* whose thematicized form **tijāti* probably underwent a shift of accent under the influence of **tjati* = Luw. *ajatti*). The old radical accentuation of the athematic verb **H^wénti* gave the expected *qati* with *-t-*.

If, in fact, there existed at a certain period in the history of Lycian a regular alternation of voiced and voiceless consonants we must expect, too, to find our voiceless *q* [*x^w*] matched by a voiced counterpart [*ɣ^w*]. Though the Lycian alphabet is basically of Greek origin, its details are clearly indigenous, and so we would expect it to contain a special letter for [*ɣ^w*]. Our attention is now quite naturally attracted by the otherwise superfluous letter *μ*. This is commonly considered a bilabial spirant⁴³ and transcribed (if at all) by *β*. But the language possesses one spirantic *b* already, as is obvious from its use in the numeral *kbihu* = Mil. *tbišu* “twice” (formed like Hier. Luw. III *tarsu* “three times”) with IE **dw-*. Nobody would, I presume, maintain in full earnest that a language could distinguish, alongside with a *w* (which is in Lycian doubtless a bilabial semivowel), two varieties of [*β*]. We have the following alternations:

p ~ *b* [*β*] *pibijeti* 149.3 and 5, 44b44, redupl. = Luw.
 *pipija*⁴⁴
t ~ *d* [*δ*] *pijeti*: *adi* above

⁴² Ševoroškin, *Orbis* 17, p. 471f. I cannot accept his example, *ibid.* p. 473 Mil. *pssat[i]* 44d23 “zerbricht”, because the compared Hitt. word *pissai-* is not certified by Friedrich’s *Wörterbuch*.

⁴³ Ševoroškin, *Kadmos* VII, p. 168; *Atti del 1° Congresso Internazionale di Micenologia* (Roma 1968), p. 466f (with the examples); *Klio* 50 (1968), p. 60: “es fragt sich, ob lykische *b* und *β* wirklich verschieden sind”, Korol’ov and Ševoroškin, *loc. cit.*, p. 538: “Viell. wurde durch *β* der spezifische ägäische Laut [β] (stimmhafter bilabialer Spirant) wiedergegeben: das gewöhnliche hl. [w] blieb ja im Lyk./Mil. erhalten”.

$\chi \sim g [\gamma]$ *pijaχq* : *agq* above

k does not alternate: *kikikiti* 55.5 redupl. vb. of unknown meaning.

We see that the distance between the alternants is shorter in the velar pairs than in those with a more advanced articulation. As the alternant of $[x^w]$ we consequently expect a $[\gamma^w]$ in order to obtain the same phonetic distance as with χ : $[\gamma]$.

The following is a presentation of the rather few examples of $^{\mu}$. It is seen in the words:

mr^{μμ}as acc.sg. 44c43, *mr^{μμ}di* 44c32 & 37, 44d48f, to which the possessive adjective *mr^{μμ}asi* 55.4 is added by Gusmani.⁴⁵ Ševoroškin's interpretation⁴⁶ as "word" may very well be correct: *mr^{μμ}as* is the object of *uwēti* which probably means "they write", cf. Lyd. *uved*, prt. *ul* "writes, wrote". Ševoroškin compares Lyd. *mru-d*, *mruvaa-d* "stele"⁴⁷ and further Avest. *mraoiti*, 3rd sg. pres. *mraoiti*, Skt. *brāvīti*. If this comparison is correct the etymology of the Indo-Iranian verb based on the root **mel-* (Gr. *μέλω* "sing", Czech *mluva* "language") must be given up. In that case *brāvīti*||*mraoiti* points immediately to **mréwə-ti* revealing a cluster $|wH|$ and not a monophonemic rounded laryngeal. A proto-form **mrewHó-*would develop regularly to **mrawγá-* and further to **mṛwγα*, and so, under the assumption of the coalescence of * γ^w and **wγ* (and * γw) into $|\gamma^w|$, I find nothing to prevent the ultimate shape $[mṛ\gamma^w\gamma^w\alpha-]$ (with gemination as in *trqqas* above).

la^μra 44c33f & 37, 44d34, *la^μri* 44c43. The latter was already in 1937 translated by Meriggi⁴⁸ as "in der Schrift", by Gusmani⁴⁹ 1968 as "stele" and by Ševoroškin⁵⁰ 1968 as "im Stein". For *la^μra* Šev. has 1965⁵¹ "writing, inscription", but later "stone slab, Steinplatten",⁵² both attempts being supported by Lyd. *λαβρονς* "battle axe" and Luw. *lawar-* "hew, break". We must then, apparently, depart from "stone" or "hew". A combination of these meanings seems to underlie the semantics of Gr. *λάωζα*

⁴⁴ Neumann, loc. cit., p. 390.

⁴⁵ *Arch. Or.* 36, p. 10⁵¹.

⁴⁶ *Lidijskij jazyk*, p. 62.

⁴⁷ *Ibid.*, p. 52.

⁴⁸ *Mélanges . . . Holger Pedersen* (København-Aarhus 1937), p. 515.

⁴⁹ *Arch. Or.* 36, p. 11.

⁵⁰ *Orbis* 17, p. 473.

⁵¹ *Issledovaniija po dešifrovke karijskich nadpisej* (Moskva 1965), p. 256.

⁵² *Lidijskij jazyk*, p. 57; *Voprosy jazykoznanija* 1968, 6, p. 73; *Orbis* 17, p. 470.

“mountain road”, which is etymologically connected with Gr. $\lambda\tilde{\alpha}\varsigma < *λ\tilde{\alpha}F\alpha\varsigma$ “stone”, $\kappa\rho\alpha\tau\acute{\alpha}-\lambda\epsilon\omega\varsigma$ “hard as stone” (*- $\lambda\tilde{\alpha}F\alpha\varsigma$) to which we may easily reconstruct the root $*leH^w-$. The Luw. $-w-$ reveals the voicing after unstressed vowels as, in the least, “Common Luwian” (Cuneiform Luw., Hier. Luw., Palaic [?], Lyc.), if not Common Anatolian. Lyc. *la μ ra* is thus seen to go back to $*la\gamma^wras < *loH^wr\acute{o}s$.

The first member of the presumed compound *almana-lax(a)* 44c60 has been compared by Ševoroškin⁵³ to Hitt. *alwanzatar* “Bezauberung, Behexung” and the whole tentatively translated by “Hexenfeldzug (?)”. It is completely impossible to see from the context what might be the role of the witchcraft thus read into the inscription. All we can see, is that a certain kind of “campaign” (*laxa-* to Hitt. *lahhai-*) is neutralized (*xradi* to Hitt. *harra-* “zerstossen, zerreiben, zermahlen”). If, however, the first component has been correctly assessed by Š., we may compare Gr. $\acute{\alpha}\lambda\acute{\omega}$ “I rage” ($< *_{\alpha}lus\acute{i}\acute{o}$) together with the words treated in Pokorny’s IEW p. 28. The point of departure must, then, be reconstructed as $*H_2lw\acute{o}-$ which must first have undergone a metathesis to $*_{\int}H_2w\acute{o}-$, thereby taking the path to $*al\gamma^w\acute{\alpha}-$, the basis of Lyc. [*al γ^w a-*] and Hitt. *alwa-*. The Hitt. $-w-$ now makes us consider the voicing Common Anatolian, still on the assumption that the example is correct. We see once again that [γ^w] may also stem from a cluster of laryngeal and $/w/$ (or vice versa).

Finally, the genuine Lycian (“Lycian A”) parts of the Xanthos Stele exhibit at 44a39 the fragmentary sequence]^{*u*}*adunimi* which recurs at 44a40 in the longer stretch]*qa^uadunimi*. Ševoroškin⁵⁴ sees in this a personal name which he identifies with Linear A *wadunimi*. I find it totally impossible to draw any conclusions from this example, seeing that the questions of the general context and of word-division are completely open, to say nothing of lexical meaning and morphology.

We have seen that Lyc. $q([x^w])$ may in some words continue an IE rounded laryngeal, and μ (β , [γ^w]) not only such a laryngeal but also a cluster consisting of laryngeal and $/w/$. Whether q may also be derived from a cluster does not appear from the examples

⁵³ *Orbis* 17, p. 479. Somewhat more elaborate Korol’ov-Ševoroškin, loc. cit., p. 533.

⁵⁴ *Nestor*, vol. 1 (1963), p. 258. Treated accordingly in the word-list by Korol’ov-Ševoroškin, loc. cit., p. 538.

treated here, but it is to be expected. I cannot agree with Ševoroškin when he repeatedly⁵⁵ maintains that Lyc. χ and q are the direct descendants of two different "Nostratic" entities, χ being the continuation of a laryngeal, q of a uvular stop. His reference⁵⁶ to Illič-Svityč, *Ėtimologija* 1965 (published 1967) p. 322, is of little avail, seeing that very few of the examples presenting uvulars furnished by the Russian-Nostratic (!) glossary *ibid.* p. 330–373 occur in IE at all, and not a single one of them has a sure correspondence in Lycian (except perhaps for Nostr. **qal* "low", IE **Hel-*, Hitt. *halija-* "kneel", adduced by Ševoroškin⁵⁷ to justify the translation of the obscure words *qliju* 44d59 and *qiqlēniredi* 44d69 as "den qlija (des Grabmals)" and "durch Niederkniefung".

Finally, one may ask whether or not the laryngeal **H^w* which has been used in several of our above calculations is identical with the laryngeal labelled **H₃* by almost common consent. This question cannot, of course, be answered with greater certainty than there prevails in our theories about **H₃* itself. If **H₃* yields Gr. *o-* as a prothetic vowel, we observe in *δρν̄μι*, *δλλ̄μι*, *δμν̄μι* a treatment differing very significantly from that of the rounded laryngeal of *ἀ(Ḟ)άσκει* and **ἀḞαλνίᾱ*. From the comparison of *δρν̄μι* and Hitt. *arnuzzi* "brings, leads" we find this laryngeal to be one of the kind that vanishes in Hittite. Another important phonological difference is observable: in *δλλ̄μι* from **δ₃ln-* the laryngeal *|H₃|* has been vocalized, whereas **ἀḞαλνίᾱ* from **H^wln-* has vocalized the *|l|* in what is otherwise the same sequence of phonemes. In other words, the **H^w* of **ἀḞαλνίᾱ* is of a more consonantal nature than *|l|*, while the **|H₃|* of *δλλ̄μι* is less consonantal, i.e. more easily vocalized, than the sonant *|l|*. Both the Hitt. loss and the relatively poor consonantal character point to a voiced spirant [*γ^w*] (for **H₃* > Gr. *o-*, Hitt. *Ø-*) as against the voiceless [*x^w*] (for **H^w* > Gr. *ἀḞ-*, Hitt. *h-*).

These conclusions may be harmonized quite artlessly with the six-laryngeal system of Lindeman, his **H₃* being phonetically a voiced spirant [*γ^w*] (gr. *o-*, Hitt. *Ø*) matched by the voiceless

⁵⁵ *Orbis* 17, p. 468; *Kadmos* 7 (1968), p. 168; *Vestnik drevnej istorii* 1969, No. 6, p. 150. His article "K rekonstrukcii fonologičeskich sistem" in *Fonologičeskij sbornik* (Donec 1968) has unfortunately not been accessible to me.

⁵⁶ *Orbis* 17, p. 468.

⁵⁷ *Ibid.*, p. 489f.

* H_3 [x^w] (Gr. $\acute{\alpha}\mathcal{F}$ -, Hitt. h -, $-hh$ -, Lyc. $q \sim \mu$). The fact that monophonemic * H^w - develops in Greek exactly like the diphonemic * Hw - (the latter in Gr. $\acute{\alpha}(\mathcal{F})\eta\sigma\iota$ above), is in full accordance with the overall tendencies of Greek historical phonology where, e.g., * k^w and * kw in the vast majority of cases exhibit the same development.

Addenda

To p. 5. Other alleged one-vowel languages are Kabardian (N.W. Caucasus) and Wishram (Chinookan group of N. America), see W. Sidney Allen, "On One-Vowel Systems", *Lingua* 13 (1961), p. 111–124, and on Kabardian especially the monograph by A. H. Kuipers, *Phoneme and Morpheme in Kabardian* (The Hague 1960) and the same's paper "Unique Types and Typological Universals" in *Pratidānam* (Fs. F. B. J. Kuiper, The Hague 1968), p. 68–88, the latter containing a highly spirited discussion of typological arguments and pseudo-arguments against the acceptability of such minimal sub-systems.

To p. 9. During a recent stay in Vienna, J. Schindler kindly drew my attention to the lengthy study by Heinz-Jürgen Pinnow, "Sanskrit – eine Sprache ohne Vokalphoneme? (Vorschläge zur Erstellung des Phonemsystems des Altindischen)", *Folia Linguistica* 3 (1969), p. 255–306, where some of my views have been anticipated. I must say, however, that I find this author's phonemization of e.g. Skt. $mṛtyoḥ$ as // $mṛty\acute{v}\acute{s}$ // and of Skt. $\acute{a}pnoti$ as // $\cdot pn\acute{v}t\acute{y}$ // (both p. 298) inferior to my own as regards both economy (why distinguish y and \acute{y} if they never contrast?) and its ability to map the morphophonemics of the language (// $\acute{y}\acute{y}\acute{v}\acute{s}$ // for $\acute{y}ur$ hardly accounts for the morphology as well as // $Xy-Xy-vS$ // does). The main point, however, whether one chooses to write // a // and to speak of one vowel or prefers // \cdot // and sees no vowel altogether is only a matter of taste, but to accept *Silbengipfel* as phonemic without realizing that the existence of one element which is always syllabic means the existence of one vowel is, at best, bad taste. My own analysis is partly congruent with that of V. V. Ivanov & V. N. Toporov, *Sanskrit* (English edition Moscow 1968 [Russian edition 1960]), p. 46. The authors, however, only claim the single vowel / a / "for the early Indo-

Aryan period" and not for Class. Sanskrit as I would and as Pinnow reads it (p. 259). The morphophonemic analysis containing one "laryngeal" written X is, it seems, entirely my own and, for the reasons stated in the text, preferable to other attempts.

To p. 25. The Pre-IE paradigm of the word for "foot", preceding IE **pód-s*, gen. **péd-s* would be **pÉdEs*, **pĒdÉs*, the two cases being distinguished by accent only. The symbols \bar{E} and E stand for the alternations $\acute{e} \sim e$ and $\acute{e} \sim \emptyset$ respectively, the phonetic development of **pĒdÉs* to **péd-s* being parallel to that of **H₁ĒdÉnt* to **H₁édnt* ("acrostatic" noun and verb in the Erlangen terminology, cf. Eichner, *MSS* 31 (1972), p. 91). Likewise **dó(m)* **dém-s* is from **dĒmEs* **dĒmÉs*. The old ergatives were, then, merely **pĒdEs* and **dĒmEs* with unspecified accent, different syntactic patterns causing the later fission into nom. **pÉdEs* and **dĒmEs* (with "unmarked" accent) and gen. **pĒdÉs*, **dĒmÉs* (accent attracted by enclitic second member of combinations like $\delta\epsilon\sigma\pi\acute{o}\tau\eta\varsigma$). (For the IE paradigms underlying my Pre-IE constructs I am indebted to the highly inspiring teaching of my distinguished friend and colleague Jochem Schindler and to countless discussions with the same during the fall term of 1973. Schindler's views on this inflexional type are set forth in his paper "L'apophonie des noms-racines indo-européens", *BSL* 67 (1972), p. 31–38).

To p. 45. W. Meid, *Die Romanze von Froech und Findabair, Táin Bó Fróich* (Innsbruck 1970), p. 163, favours the equation of OIr. *-f-* with Lat. *-b-*, but seems to forget that the underlying segment was **-bhw-* and not just **-bh-*.

To p. 63. Lyc. *tadi*, 3rd pl. *tqti* is perhaps rather $< *táati$ $*táanti$, a *mi*-transfer of Hitt. *dāi tijanzi* (**dhéH₁-oi* **dhéH₁onti*). As shown by *aitē* "they made" of the trilingual inscription recently discovered at Xanthos, a sequence *-ájanti* would undergo syncope and denasalization of the resulting *i*-diphthong (*-ainti* $>$ *-aĩti* $>$ *-aiti*) and so *tadi tqti* probably never contained any *-j-*. Likewise *qqti*, 3rd pl. *qqñti* is probably the thematicized variant based on the stem of Hitt. *hannanzi*, i.e. **H^wéneti* **H^wénonti* with syncope of unstressed vowels.

The trilingual stele of Xanthos whose text has now been published in the *Académie des Inscriptions & Belles-lettres, Comptes rendus des séances de l'année 1974*, p. 82–93 ("Le texte grec" by

H. Metzger), p. 115–125 (“Le texte lycien” by E. Laroche), and p. 132–149 (“Le texte araméen” by A. Dupont-Sommer), contains valuable though hardly conclusive evidence for the phonetic value of the letter ^μ here advocated. The four occurrences are all variants of the personal name *Ar^{μμ}azuma* (line 8), *Ar^{μμ}azumahi* (18), *Er^{μμ}azuma* (27 f), *se-R^{μμ}azumaha* (24 f), rendered by Ἀρχεσιμυς in the Greek version and unfortunately damaged beyond the initial *R* [in its two occurrences in the Aramaic text. Now Greek <κ> is the transcription of Lyc. *k* (*Εκκτομνω* = *Katamlah*, *ibid.* line 2), *χ*, *g* [*γ*] (44c31 *Κα[ρ]ιχα* = *Xeriga*), and *q* [*x^w*] (*Qñturahahñ* tril. 10 = *Κονδορχσιος*), so why not of [*γ^w*] as well? However, the Greek rendering may well be influenced by popular etymology: ἀρχέσιμος “assisting” (sc. θεός on a Jewish inscr. of Syria, CIG 9899), which makes the example inconclusive one way or the other, as observed by my friend and colleague Martin Peters of Vienna.

Typographical Note

The Lycian letter resembling an archaic M with five hastae is here for technical reasons printed ^μ. This is no recommendation for future transcription. If my phonetic interpretation is correct one would suggest to write *γ^w* or *γ^o*, the best procedure being perhaps for the time being Laroche’s *Ar??azuma*.

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