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I. AUFSÄTZE

The Expression of Purpose in Indo-European

Abstract

The early Indo-European dialects indicate two means for expressing purpose in the parent language: nominalization (reflected in infinitive and supine propositions of purpose) and finite constructions characterized by the functionally equivalent markers of interclausal relationship **i̯o* and **kʷo* (reflected in relative clauses of purpose, as well as purpose constructions introduced by a conjunction). This analysis of markers of purpose suggests an explanation for the dichotomy of sentence connectives which develop from **i̯o/kʷo* into coordinators on the one hand and subordinators on the other.

0.0. In this paper we are concerned with the prehistory and development of so-called purpose or final constructions in the ancient Indo-European (IE.) dialects. We shall confine ourselves to the three major types of purpose constructions attested in Indic, Greek, and Latin: (1) infinitive; supine propositions, (2) finite clauses introduced by a conjunction, and (3) relative clauses[1]. We shall also consider important evidence from Anatolian which bears crucially on our explication of the history of purpose clauses in Indo-European. We shall present claims about the types of constructs from which the extant purpose expressions originate and shall develop hypotheses concerning the nature of the diachronic developments which produce the extant patterns. In this way we hope to define the nature of purpose constructions in relation to the general phenomenon of subordination and to provide a basic insight into the process of subordination itself.

[1] All other types of purpose expressions, e. g. the Sanskrit future active participle, are language specific and clearly of late origin. These will not be considered in this paper.

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1.1. We shall first consider the use of verbal nouns (i. e. infinitives and supines of purpose) in our examination of purpose constructions in the most ancient IE. dialects. Vedic, Latin, Greek and Hittite employ infinitives of purpose. (See examples 1.*i*–1.iv.)

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1. i. indram avardhayann ahaye hantavā u
they strengthened Indra to slay the dragon
(RV. V 31,4)

ii. venerat aurum petere
He had come to seek the gold.
(Plaut., Bacch. 631)

iii. ēōteu d' Idaios ítō koílas epì nēas eipémen Atreídēs
At dawn let Ideas go to the hollow ships to declare to Atreus' sons.
(Il. 7 362ff.)

iv. nu-mu-za-kan MI.KAM-za u̯alḫu̯anzi zikkir
They set out to seize me at night
(KBo. IV 4 III)

Although *-tum* (from the accusative of *tu*-stem verbal nouns) occurs in all infinitive contexts in Sanskrit, including purpose, we find that this is not so in the earlier language. Jeffers (1975) shows that in Vedic there are two groups of infinitives. One is archaic and unproductive, and is apparently dative-locative in origin. The second group is later and productive and occurs in the genitive-ablative, dative, and accusative. This group is based on *ti*-stems, *tu*-stems, and radical alternating with *i*-stems (See Kantor and Jeffers 1976). Taking the *tu*-stems as illustrative (they show the greatest variety of case forms), we find that infinitives in *-tos* (gen.-abl.) never occur in purpose constructions; infinitives in *-tave* or *tavai* (dative) are regularly used in purpose contexts; infinitives in *-tum* (acc.) are used to indicate purpose with verbs of motion. We note that verbs of motion in IE. seem universally to take the accusative of goal.

1.2. In Latin we find two kinds of verbal nouns expressing purpose: supines and infinitives. The supine, as the Sanskrit infinitive, is a *tu*-stem verbal noun which occurs primarily in the

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dative or accusative. The accusative supine occurs almost exclusively with verbs of motion. The dative supine occurs regularly in other purpose contexts.

The Latin infinitives in *-re* and *-ri* reflect their origins as dative-locative forms of *s*-stem nouns (Leumann, et al., 1963: 328, Jeffers 1975: 142, fn. 15, 1977: 181ff.). Infinitives of purpose may also follow verbs of motion in Old Latin. The supine, an accusative case verbal-noun, comes to replace the infinitive as a means of expressing purpose in this context (Bennett 1910: 418–19; Jeffers 1975: 146).

1.3. In Greek we also find infinitives of purpose, and the Greek infinitive again represents an earlier verbal noun in the locative, for the most part reflecting the endingless locative as in Arcadian *-en*, and general Greek *-ein* (< **-esen*) and *-men*[2].

1.4. In Hittite, we find that there is associated with every verb a verbal substantive, generally termed the infinitive (Friedrich 1960: 142) which is nominal (i. e., it is inflected and takes objects in the genitive). For *mi*-verbs which show ablaut the ending is *-atar*; for other *mi*-verbs and *ḫi*-verbs and ending is *-uwar*. These verbal nouns show case forms in the genitive, dative-locative and endingless locative. The dative-locative forms function like the infinitives of purpose just discussed. The dative-locative form of *-atar* is *-anna*; for *-uwar* the dative locative is the secondary form *-uwanzi* (Friedrich 1960: 142–3).

1.5. It is clear that the use of action nouns of the restricted set described in Jeffers 1975 were commonly used in PIE. in most typical purpose contexts. We can furthermore reconstruct the use of dative-locative case forms of the nominal in most such propositions, with the exception of situations where the verb of the matrix clause is a verb of motion, in which case accusative verbal nouns would have occurred. These nominal constructions are generally reflected in the so-called infinitive and/or supine phrases (propositions) of purpose.

2.0. We turn now to cases where purpose is expressed by full subordinate clauses. We shall treat first those cases where

[2] For more complete discussions of Greek infinitive formations, see Chantraine 1961: 273–80, and Jeffers 1977: 20ff.

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the purpose clause is introduced by a conjunction, and then consider relative clauses of purpose.

2.1. In Sanskrit the basic purpose conjunctions are *yad* and *yathā*. *yad*, from **i̯o*, is widely considered to be etymologically equivalent to the nominative-accusative neuter of the relative pronoun and stereotyped as a wide-ranging conjunction. *yathā* is also held to be a derivative of **i̯o*, with the clitic particle *-thā* going unexplained. (Mayrhofer 1953).

2.2. The basic subordinator in Latin is *ut*, which is related to the relative stem **kᵘe/o*, from **kᵘuta* + *i* or **kᵘuts-* (Leumann, et al. 1963: 124).

2.3. In Greek we find five conjunctions which may indicate purpose, all of which are related to the relative stems: (1) *héōs* from **ya-wos* (cf. (Skt.) *yavat* < **ya-wn̥t*) (Schwyzer I 528), (2) *hóphra* from **yo* + *phra*, (*-phra* is an unexplained particle (Schwyzer I 631); (3) *hópōs* from **yod* + *kʷo-*, with unclear inflectional morphology (Schwyzer I 617); (4) *hōs* from **yōs*, (Schwyzer I 528), 614ff.); (5) *hína* from **ye/o*, probably exhibiting the same ending as the instrumental marker of Skt. *-ena* (Schwyzer I 615). *hína* later becomes an all-purpose subordinator in Greek, surviving into the modern language as *na*. Notice here that all subordinating conjunctions associated with purpose constructions are related to the relative stems. Below are examples of purpose clauses with each of the conjunctions referred to in section 2.

2. Vedic

i. gṛhān gacha gṛhapatni yathā asaḥ
go to the house so that you may be mistress of the house
(RV. X 85, 26)

ii. sa ā vaha devatātiṃ yaviṣṭha sardho yad
So bring hither the gods, O youngest, so that today you
adya divyam yajāsi
may worship the heavenly host
(RV. III 19, 4)

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Latin

iii. revorti ut me purgarem
I returned to clear myself.
(Plaut., Amphitruo 909)

Greek

iv. óphra and hós
. . . hízeu emeîo óphra ídēis . . . hos moi kaì tónd'
. . . sit before me so that you may see . . . and so you
ándra pelőriou exouomēnēis
may tell me who the huge man is
(Il. 3, 162ff.)

v. hópōs
parakaleîs iatroús hópōs mē apothánę
you call on physicians so that he may not die.
(Xen., Memorabilia 2, 10, 2)

vi. hína
katámene hína kaì perì sou bouleusómetha
remain behind that we may consider your case also.
(Xen., Anab. 6, 6, 28)

vii. óphra
Trōsin eph' hippodámois ithýnomen óphra kaì Héktōr
We shall drive (them) against the horse-taming Trojans
so that Hektor knows . . .
eísetaí . . .
(Il. 8, 110)

viii. héōs
dőken . . . eláion héiōs khytlősaito
she gave olive oil so that she might annoint herself.
(Od. 6, 79)

3.0. In these same languages we find purpose expressions which take the form of relative clauses[3]. In such cases, relativi-

[3] In Indic the decline of the relative clause of purpose is already seen in the Brahmanic period as pointed out, e. g., by MacDonell (1910). The situation in Greek is unclear, since the so-called relative clauses of purpose are of two types: those with the subjunctive in the relative clause and those with the future in the relative clause. Only the latter type survives into Attic usage, but whether such constructions should be classified as true relative clauses of purpose is at present unclear and under investigation by the authors.

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zation serves to indicate the semantic focus of the purpose construction. That is, relativization is one means of indicating morphologically and syntactically that a noun which occurs in both the main and subordinate clauses is being focussed upon (cf. Schachter 1973). Thus in Latin, for example, we find conjoined purpose clauses in which one clause is introduced by *ut*, and so is neutral as to focus, while the second purpose clause is introduced by a relative word which signals the focus of the clause (cf. Pepicello IF. 82). This can be seen in sentences like (3.i), in which we find two clear purpose clauses, one introduced by *ut*, the other by a relative word. 3.ii–3.iv are representative examples of relative clauses of purpose.

3. i. Verum certum est confidenter hominem contra conloqui, qui possim
I'm resolved to speak up to the man boldly, so that I'm able
videri huic fortis, a me ut abstineat manum
(Plaut., Amphitruo 339)
to seem strong to him, and so that he'll leave his hands off me.

ii. Mittitur L. Decidius Saxa qui loci naturam perspiciat
(Caes., B.C. 5, 10)
L. Decidius Saxa is sent to inspect the nature of the place.

iii. Scribebat Aelius orationes quas alii dicerent
(Cic., Brut. de Claris Oratoribus 56)
Aelius wrote speeches for others to read.

iv. Curio praemittit equites qui primum impetum sustineant
(Ceas., B.C. 2, 26)
Curio sends forward cavalry to withstand the first attack.

4.0. On the basis of the comparative evidence of the three anciently attested non-Anatolian IE. dialects, we are forced to conclude that the markers which we generally associate with relativization in IE., **i̯o* and **kʷo* play a significant role in the development of finite final clauses in the IE. languages. All three languages offer relative clauses of purpose, and all three languages

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make use of conjunctions (albeit different ones) based on the **i̯o/kʷo* stems in subordinate purpose constructions. We lack, however, some hypothesis about the sequence of events which might have led to the obviously related but divergent constructions of purpose in the extant languages. It seems appropriate to consider first these so-called "relative particles" themselves, **i̯o* and **kʷo*, which characterize all finite purpose constructions.

4.1. It has become increasingly clear that the forms **i̯o* and **kʷo* did not serve originally as relativizers of the type which we find in the pronominal systems of languages like Sanskrit, Greek, and Latin. As early as 1932, Porzig, in his Die Hypotaxe im Rigveda, calls attention to the non-relative conjunctival uses of *ya-*. Recently, and in large part as a consequence of the integration of Anatolian evidence in attempts at the reconstruction of Proto-Indo-European (PIE.) syntactic patterns, scholars like Watkins (1963) have been suggesting that the particles **i̯o* and **kʷo* 'belonged originally to the set of connectives' (1963: 16). Watkins apparently wishes to class these particles with sentence connectives like **so-*, **to-*, **no-* and **e-*, which particles may occur clause-initially or as clitics attached to some other clause-initial word. However, **i̯o* and **kʷo* are to be distinguished syntactically from these other sentence particles in that there seems to be no convincing evidence that they could appear clause-initially in early times. It might well be suggested that this formal syntactic difference might be indicative of a functional distinction between simple narrative connectives like *no-* and the functionally equivalent (Watkins 1963: 11) clitics **io/kʷo*.

4.2. More recently, in his monograph Die Indischen Sprachen (1971), Gonda coins the somewhat puzzling term *includer* to characterize PIE. **i̯o*, and he goes on to claim that the 'relative function of (*ya-*) developed . . . in those sentences which contained an anaphoric demonstrative (resumptive) pronoun, or another form functioning as a correlative'. (140) Gonda's claim appears quite convincing. The subordinate character of the **i̯o/kʷo* clause might then be seen to be somehow marked by either of these clitic particles, while the focus of the interclausal relationship would in part be clarified through clitic pronominal reference.

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The evidence of Anatolian clearly demonstrates a pattern of sequencing for clitic particles which finds significant correspondence with relic forms in other old IE. dialects (Watkins 1963). That **i̯o/kᵘo* appears in the first clitic slot is well established, as is the fact that pronouns fall into the second slot. We might then reconstruct patterns like **no-* + $\left\{\begin{matrix} i̯o \\ k^{u}o \end{matrix}\right\}$ + *-(o)s* (nom.). With the shift away from the use of cliticization toward a greater dependence on isolated words, a development which we see in virtually all the extant dialects outside Anatolian, several such sequences might well be interpreted as mono-lexical inflected words (Hittite *ia-aš* = Skt. *yas*). Such a reinterpretation might establish a pattern for the spontaneous generation of a full pronominal paradigm—the origin of relative pronoun/adjectives.

4.3. We are convinced that **i̯o/kᵘo* is not to be construed as a relative marker *per se* for PIE. The diverse reflexes of these particles in relative pronouns, indefinite/interrogative pronouns, coordinating conjunctions, and subordinating conjunctions demands some coherent explanation. Watkins' assumption that **i̯o/kᵘo* were simple sentence connectives, like **no-* etc. is oversimplistic. If Watkins' claim is correct, it would be difficult to explain the cooccurrence of a clause particle like *no-* and a clitic like **kᵘo* (e. g., **no-kᵘe* > OIr. *noch*). Such a sequence would be inexplicably redundant. We have already noted that **i̯o/kᵘo* are to be distinguished from **no-*, **to-*, **so-*, **e-* on grounds of syntactic distribution. The distributional facts correlate neatly with certain taxonomic and semantic characteristics of these particles. Consider first an important and telling syntactic/semantic distinction between the Hittite forms *-ia* (< **io*) and *nu-* (< **no*). While both forms are commonly translated as 'and', they are clearly not synonymous in Anatolian. *nu-* serves to move the discourse forward. It is easily translated 'and then' and may in certain contexts more appropriately go untranslated in western European languages. It is commonplace for three or more *nu-* clauses to follow one after another.

4. i. ták-ku ERUM ^{URU}*HA-AT-TI IŠ-TU* KUR^{URU} Lu-ú-i-ya-az LÚ ^{URU}*HA-AT-TI* ku-iš-ki da-a-i-iz-zi na-an A-NA KUR ^{URU}*HA-AT-TI* ú-wa-te-iz-zi

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If any Hittite man steals a Hittite slave from the country of Luwiyas, and (then) brings him to the country of Hatti[4].

(Laws I 20)

ii. ták-ku DUMU.SAL-aš LÚ-ni ha-me-in-kán-za nu-uš-ši ku-ú-ša-ta be-da-iz-zi appí-iz-zi-na-at at-ta-aš an-na-aš hu-ul-la-an-zi na-an-kán LÚ-ni túh-ša-an-zi kuú-ša-ta-ma 2-*ŠU* šar-ni-in-kán-zi.

If a girl is betrothed to a man, and he pays the bride-price for her, (and) afterwards (her) father (and) mother violate it (i. e. the marriage contract), and (then) take her away from the man, they return the bride-price in double the amount.

(Laws I 29)

On the other hand, -*ia* generally connects two clauses in which the described actions (or states) are intimately related in time and space, or where the connected clauses refer to parallel notions (see Friedrich 1959: 116). Sequences of three or more clauses with the clitic -*ia* are at best rare. Consider examples (4.iii–4.iv) and note the difference between these and discourses of the type exemplified in (4.i and 4.ii).

4. iii. [(ták)]-ku ^{LÚ}DAM.GÁR ^{URU}*Ha-at-ti ku-iš-ki ku-en-zi I ME* MA.N[(A)] KÚ.BARBAR *pa-a-i*.

If any one kills a Hittite merchant, he pays one and a half pounds of silver and gives his farm buildings in security.

(Laws I 5)

iv. *ma-a-na-aš* SIG-*at*-[(ta-ri-ma)] *nu-uš-ši* 6 GÍN KÚ. BABBAR pu-a-i ^{LÚ}A.ZU-ia *ku-uš-sa-an a-pa-a-aš-pát* [(pa-a-i)]

When he_1 gets well, he_2 pays him_1 six half shekels of silver, and then he_2 pays the physician his fee.

(Laws I 10)

[4] All English translations are after Sturtevant, E. and G. Bechtel (1935), A Hittite Chrestomathy, Philadelphia.

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If we consider connectives like *nu-* and the clitic *-i̯a* to be coordinating conjunctions, (and it is questionable, despite the common translation as 'and', that *nu*, etc. should be considered coordinators at all), it is clear that these forms differ with respect to the scope and the nature of the coordination involved.

4.4. That *i̯a* marks some intimate relation between two clauses is a characteristic perfectly consistent with the morpho-syntactic classification of this particle in Hittite. As noted above *i̯a* always occurs in clause second position. Another particle falls consistently into that slot; it is *ma* 'but'. *-ma* like *-i̯a* is a conjunction (or, more properly, a sentence operator) which marks a biclausal interrelationship.

4.5. If we consider the development of **i̯o/kʷo* in the other early attested languages, we find histories which are not inconsistent with the use of *i̯a* in Hittite which we have described here. That is, **i̯o/kʷo* regularly marks, often in conjunction with other particles, some intimate relation between two clauses; the clause particles which are derived from **i̯o/kʷo* develop in two distinct ways: on the one hand, **kʷo* is reflected in markers of coordination like (Lat.) *-que* (Gk.) *-te*, and (Skt.) *-ca*, corresponding precisely to Hittite *-i̯a*; on the other, **i̯o/kʷo* develops as a marker of interclausal relations, serving as the root for relative words, and the base for many subordinating conjunctions, (2.). This dual development of connectors which indicate sentential interrelationship underscores the close functional relationship of coordination and subordination. Indeed, we see that the former indicates textual continuity while the latter serves to focus on some element common to two or more related sentences in a text.

5.1. Since a final or purpose clause represents just one type of subordinate which might have been announced through the use of these clitic particles, genuine examples of 'relative clauses of purpose' which occur in the early dialects would appear to reflect the archaic, undifferentiated use of **i̯o/kʷo* as a marker of an interrelationship between two clauses. As relative pronouns develop, probably through reanalysis of clitic, resumptive pronouns as inflectional endings attached to **i̯o/kʷo* (4.2), the perception of coreference between such a pronoun and its ante-

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cedent gradually fosters the restricted use of relative clauses to the kinds of adjectival contexts we generally associate with relativization.

5.2. Most finite purpose constructions in the ancient dialects are introduced by conjunctions which, as we have seen, are based on these same particles **i̯o* and **kʷo*. We must assume that these conjunctions develop through processes similar to those which produced the relative pronouns. A sequence of clitics, always beginning with one of these two particles, is reinterpreted as a single morpheme, and is isolated as the marker of subordination in a specifiable set of semantic contexts. In at least one notable case, a form otherwise associated with the relative pronoun paradigm (Skt. *yad*) comes to be stereotyped as a purpose conjunction as well. Here, we have considered only those markers associated with purpose. However, the implications of this development are, of course, more general. Conjunctions other than those discussed here show corresponding developments, and some of the forms we have discussed are themselves used in subordinate clauses not associated with purpose. However, where such functional duality does occur, differences in syntactic patterning serve as formal devices which distinguish the various clause types. We return to this point in a moment in (6).

5.3. Purpose clauses with conjunctions suggest the earlier occurrence of subordinate clauses marked by **i̯o/kʷo*, but without any resumptive pronoun. This is to be expected for purpose clauses, since a coreferential noun may, but does not necessarily occur in the main clause. The precursors of relative clauses of purpose and those of purpose constructions with a conjunction must have been contemporary. But, since coreference is not a characteristic of all purpose clauses, after the phonological and morphological changes which produced relative pronouns and purpose conjunctions, clauses with conjunctions would be more widely serviceable as general markers of purpose. (In conjunction with this claim, recall our earlier remarks concerning the probable development of restrictions on the use of relative clauses in general (5.1)).

5.4. This brings us to a final note about purpose conjunctions. We pointed out above that the IE. subordinating particles **i̯o*

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and **k^{w}o* were clitics. Yet, outside Anatolian, conjunctions are generally clause-initial forms. This is not, however, a problem for our analysis, since substantial evidence exists which establishes the early occurrence of such conjunctions in second position in their clause. As Watkins (1963: 18) points out, after Minard (1953), subordinating conjunctions commonly follow sentence particles in Vedic prose (e. g. *sa yadi*, *sa yatra*, etc.). See, for example, (2.i) and (2.ii) as well as (5.i) below.

5. i. idam pātram apāyi matsad yathā saumansaāya devam
this bowl has been drunk up in order that it may exhilarate the god to benevolence.
(IV 44, 16)

With the demise of the system of cliticization, with the phonological and morphological innovations which replace sequences of clitics with full words (specifically, subordinating conjunctions) and especially with the particularization of the interclausal relationships which these conjunctions come to define, we should not be surprised to see the movement of these forms to a position where a statement of interclausal relationship is most readily perceived—i. e., clause-initial position.

6.0. Let us return now to the differences in syntactic patterning which distinguish subordinate clauses of purpose from other types of subordinate clauses, which are introduced by any of the conjunctions we have dealt with above.

6.1. Besides its function as a marker of purpose, Vedic *yad* introduces temporal clauses, conditional clauses, and nominal complements. Hence, it may be translated 'when', 'if', or 'that'. In each such case the verb of the *yad*-clause may be, and usually is, in the indicative, and the clause itself is preposed with respect to its matrix. A clause of purpose introduced by *yad* is always postposed to its matrix, and the verb of that clause is regularly subjunctive. The optative occurs infrequently. Similarly, purpose or final clauses introduced by *yathā-* are always postposed to their matrix, and generally include a subjunctive verb. As in the case of *yad*-clauses of purpose, the optative is rare in corresponding *yathā*-constructions. *yathā-* also has the meaning 'as'. When *yathā*, meaning 'as', introduces a

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full clause, that clause is preposed to its matrix (which usually shows the correlative particles *eva* or *tathā*), and the verb of such a clause is often indicative.

6.2. In Latin, we find that *ut*, in addition to its use as a purpose subordinator, may indicate a time relationship, i. e. 'when', and later becomes an all-purpose subordinator in Latin, similar to *that* in English. In its time-adverbial function, the clause introduced by *ut* may either precede or follow the main verb of a sentence. In its use as a purpose subordinator, however, we find that the *ut*-clause seems always to follow the main verb. In addition, we note that in the time adverbial use of *ut* clauses, the verb in the *ut* clause is in the indicative mood, and in the tense appropriate to the time relationship being described. In the purpose use of *ut*, however, the verb in the *ut* clause is always subjunctive, and is present or imperfect, depending upon whether the tense of the main verb is primary or secondary, respectively.

The relative marker in Latin, as an indicator of purpose, also introduces a subordinate clause in the subjunctive, and this clause usually follows the clause containing the noun it modifies. Other types of relative clauses in Latin are not fixed as to word order (e. g., *qui fortis est, idem est fidens,* Cic., T. D. 2, 54), nor as to the sequence of moods. Thus, in determinative or generalizing relative clauses (cf. Woodcock 1959: 187ff.), the relative clause is regularly in the indicative, and even in some cases the casual use of a relative clause will contain an indicative verb, e. g., *res loquitur ipsa, judices, quae semper valet plurimum,* Cic., Mil. 53. We see, then, that the restrictions on clause order and sequence of moods that we found with purpose clauses introduced by *ut* seem to hold also for relative clauses of purpose.

6.3. In Greek, as mentioned above, we again find that the purpose conjunctions are related to the relative stem. We find also that each of these conjunctions function also to introduce adverbial clauses, and that there are certain restrictions on clause order and sequence of moods that hold in the former case, but not in the latter. Specifically, in their use as purpose conjunctions, we find that the clauses containing *hōs, hópōs, óphra, héōs* follow the main clause (cf. Meillet-Vendryes 1948: 587), and

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that it is usually the case that the subordinate clauses introduced by these conjunctions will be either in the subjunctive or optative, depending upon the tense of the main verb (for exceptions see Smyth 1963).

Only with *hina* do we find a minor deviation from the pattern described. In addition to its use as a purpose conjunction, hina survives in its use as a relative subordinator for 'where'. While the locative use of hina does not exhibit mood cooccurrence restrictions between the main and subordinate clauses, in all of the cases examined so far, the locative clause introduced by *hina* follows the main clause, as is also the case with the purpose use of *hina*.

In Greek, the relative marker also serves to introduce a subordinate clause of purpose. Relative clauses of purpose always follow the main clause, and they exhibit the general sequence of mood restrictions that hold for conjunction-introduced purpose clauses. These restrictions do not hold in general for other types of relative clauses.

Let us recapitulate the differences in syntactic patterning which distinguish subordinate clauses of purpose from other types of subordinate clauses, when any of the conjunctions we have dealt with in 3 serve a dual role.

6.4. Latin *ut*, Greek *héōs*, *óphra*, and *hópōs* and Skt. *yad* may introduce temporal (or modal) adverbial clauses, as well as purpose clauses. Gk. *hína* has a local usage, Skt. *yad* is found also in conditional constructions, and Skt. *yathā-* often means 'as'. In all these contexts the subordinate clause may or must precede the matrix clause, and may or must include an indicative verb. However, purpose clauses introduced by any of these conjunctions *must* follow the matrix clause, and invariably though not surprisingly, they always include a subjunctive or optative verb. We notice also that relative clauses, when functioning to express purpose, always follow the main clause and always show verbs in the subjunctive in Latin and in Homer, and in the subjunctive or optative in Vedic. The optative is rare in Vedic prose, however.

7.0. In discussing the matter of clause order for purpose in Greek, Meillet and Vendryes (1948: 651) attribute the consistent

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postposition of purpose clauses to an original situation in which the two clauses were simply juxtaposed. This is precisely the case in Hittite, where no special connecting words (be they conjunctions or pronouns) are used to mark final clauses.

The suggestion of Meillet and Vendryes, while probably valid as far as it goes, implies that no marker of interclausal relationship was present in the parent language. We would revise that suggestion. While no mark of subordination particular to the notion of purpose was present, we claim that the comparative evidence suggests that **i̯o/k*o* commonly (though probably not always) occurred in such contexts, with a resumptive pronoun where appropriate. Although Anatolian has restricted the use of **i̯o* (Hittite *-i̯a*) to contexts of simple conjunction, the relative clause of purpose, and the relative purpose conjunctions in Latin, Sanskrit and Greek reflect just one of the contexts in which the **-i̯o/k*o* particle functioned productively to mark a subordinate clause in the parent language.

8.0. It seems, then, that the early dialects indicate two means of expressing purpose in the parent language: nominalization and constructions marked by the pre-relative particles **i̯o* and **k*o*. In both cases we have suggested how these situations might have arisen, and in the latter instance we have proposed a development from pre-relative finite clauses of purpose to the attested dichotomies between relative clauses of purpose and conjunction-introduced purpose clauses. Moreover, we have shown how such a development accounts for the syntactic and semantic peculiarities of purpose clauses, and for the dichotomy of sentence connectives which develop from **i̯o/k*o* into coordinators on the one hand and subordinators on the other.

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Proto-Indo-European Vocalism and the Development of the Indo-European Declensional Models

1.0. In this article two new ideas are accepted concerning Proto-Indo-European (PIE.) inventory of phonemes:

1 – the reinterpreting by Th. Gamkrelidze and V. Ivanov of the three series of the Indo-European (IE.) stops (traditionally **b, *d, *g – *bh, *dh, *gh – *p, *t, *k*) as glottalized—voiced (aspirated)—voiceless (aspirated) (**(p'), *t', *k' – *b(h), *d(h), *g(h) – p*(h), *t(h), *k(h)*) (see rf. 8; 7);

2 – the reinterpreting by E. Pulleyblank of the primary opposition **e* : **o* as opposition by the rise of the tongue, i. e. **ə* : **a* (32; 13). Nevertheless we consider the first to be *ɛ—i. e. of the front series and of the middle rise.

1.1. The starting points of this article are:

a) PIE. phonology springs out of the state of syllabicity ("monovocalism") (cf. 20 : 15.5,6);

b) PIE. was a language of the fientive typological structure, that in traditional terms is still named "ergative"[1]. The ergative structure, however, is that typological model where the subject of the transitive action is treated otherwise than the subject of the intransitive one, while the latter is treated in the same way as the object of the transitive action (see 15 : 48)—cf. Georgian *mezobel-ma aašena saxl-i* "the neighbour built a house", the word *mezobel-* "neighbour" being in the ergative case opposed against the absolutive case of the word *saxl-* "house", and *mezobel-i mok'vda* "the neighbour died", *saxl-i šendeba* "the house is built". Research in the field of Indo-European shows, however, that the category of transitivity-intransitivity is secondary here. Thus A. Savčenko writes: In PIE. "vyraženie subjekta dejstvija érgativnym ili absoljutnym padežom zaviselo ne ot perexod-

[1] See rf. 25 with bibliography on p. 114–115.

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nosti ili neperexodnosti glagola, a ot glagol'noj formy dejstvija ili sostojanija" (34 : 90). In other words, PIE. was a structure, the components of which were destined to express the correlation between the acting and the inert participants of the process and not between subject and object as it takes place in languages of the ergative or of the nominative structure. Correspondingly all PIE. verbs were lexicalized according to processitivity-stativity and not according to transitivity-intransitivity. Substantives as well were divided into processive and inertive. Such a structure is usually called "active" (cf. 16 : 4), though the term "fientive" (cf. Lat. *fiens* and the semitologic tradition to use this term for the corresponding verbs) seems better suited, as it protects us from associations with the diathesis of activity-passivity. So one must speak not about the ergative and the absolutive, but about the fientive and the inertive forms (cf. 25 : 116[19]; 31 : 12);

c) the opposition */ε/ : */a/ from the very beginning was apophonic and appeared first in final, where it had developed from the corresponding allophones of the primary syllabeme: [*a*] came into being under the sense intonation of the fientive forms and contrasted with [ε], which was not connected with intonation;

d) for that period one must speak about sense intonation and not about pitch or accent. From the grammatical point of view pitch accent and accent are the same, since they have the same function of distinguishing separate morphophonemic elements (e. g. phonemes, as in Lith. *siųsiu* "I'll send" : *siúsiu* "I'll sew", or morphemes, as in Russ. *zámok* : *zamók*). In this respect accent (or pitch accent) is a phenomenon similar to the distinctive features of phonemes (e. g. aspiration, labiality, nasality, etc.) and its existence in a language is no more necessary than any of them. As for intonation, its presence in a system cannot be secondary because it always exists and always distinguishes whole units (phrases, sentences, exclamations). This distinguishing of the units carrying semantic meaning burdens intonation with the function of grammatical semantics. Therefore no language exists without intonation; intonation is obligatory for

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every language. On the other hand, some semantic-grammatical functions (e. g. the distinguishing of forms) may be given only to such a phonetic (or morphological) sign, which has already existed in the system; new signs never appear and are never created to express new categories. As J. Kuryłowicz remarks, "An empirical fact to be frequently observed in linguistic diachrony is the eviction or replacement of a morph by a new morph only in the former's primary or secondary function. It is not the morpheme as such which is being renewed but only the morph corresponding to a part of its functional range" (19 : 14).

Thus accent (or pitch accent) cannot come into being, for example, due to the necessity to differentiate cases, not having had some other functions earlier; if accent (or pitch) does not exist, it cannot receive any function = come into being due to this function; functions do not produce signs for themselves. On the contrary, sense intonation does always exist in the language and always with its semantical function. Accent arises due to phonetic reasons and cannot receive a grammatical burdening until the sense intonation loses it;

e) the allophones [*a*], [ε] also existed in the neighbourhood of H_2, H_3 and H_1,*Y*, but in all instances the allophones were phonemicized only when the accentuational model appeared;

f) because of the complementary distribution */*C[CΛ,ΛC]*/ in one-consonant roots, PIE. might have been able to have the vocalic onset, where the opposition */ε/ : */*a*/ was never represented while being neutralized in */*A*/; we have this primitive *a*- in *ago*, ἄγω, axis, ἄξων, etc. (although in many cases *a*- derives from *H_2a- as in Hitt. *ḫarki-*, Gk. ἀργής)—cf. the neutralizing of *e*- and *a*- in Lithuanian *ẽžeras* and Gk. 'Αχέρων (M. Vasmer). As for initial IE. *e*-, it must have arisen from *H_1ε-;

g) there existed three laryngeals in PIE.: *H_1, *H_2, *H_3; the primitive syllabeme had a timbre *[ε] in the neighbourhood of H_1, **Y*, and the timbre *[*a*] in the neighbourhood of *H_2, *H_3. When the phonemes' oppositon */ε/:*/*a*/ came into being, all **a* that had developed from combinations with the laryngeals *H_2, *H_3, as well as all initial *a*- were not apophonic; in the other instances **a* was apophonic:

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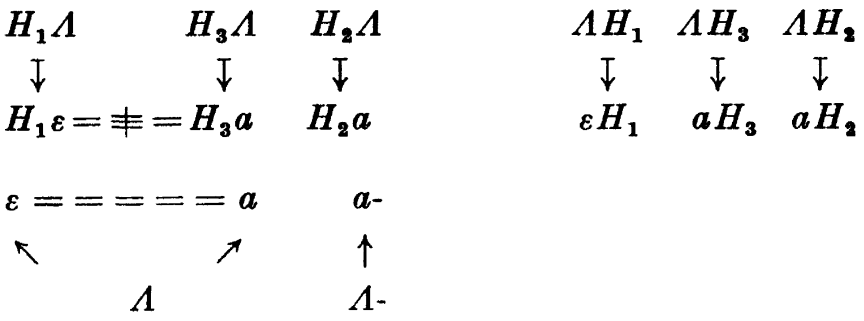

(= apophonic relation; ≠ non-apophonic relation)

Later in Anatolian $*H_1$, $*H_2$, $*H_3 > \underline{h}$, and as a rule $*H_1$- $> \emptyset$-; at the same time in Balto-Germanic (for the term cf. 23: § 186–188):

$*\varepsilon > e$; $*a$- $\rightarrow a$-; $*$-a-, $*$-$a \rightarrow a$;

$$*H_1\varepsilon > *H_1e > e;\ *\varepsilon H_1 > *eH_1 > \bar{e}$$
$$*H_1a > *H_2a > a;\ *aH_2 \rightarrow *aH_2 > \bar{a}$$
$$*H_3a \rightarrow *H_3a > a;\ *aH_3 \rightarrow *aH_3 > \bar{o}$$

(it is lengthening when the development of a into $\bar{o}$ is typologically natural), while in the other languages:

$*\varepsilon > e$; $*a$- $\rightarrow a$-; $*$-a-, $*$-$a > o$;

$$*H_1\varepsilon > *H_1e > e;\ *\varepsilon H_1 > *eH_1 > \bar{e}$$
$$*H_2a > *H_2\mathring{a} > a;\ *aH_2 > *\mathring{a}H_2 > \bar{a}$$
$$*H_3a > *H_3\mathring{a} > o;\ *aH_3 > *\mathring{a}H_3 > \bar{o}$$

(H_2, as well as uvulars $\geq$ gutturals (36 : 96–98f.), hinders a turning into $\check{o}$, H_3 on the contrary props it up).

Only in later epochs $*a > o$, $*\bar{o} > \bar{a}$ in Slavo-Baltic, and $*\bar{a} > \bar{o}$ in Germanic.

Thus we have in Hittite *ešmi* ($*H_1$-), *ḫant-*, *palḫi-* ($*H_2$), *ḫaašdueir*, *laḫu-* ($*H_3$) and Lith. *esmì*, Lat. *ante*, *plānus* Gk. ὄζος, λούω — in this way Hittite, Baltic[2] and Germanic a reflects the original IE. $*a$, older than the difference between a and o. But in Baltic there existed long $*/\bar{a}/$, $*/\bar{o}/$ and no short $*/o/$—cf.

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[2] Cf. also the opinion of W. R. Schmalstieg about the original character of Baltic a (rf 35).

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Pruss. *poieiti*, *poūton*	—	Aeol. πώνω, Lat. *pōtus*,
Latv. *māte*	—	Lat. *māter*, but
Lith. *nãmas*	—	Lat. *domus*,
Lith. *ašìs*	—	Lat. *axis*.

The same must have been in Germanic, though Balto-Germanic **ā* turned into **ō* here: ***mātēr* > **mōtēr* > *muoter* (in German just as in Lith. *mótė* (< Balt. **mātēr*), which gives later *mûotę* in Samogithian). So the attempt of O. Szemerényi fails to prove the existence of the original Baltic **o* by the oldness of Baltic */ā/ : */ō/* (38 : 84).

Slavic must have emerged as a later development of the peripheral Baltic dialects (cf. 23: § 183–185) and thus its *o* cannot support any *o* of other languages as if deriving from the hypothetic IE. **o*, i.e. in this respect Sl. *domъ* ≠ Lat. *domus* so soon as Sl. *osь* ≠ Lat. *axis*. There is no reason to treat Sl. *o* as an archaism and Sl. **ō* > *ā* as an innovation; it is more believable that both **ō* > *ā* (γι – γνώ – σκω: *zna-ti*) and **a* > *o* (Hitt. *watar* : *voda*) are Slavic innovations. This is quite obvious just in such cases as *osь*, where *o-* corresponds to the primitive IE. *a-* (*axis*, ἄξων).

1.2. To finish this short observation it must be added that the stage of syllabicity hardly indicates Proto-Indo-European and is possibly a reconstruction of something preceding PIE. ("Nostratic"?). As for the stage of bivocalism **ε—*a*, it seems to be something common to PIE. and to some other "parent languages". Only the stage with **e*, **a*, **o* may be considered as really Indo-European, though it was by no means a common IE. stage, since **o* was being represented only in Italo-Celtic and Balkanian (Graeco-Armeno-Balkanian) (in Slavic—as a later Slavo-Baltic innovation). But since the previous **ε—*a* stage was not pure Indo-European, it may be said that Common IE. has never existed at all[3].

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[3] Cf. the author's views on the development of "parent languages" in IF. 82 (1977) and LP. 21 (1978).

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2.1. So, as it is asserted above, the development of IE. vocalism was connected with the appearance of the primitive opposition */ε/ : */a/ from the very beginning manifested in primitive apophony.

The problem of the origin of the IE. apophony (tradit.) **e*–*o** has found no solution acceptable to everyone. There is a number of hypotheses most of which derive it from a pure phonetic alternation without functional meaning and unconnected with the morphological oppositions. The most popular explanation is that of J. Kuryłowicz, who states that this alternation arose from the syncretism of the reduced **e*, **o* before resonants where **o* is generalized in the unreduced positions and later—in all the stems in the same grammatical form (18 : 38–40). But this opinion conjectures the opposition */e/ : */o/ as having already existed in PIE., and therefore cannot be accepted by those adhering to the syllabicity of the initial system. A quite new hypothesis has been proposed by N. Andreev, who derives the apophony **e*–**o* from the different timbre of the homobasic stems with the determinatives **X*^{w}, **W* and **X*^{y}, **Y*: other determinatives were drawn into the copulated sense oppositions and timbre became generalized as a distinctive feature among the corresponding designata (1 : 5). In this way the semantic difference becomes the reason for the phonemicizing of the timbre opposition. But it might have happened only in the case of neutralization of the corresponding determinatives, i. e. by their merging into one sound. Every new phoneme is in fact the result of such a system changing where two members of the system coincide (cf. 17 : 104f.). If determintives maintain their differentiation, the phonemicizing of the timbre oppostion becomes senseless. In this article one more attempt is made to connect the emergence of apophony with the semantic factor, namely—with the grammatical meaning of forms. In these speculations attention is drawn primarily to the peculiarities of the prenominative structure of PIE.

2.2. Though the sigmatic formant of the nominative/genitive < fientive is characersitic of all IE. area, such fact as the absence of this formant in the demonstrative pronoun **sa* (Gk. ὁ Goth. *sa*), which is undoubtedly of fientive origin (cf. its sup-

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pletivism—acc. Gk. τόν, Goth. *þana*, as well as the single stem in neuter—Gk. τό, Goth. *þata*), makes us suppose that the sigmatic formant is not a primary marker of fientive. On the other hand, it must be mentioned that the abstract substantives of the *o*-class are characteristic of the toned base (cf. Gk. χρόνος "time", Skt. *váraḥ* "choice"), while the agent nouns have the toned thematic vowel (Gk. τομός "cutting", Skt. *varáḥ* "bridegroom"); neuter abstracts are shown to have the toned base (Gk. γένος, Skt. *jánaḥ* "kin"), while the processive nouns in **-ti* are characteristic of the toned suffix (Lith. *mirtìs*, Skt. *mṛ́tiḥ*, Lat. *mors* "death", "dying" < **mṛ́tis*, the atonic base reduced). As it has been demonstrated (3 : 53–54, 61), neuter substantives of the *i*-, *u*-classes historically belong to the barytonic paradigm, while substantives, which are not neuter, belong to the oxytonic paradigm. The only conclusion to be drawn is that *such shifts of tone reflect nothing else but the initial differentiation of the fientive from the inertive forms.* This agrees well with the fact of the two types of accentual paradigms, oxytonic and barytonic, the original destination of which otherwise seems to be incomprehensible. So at some stage of the preflexional epoch all inert substantives must have been barytonic, fientive substantives having been barytonic in the inertive case and oxytonic in the fientive case. Now it becomes clear that the mobile tone paradigm reflects the original shifts of tone in the initial paradigm of the fientive nouns, while the barytonic paradigm is based on the base-toned paradigm of the inert nouns. The existence of the mobile (> oxytonic) paradigm in the inert (noncommon gender) nouns (e.g. Lith. gen. *medaũs*) is a result of later levelling and contamination of the both types (see 3 : ibid.; 23 : § 148, 152).

2.3. One fact demands especial attention, i. e. the connection between tone and apophony. In IE. not only is the atonic base usually reduced, but the full grade of apophony in the suffix may be connected with the presence of tone there. It is obvious in *u*- and *i*-stems where the oxytonic fientive nouns (marked by V. Mažiulis as u_1, i_1 (23 : 262–263)) are charactertistic of the full grade of apophony, and where the barytonic inert

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nouns (marked by him as u_2, i_2[4]) are characteristic of the reduced grade of it—cf. Skt. gen. u_1 *śatro-ḥ*, gen. i_1 *agne-ḥ*, gen. u_2 *madhu-*(+*na-ḥ*), gen. i_2 *vāri-*(+*na-ḥ*), acc. u_1 *śatru-m*, acc. i_1 *agni-m*, acc. u_2 *madhu*, acc. i_2 *vāri*. Here we see that the suffixal vocalism in the accusative forms[5] of the fientive noun entirely corresponds to that in all forms of the inert noun in accordance with the origin of the accusative and the genitive cases[6]. The same picture may be simply reconstructed[7] for Balt. gen. u_1 **sūnãus* < fient. u_1 ***sunáw(s)*; gen. i_1 **ugnẽis* < fient. i_1 ***ugnéy(s)*; gen. u_2 **mèdus* < inert. u_2 ***médu*; gen. i_2 **àkis* < inert. i_2 ***áki*; acc. u_1 **sűnun* < inert. u_1 ***sűnu*; acc. i_1 **ùgnin* < inert. i_1 ***úgni*; acc. u_2 **mèdun* < inert. u_2 ***médu*; acc. i_2 **àkin* < inert. i_2 ***áki*.

Did the apophony (traditionally) **ew/ow*—**u*, **ey/oy*—**i* mark fientivity-inertness, the same must have been the functions of this apophony (**e/o*—*Ø*) in the non-sonantic stems. Further will be shown how this primitive quantitative apophony have developed from the primary qualitative apophony carrying the same functions.

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[4] According to this pattern the author introduces signs o_1, C_1 for the common ("animate") gender stems, and $o_2, \bar{a}_2, C_2$ for the non-common ("inanimate") gender stems (cf. rf 26: 334_{20}f.).

[5] The accusative forms derive from the inertive case and the genitive forms of the fientive nouns derive from the fientive case. The inertive nouns have their genitive rebuilt according to the genitive form of the fientive nouns (cf. 3.3 in this article).

[6] In accordance with 23: § 160, 164.

[7] The original meaning of this root may be older than 1E.—many thanks to Mr. G. Carr-Harris, kartvelologist and antiquarian, who has drawn my attention to the root **di-* in Kartvelian as possibly connected with the word "God" (I think that the reduplication of this root has produced **did(i)-* "big", "large" in Kartvelian and also in Baltic)—cp. e. g. Georgian *diaγ*! "yes", typologically the same model as Engl. *by God*! Arab. *bi-llāh*! "by God", "trully" (according to A. Shanidze, The Fundamentals of the Georgian Grammar. I, Morphology. Tbilisi 1973, § 651 [in Georgian], *diaγ* < **dia γmerto*! "Mother-God!", though there exists the form with the preserved *-a* after *γ*: *diaγac*, *-c* being enclitic; thus the final *-γ* reveals itself as usual particle *-γa* "only", emph. "ever"); if so, Baltic **dainā* "folk-song", Av. *daēnā* "religion" come from the word that meant worship hymns, orakles—cf. Lat. *divinum* with the same suffix.

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2.4. So we assume that at some stage of PIE. the sense intonation distinguished the final syllabeme in the fientive case. Under the constant influence of the sense intonation an allophone diverged out of this syllabeme which contrasted with intonationally unmarked allophone in the degree of aperture. We consider the first to be *[*a*] and the other *[ε] notwithstanding the fact that the majority of hypotheses connect **a* (tradit. **o*) with the atonic position (cf. 10 : 156). In fact such a connection is based on separate evidence as Gr. νέος or pairs of the type δαίμων, δαίμονος : ποιμήν, ποιμένος which are formations of different epochs and structure. On the other hand, in later times the original atonic **ε* (tradit. **e*) was either eliminated at the end of the stem (see further 2.6) or (in a still later period) was ousted by the thematic **a* (tradit. **o*) (further 3.3). These processes have changed the original picture. Accordingly we consider such explanations of the instances like Skt. *gáya-* (not *jáya-*!) or Gk. ποδός, which allude to analogy, the later generalization, etc., as being merely ad hoc (cf. 5 : 111–112, 123). Gk. ποδός or φόρος are no less rare than νέος.

Thus at some early stage there existed pairs:

fient. **thʌmʌ̲* "secans", "cutting" : inert. **thʌmʌ* "secantem", "cut"

fient. **phʌt'ʌ̲* "pes", "leg" : inert. **phʌt'ʌ* "pedem", "foot"

(intonationally marked syllabeme is here underlined).

Marked and unmarked syllabemes manifest themselves in allophones *[*a*] and *[ε]:

fient. **thʌma̲* : inert. **thʌmε*
fient. **phʌt'a̲* : inert. **phʌt'ε*

Since the phonological oppositions of vowels did not exist in that period, there were no obstacles for the primitive vowel harmony, the timbre of the final syllable being able to extend on to the root (if there were no $*H_1$ or *Y there):

fient. **thama̲* : inert. **thεmε*
fient. **phat'a̲* : inert. **phεt'ε*

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2.5. With the quality of vowels keeping their reciprocal differentiation, the both allophones might appear in the same intonational position. Therefore the difference between them gained a new motivation perceiving the first as a stressed [*á*] and the second as a neutral unstressed [ε]. In this way intonation lost its grammatical function and the allophones became phonemicized in the primary vocalic opposition */*a*/ : */ε/. The accentuational model having appeared, the unstressed ending of the inertive form induced the inner stress there:

fient. **thamá* : inert. **thémε*
fient. **phat'á* : inert. **phétε*

(cf. Gk. τομός : τόμος, gen. ποδός : nom.-acc. πέδον).

The marking of the fientive by the ultima opposition */*a*/ : */ε/ meant the appearance of vowel alternation which thus from the very emergence of the vocalic phonemes was not automatic and had its functional burdening, i. e. was apophonic. The alternation **a*—*ε inside the root had not such burdening, since apophony was first of all conditioned in the final vowels, which were independent of the environment, but had no grammatical function within and was not possible beside **H*$_1$, **Y*. Therefore the levelling of the root timbre might take place in the corresponding fientive and inertive forms:

fient. **thamá* : inert. **thámε* || fient. **thεmá* : inert. **thémε*
fient. **phat'á* : inert. **phát'ε* || fient. **phεt'á* : inert. **phétε*

2.6. The age of pleophony ends together with the appearance of accent and reduction. The epoch of J. Kuryłowicz's roots begins with vowels weakened and eliminated in the atonic position (cf. 17 : 122–125), the epoch of E. Benveniste's states of stems and of R. Anttila's "jump rule" (2 : 4.6f.). At the same time the class of sonants comes into being with their syllabic-vocalized and consonantal variants, and thus sonantic stems' paradigms are formed. Their development is connected first and foremost with the fate of the final *-ε of the inertive. Since the stressed **-a* from the very beginning was the marked member in the opposition **-a* : *-ε of the fientive and the inertive forms, the unfunctional unstressed **-ε* was able to be apocopated due to

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reduction, and so the final **-a* became (as the marker of fientive) opposed both to **-ε* and to **-Ø* of the fientive forms:

fn. **thamá* : in. **thámε* ‖ **tham*; fn. **thεmá* : in. **théme* ‖ **thεm*
fn. **phat'á* : in. **phát'ε* ‖ **phat'*; fn. **phεt'á* : in. **phét'ε* ‖ **phεt'*

Here we have the origin of the quantitative vowel gradation as well as (in germ) the foundations of the future thematic (with the thematic vowels **-a*, **-ε*) and athematic stem models.

2.7. To show the development of the sonantic stems, the model **t'ΛyΛ*[7] (root) + **wΛ* (determinative) appears to be illustrative enough. At the stage of syllabicity there must have existed the opposition: fient. [**t'εyεwa̯*] "God of heaven": inert. [**t'εyεwε*] "heaven", "sky". The generalizing of the timbre **[a]* onto the root in this concrete instance was impossible because of the radical **y*. After the phonemicizing of **[ε]*, **[a]* and the emergence of stress, the first vowel to be syncopated was the one immediately preceding or following the stressed syllable; then the reduction of the unstressed syllable took place in the fientive form:

fient. (***t'εywá* >) **t'iwá* : inert. **t'éywε*.

As historically testified (though irregular in aspect of Benveniste's binomials), the stem **t'εywá-* (Pruss. *deiwas*, Lith. *dievas*, Skt. *devá̍h*) seems to be the result not of the attachment of the hypothetic thematic vocal formant, but of the levelling of the paradigm on the basis of the inertive form.

Thus for the period in view the following rule may be conjectured: **εSε* > **S̥* in unstressed position, in our case— **εyε* > **i*. According to the reduced model **phét'ε* > **phεt'*, however, another form of inertive was possible, which came into being through the apocopation of **-ε* and the later reduction of the unstressed **-εw* : **t'éyεwε* > **t'éyu*, i. e. in accordance with the above mentioned rule. The pattern **t'éyu* is historically testified in the inertive **t'éru* Russ. *derevo*, Gk. δόρυ. The single fientive form contrasting with this pattern might be only **t'yεw* < ***t'εyéw*, since inert. **t'éyu* is only a reduced equivalent of ***t'éyεw*, fient. **t'iwá* already corresponding with the other inertive form **t'éywε*.

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It is clear that the loss of the fientive marker *-*a* in fient. **t'yεw* might have caused the transference of *a*-vocalism into the stem: **t'yaw*. In this way two facultative variants have appeared, the full grade of vocalism **εw/aw* becoming the marker of fientive in u_1-stems.

Other sonantic stems' paradigms must have arisen in the same way. For all instances the formulas will be:

fient. $*C_1\varepsilon C_2S\acute{a}(*C_1aC_2S\acute{a})$: inert. $*C_1\acute{\varepsilon}C_2S\varepsilon$
x : inert. $*C_1\varepsilon C_2\overset{\circ}{S}$
x = fient. $*C_1C_2\varepsilon S \parallel *C_1C_2aS$.

In later epochs these models are complicated with extensional derivations, infixal ones, etc. (cf. 3: Ch. IV, VI).

So the quantitative alternation of the full and zero grades is of combinatorial origin. This alternation receives functional burdening through the opposing of stressed morphemes to unstressed ones in the corresponding fientive and inertive forms—cf. such apparent relics as Skt. gen. *agne-ḥ* : acc. *agni-m*.

2.8. As for the grade of lengthening, the most popular opinion is that its origin is much older than the dissapearance of laryngeals (cf. 17 : 92–97; 18 : 142–165; 19:198; 20:2.3a, 15.4; 36: 90–92, 95–96). The most realistic is to suppose the primitive lengthening first in monosyllabic fientive forms[8] of the **t'yεw* > **t'yēw* type, where it compensated the impossibility of opposing the stressed marker of the fientive against the unstressed stem. That primitive length was not phonological; its phonemicizing might have happened only together with the emergence of length caused by the disappearing laryngeals[9]. But as the non-laryngeal lengthening is present not only in the sonantic stems, its phonemicizing (ipso facto—the vanishing of laryngeals), as will be shown further, must have taken place only in a much later epoch, when the sigmatic marker of the fientive case had already come into being (otherwise we would not have had length

[8] The first to connect the appearance of length with monosyllabic forms was M. Leumann (IF. 61, 1952, 1–16); O. Szemerényi has developed this idea, but in a way opposite to our view (rf. 39: 108–111).

[9] It is Andreev who also connects the appearance of the phonologized non-laryngeal length with the period of vanishing laryngeals (rf. 1).

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in Dor. πώς, Lat. *pēs*, Ved. *pāt*). Consequently the length in Gk. *Zεῦ*, Lat. *Iū-* (**Iou-* < **dyēu-* according to Benveniste) *-piter*, Skt. *dyaúḥ* has nothing in common with laryngeals (i. e. not as assumed by Benveniste 3:59–60).

2.9. So the following epoch must have been characterized by the appearance of the additional marking of the fientive by the demonstrative pronoun fient. **sa* (Skt. *sa*, Gk. art. *ὁ*) (its inertive being **thε* ≥ **tha*—Skt. neutr. *tat*, acc. masc. *tam*, Gk. τό, τόν) in the function of the enclitic article. The development of the enclitic article out of the pronominal form has enough parallels in the older as well as in the newer stages of IE. (13: 239–241; 33). Such article determining and identifying the fientive forms of different structure (as **phat'á* and **t'yēw*) was postponed at the end of the fientive case form and gradually coalesced with the latter. Later it became the single marker of the fientive (13:237), while **-a* had been deprived of morphological burdening.

**sa* might have played the role of determinant in much earlier epochs in parallel with the ancient (judging by traces in neighbouring linguisic families—see 31:15–18) enclitic article (indefinite pronoun) **mε* which marked the inertive case of the fientive nouns as opposed to the inertive form of the inert nouns[10]. The merging of the stem and this **mε*, further turned into the accusative formant, must have taken place contemporaneously with the merging of **sa* and the stem.

There are no obstacles to the conviction that the necessity for the additional marking of fientive was caused by the destruction of the primary model fient. **-a* : inert. **-ε* ‖ *-Ø* in the sonantic stems' paradigms and by the appearance of fientives with the full grade of vocalism in the sonantic determinative. While the forms with final **-s* (**t'yēws* ‖ **t'yāws*) struck root and thus the functions of the final **-a* became weaker in the other stems, the sonantic stems' pattern with **-s* became fixed in the other stems as well. First and foremost it was possible to supplement the consonantic inertives **phεt'*, **phat'* with the corresponding

[10] For typological parallels for the two kinds of inertive see rf. 15: 114, 115, 241.

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fientive correlates **phēt's, *phāt's* whose length was determined in accordance with the monosyllabic sonantic stems. Such innovations existed side by side with the new fientives **phɛt'ás, *phat'ás* having their *-s* added to the ancient fientive stem.

In Balto-Germanic the non-laryngeal lengthening of PIE. **a* resulted in **ō* (cf. Lith. *akmuõ, rẽmuo* < **-mōn* : Gk. ἄκμων; Sax. *hano* < **-ōn*; Lith. *vil̃ko*, Latv. *vìlka* < baryt. **-ō* [23:§12]: Lat. *lupō*). The reason appears to be the timbre of the lengthened **ā* in Balto-Germanic. This timbre, as well as that of **ā* < **aH₂*, was obviously close to *ō*: [å]. Such articulation must be regarded as pharingalized (and not labialized), because the pharingalized articulation is reconstructed not only for Baltic long **å*, but also for the short **å*[11].

It must be assumed that in case of the laryngeal lengthening the timbre of **H₂* hindered the emergence of *ō* and thus we have **å*. By the non-laryngeal lenghtening, however, there were no obstacles for the development **å* ≷ **å* > **ō*.

Later the timbre *å* was generalized on **a-* as well (this is the reason why in Slavo-Baltic this primary **a* has turned into *o* in all positions).

Contemporaneously with these processes of Balto-Germanic the transition of **-a-, *-a* into *o* (e. g. **phat'ás* > **podós, *phāt's* > **pōds*) took place in all the other non-Anatolian languages; only afterwards did the laryngeals begin to disappear and the length—to be phonologized (otherwise we should have **pāds* and not **pōds*, Dor. πώς).

2.10. The epoch of vanishing laryngeals was also that of the intensive disintegration of the main IE. areas into the known groups, such as Baltic, Germanic on the one hand, Greek, Balkanian, Armenian on the other, etc. It is obviously the time when the pitch accent comes into being (cf. 11:215–216). The origin of the pitch accent seems to be connected with the history of diphthongs and not with the traces of laryngeals. The rise of this phenomenon was caused by similar factors everywhere, though the concrete development in separate IE. branches might have been various. According to A. Girdenis[12], in Baltic the accute

[11] See A. Girdenis' review in Bl. 13–1 (1977) 300–303.

[12] Personal letter of the 4 of May 1978.

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pitch appeared first in long diphthongs, while the circumflex—in short ones. Later they were both generalized on monophthongs in the corresponding positions, where this generalizing was conditioned by the same grammatical form, e. g. **sūnãus → *galvãs*. I accept the idea of A. Girdenis about such generalizing utterly, but as to the development of the pitch accent in diphthongs, this possibility became clear to me independently and I give it quite a different explanation. The primitive pitch accent must have arisen together with the primitive lengthening in the monosyllabic fientive forms of the sonantic stems' type **t'yēw*. As the lengthening was only compensation of the lost additional marking of fienvity by stress on the final **-a*[13], the character of such final stress might have manifested itself on the final sonant thus producing the primitive circumflex: **t'yε̃u*, *ε̃u* being long syllable equal to *ēw*. Now I use the idea of A. Girdenis and treat the generalizing of this circumflex in monophthongs in such a way that at the stage of the sigmatic marker the monosyllabic type fient. **phēt's* must have been formed as a correlate to inert. **phεt'* in entire correspondence to the sonantic type, i.e. with lengthening and circumflex jointly: **phε̃t's*. Together with the disappearing of laryngeals and the phonologizing of length, such circumflex became systemic. As contrasting to it, in all the instances of the laryngeal length the high pitch developed. In later epochs the further generalizing of the pich accent took place in the corresponding monophthongs in the course of the development of the new grammatical forms.

3.0. Consequently, the sigmatic formant of nominative and genitive singular must have derived only from the enclitic article **sa* (cf. 13:237; 4:§ 3). There are no grounds to reconstruct the special nominative inflection (tradit.) **-es* in consonantic declensional model as well as to assume the later reduction of such inflection (cf. 17:92–93). The vowel reduced before *-s*, whether it really existed or not, may have been nothing else but the ancient stem ending. Thus as far back as 1956 J. Kuryłowicz tended to recognize thematic vowel as a manifestation of

[13] Exactly speaking, this final **-a* has never existed in the forms of the type in view and the loss must be understood systemically—cf. above 2.7 about the correlative origin of the type **t'yēw*.

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the stem ending (18:97, 131). It is a pity that he retracted his opinion in 1964 (19:117). The varying of the thematic *ε (tradit. *e) or *a (*o) in the endings of nominative and genitive is connected with the later fate of those stem endings and does not point to the hypothetic inflections (tradit.) *-es/*-os. The elucidation of this fate is equal to solving the problem of the origin of the thematic declensional pattern.

3.1. The appearance of the equivalent forms

fient. *phat'ás ‖ *phɛt'ás ‖ *phāt'sa ‖ *phēt'sa : inert. *phɛt' meant the conjoining of the enclitic article *sa to the two variants of the same stem, i. e. the facultative status of the final *-a as of the consonantic stem's ending in sandhi. The same sandhi phenomenon seems to be the reason of the alternation -Ca/-C in the forms of Hittite and Aryan dative-locatives as Hitt. *nepiša ‖ nepiš* "to the sky" (side by side with the regular *nepiši*), *keššar-(ta)* "in (thy) hand", *dagan* "to the ground", Skt. *s(ú)var* "in, to the sun", *kám* "in water", Avest. *dąm* "in house" (cf. 29).

Of whatever origin it may be, the alternation -Ca-/-C is also testified in neighbouring linguistic families. In all instances it might have arisen because of the restoring of the final open syllable in sandhi, the word structure being *CVCVCV* and *CVCVC*—in sandhi the latter model manifests itself as *CVCVCa*, with *-a* as an equivalent for any *-V*. Such regularity is not excluded in systems with the primitive word structure. Thus the original structure *CəCəC*[ə] is reconstructed for Proto-Kartvelian (9:370 in Georgian, 470–471 in Russian) where the alternation *-Ca/-C* is also represented[14]. Without any doubt this

[14] In all Kartvelian languages we find *a*-vocalized and non-vocalized variants of the stem ending before case inflection—cf. Svan. translat-adit-erg. *txuma-d* (poet.) ‖ *txum-d* "up to the peak", dat .-loc. *t'uba-s* (poet.) ‖ *t'ub-s* "to, in the stream", Zan. dat. *gur-s* "to heart", but adv. *xšira-s* "often", translat.-adit. *čkimo-t* "to me" : Georg. *čem-da* "to me", *kalaka-d* "to town" : *Ierusale(i)m-d* "to Jerusalem", dat.-loc. *adgil-s(a)* "to place" : adv. *adgila-s* "in place". Such vocalization makes clear the nature of the so-called "emphatic" *-a* appearing after the case inflectional as a result of the rule of combinability between stem and inflectional morphemes—cp. *kalaka-d, adgila-s : čem-da, adgil-sa* (rf. 28).

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alternation is testified in Afro-Asiatic (6:45–49) where on the basis of the *a*-vocalized variant of the inertive bare stem the marker of feminine has developed (see 28; 29).

As it may be seen, the alternation *-Ca/-C* was able to appear only in sandhi (e. g. by conjoining the future inflectional element). In this way the manifestation of Benveniste's binomials was conditioned between stem final and case ending: **phat'ás* : **phāt'sa*; **ghɛrás* (Lat. *ferus*) : **ghērsa* (Gk. θήρ). It may be assumed that such ratio was independent of the binomial relations with the root vocalism, whether they existed or not —cf. **t'iwás* : **t'yēwsa* and **phat'ás* : **phāt'sa*.

3.2. In the fientive epoch the enclitic article **-sa* was not added to heteroclites. We consider them to be derivatives of the archaic inert nouns with determinatives **-w*, **-s*, **-th*. The additional **-r*, **-n*, conjoined to these stems later, were derivational means to form secondary inert (Hitt. *watar*) and fientive (gen. *wetena-*, cf. the usage of *-n-t* in Hittite to transfer inert nouns into the fientivse class—E. Laroche and V. Ivanov, 13:52–54) lexemes out of the primary inert ones. Since agent nouns in **-thēr* (tradit. **-tēr*) originate from the corresponding adjectives connected with the heteroclite type (3: Ch. VI, 2), we take risk to illustrate our idea by comparing 1) Hitt. *-pat* (= **-pH$_3$t*?—the zero grade because of the enclitic position), Lith., Latv. *pat* (the same? the full grade as in *pats*?) "just", "also", "self" ← adv. "willing"(?), 2) Gk., Lat., Skt. *patĕr*, *pitár* (the zero grade, the full grade in Lat. *potis*, Gk. πόσις, Skt. *pátiḥ*) "father" ← adj. "willing". We cannot accept O. Szemerényi's view that Hitt. *-pat*, as being identical with prep. *poti*, has nothing in common with Lith. *pat* used as a calque of Russian constructions (37:357). The Russian influence is young, while *pat* is usual also in Latvian in the same meaning which is much closer to Hittite than to Russian (in Russian—*samyj* and *že*). Heteroclisies as well as suppletivism (cf. **sa/*tha* was archaic means to differentiate fientivity-inertness before the sigmatic fientive marker emerged.

3.3. The very emergence of patterns with the thematic vowel and without it (**t'iwás* ‖ **t'yēws(a)*, **phat'ás* ‖ **phāt's(a)*—*(a)*

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disappears according to the thematic pattern[15]) does not mean yet the rise of the thematic and athematic declension[16]. To be so the thematic vowel must be specialized for all case forms in a group of words, while the other words will be characterized by the absence of this vowel in all cases. Such process is connected with the changing of the whole linguistic structure, the fientive structure being nominativized and decaying.

At some early PIE. stage the fientive was first marked by sense intonation and later by vowel gradation. This case was possible only for the fientive nouns. The inertive form was common both to them and to the inert nouns where it was the single indeclined form. At the later stage of this linguistic structure the fientive case is marked by the enclitic article **sa*, the inertive case of the fientive nouns already having the enclitic **mɛ*:

fient. **t'iwás* ‖ **t'yēws* "Deus"	: inert.	**t'éywɛm* ‖ **t'éyum* "Deum", "sky"
—	: inert.	**t'éru* "tree"
fient. **phaṭ'ás* ‖ **phɛṭ'ás* ‖ **phāt's* ‖ **phēt's* "pes"	: inert.	**phét'ɛm* ‖ **phɛṭ'm̥* "pedem", "foot" → "step" → "ground", "place"

At all the stages of the fientive structure the stem ending **-a(-)* was able to appear only in the fientive forms. In the course of the nominativizing of this previous structure the former fientive case form began to serve also as nominative and genitive. Meanwhile the former inertive form (having enclitic **m(ɛ)*[17] in the instance of the fientive nouns) began to be used also as accusative, nominative and dative-locative (cf. 26). The stem ending **-a-*, as a former character of the fientive forms, began to be

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[15] Its relics may be seen in the Samogithian "non-organic" *-ə* (for its evidence cf. the article of A. Girdenis & A. Rosinas in GL. 17, 1977, No. 1) which restores itself in the full shape of *a* in a kind of "pig Latin" (*ap/up/ẹp* inserted): *ràts*—*rapàtapasapa* "wheel", but *sùlàšẹnẽ·s*—*sùpu làpašẹpẹnẹpẽ· s* "with bacon".

[16] For the striking parallelism in Kartvelian see rf. 28.

[17] In final the opposition */m/* : */n/* was neutralized, therefore **-m* = **-n* (rf. 12: 24–25).

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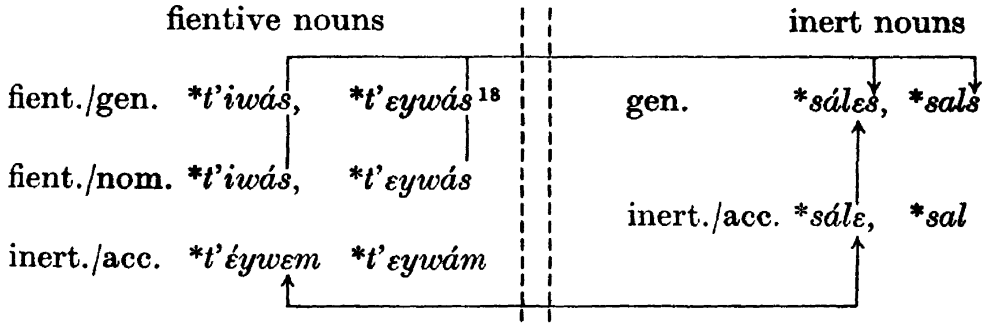
generalized on all the cases of the nominative system exclusively in the fientive nouns. While being a member of the alternation *-Ca/-C*, it received semantic-morphological burdening, since the inert nouns were characterized by the absence of this **-a-* in all cases including genitive. The latter fact is clear, because, when the fientive system was decaying, the inert nouns became able to use for genitive the sigmatic marker of the fientive nouns —cf. gen. Skt. *dyoḥ*, Gk. δεσ(πότης) < **dem-s-*. Thus the primary alternation *-Ca/-C* made a step towards morphologization **-a-* ‖ *Ø* (**-a-* as a grammatical sign opposed to zero) which was finished in compromise: while zero became specialized for the inert form of nominative-accusative, the stem ending **-ε* of the inertive forms, having been ousted from accusative of the fientive nouns by the thematic **-a-*, was still remaining possible in the inert nouns and was fixed there by the sigmatic genitive formant, historically alien to the inert nouns:



Such thematizing of the athematic genitive conditioned the later "correction" of this ending according to the thematic nominative-genitive pattern **-as*[19]—cf. Lith. *šunès*, Lat. **pedes*,

[18] The stem is a result of levelling in accordance with inert/acc. **t'éγwεm*.

[19] At first the thematic genitive-nominative ended in **-as*, still preserved in Hittite and Prussian. While the fientive structure kept on decaying, the necessity to differentiate cases became stronger and in many languages the genitive form was substituted by the degenitive adjective derived from the genitive from by the relative suffix **-(i)ya-* (as πάτριος from πατήρ, *áśviyas* from *áśvas*, etc.). This stage is represented in Luwian and Lycian forms with *-šši-*, *-ši-*, e. g. *maššanaššiš* "of deity", "divine" < nom.-gen. **masana-s* + *ya-* + case endings. The next stage is represented in Aryan where *devásya*, earlier—the declinable degenitive stem derived from gen.(-nom.) **devás*, became fossilized as innovative genitive. The same is true for Gr. λύκου, -οιο < **-οσιο*; in

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**sales* : Lat. dial. *NOMINVS* < **nominos*, Gk. ποδός, ἁλός (with the oxytonic accent as in the fientive nouns). In this way the triad of the nominative and genitive endings **-s*, **-as*, **-es* (tradit. **-s*, **-os*, **-es*) came into being.

As for the thematic stems, the ε-vocalism in vocative seems to be their very archaic feature as exactly reflecting the primitive inert bare-stem form—cf. Gk. φίλε, Lat. *ocule*, Lith. *výre*.

Thus the appearance of the thematic and athematic declensional models became a fact. Their further history is connected with the redistribution of stems by gender and with the contamination of forms.

3.4. Once the thematic **-ε-* was ousted from the inertive/accusative form of the fientive nouns, the regularity emerged for the fientive nouns to be gathered in the thematic declension and for the inert nouns to be gathered in the athematic declension. This was made possible due to the coexistence of the nominative structure with the decaying but still surviving fientive structure. On the basis of the inertive form of the fientive nouns new inert nouns began to develop because of the syntactical identity of the inertive case form of the fientive nouns with the inertive form of the inert nouns in the fientive sentence and in the inertive sentence.

We dare say that the bicasual system of the later fientive epoch had to reflect the earlier epoch when the inertive form of the fientive noun was rather an alloseme than a really case form. The merger of the both forms in one lexeme was connected with the development of the mixed type of the fientive

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Sl. *če-so*, Pruss. *ſ-te-ſſe* we have the *o/e*-stem and not the *yo*-stem archaic (de?)genitives (cf. alive degenitives Russ. *ix-nij*, *jevo-nyj*). The idea that in all these patterns we have gen. **-s* + (in tradit. voc.) **yo*, **o/e*, but not gen. **-syo*, belongs to V. Ivanov (rf. 12: 28; cf. also rf. 23: 91, 92). As for the particle **ya*, its root seems to be the "PIE." formant of "genitive", exactly—relative pronoun (survives in Celtic and Italic *o*-stem gen. *-i* > *-ī*, outside IE.—in Kartvelian *-i* (later + gen. $-s_1$, instr. *-t*, *-v*—see rf. 28) or in Semitic genitive (and later—nisba—see rf. 31). This primitive "genitive" was not a case form but a construction of the non-declined noun with the relative particle pointing to possessor (the "PIE". stage).

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construction[20] and the rise of the primitive bicasual declension. So in the later fientive epoch the inertive form, e. g. "heaven", "the sky" belonged to the same fientive lexical item as the homoradical inertive form "Deum" or the fientive correspondence of the latter "Deus", as well as, e.g. "step" was an alloseme of the lexeme "leg". While the relexicalization of the inertive forms took place in the arising nominative system, new inert nouns came into being in the inertive/accusative form of the fientive nouns with its postponed nasalization — cf. Gk. neutr. ζυγόν "yoke" side by side with masc. ζυγός, which possibly is not a later innovation and resembles some fientive meaning. Or, using our example:
nom.acc. Hitt. *pedan*, Skt. *padám*, Gk. πέδον out of inert/acc. **phat'ám* ‖ **phét'am* < inert. **phét'ε* ‖ **phεt'* ground, "place" ← "step" ← "foot" ← "pedem": cf. Hitt. *pata-*, Dor. πώς, ποδός, Lat. *pēs*, *pedis*, Skt. *pāt* out of fient./nom.-gen. **phat'ás* ‖ **phεt'ás* ‖ **phāt's* ‖ **phēt's* < fient. **phεt'á* "pes". Here is the origin of the thematic neuter gender nouns[21].

The creating of the new inert nouns on the basis of the inertive/accusative form of the fientive nouns reveals the reason why "the sign of animate object" *-m* has been removed into inertive/nominative-accusative of the (thematic) inert nouns from inertive/accusative of the (thematic) fientive nouns. Merely testifying that the nasalized accusative of the fientive nouns is historically the same case as the non-nasalized accusative(-nominative) of the inert nouns, does not mean to explain the very transferring of nasalization from one declensional model to another. "Ex-

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[20] According to G. Klimov, there are three stages of the development of sentence of the fientive structure: the verbal, mixed and nominal ones. The verbal fientive construction is such a type where the syntactical relations are expressed in the verbal form; in the mixed type they are expressed both in the verbal and in the nominal form; in the nominal type they are expressed in the nominal form only. The development is supposed from the verbal to the nominal type (rf. 16: 58; 15: 165–171).

[21] V. Mažiulis gives quite another explanation based on the identity of the regular consonantic datives in **-i*, **-ei* with the irregular *o*-stems' datives in **-i*, **-ei*, that both stems' declensional patterns were thus able to fall into contamination (rf. 23: § 82).

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planations" of such a kind may be found in my earlier articles (cf. 24:33; 26:333, etc.). The identity of case does not explain anything, since the nominative-accusative of i_2-, u_2-, C_2-stems historically has no nasalization (cf. Hitt. *ḫuwaši*, Pruss. *meddo*, Gk. γένος, etc.)[22]. The key to the problem is that the nasalization was not transferred, but rather the complete thematic inertive case form of the fientive nouns began to be used as a new inertive noun. The coincidence of the case form (accusative, accusative-nominative) only made this process possible. In i_2-, u_2-, C_2-stems, however, i_2- and u_2-inertive forms had their "'thematic" (apophonic) correlates in $ai/\varepsilon i_1$-, $aw/\varepsilon w_1$-fientive case forms which were imposible with the nasal formant of inertive, since their inertive case form was a quite different stem. Having no nasalized correlates in their own stem-pattern, the (inertive →)accusative forms of *i*-, *u*-stems might have received nasalization only by analogy from the thematic *o*-stems. In certain instances it has happened and in certain not. As for the non-nasalized o_2-stem nominative-accusative singular in Baltic and Slavic (see 23: § 47), it appears to be no older and no younger than the nasalized one, since originally athematic nouns, though thematized in the same way, were not receiving nasalization in course of this process for the systematic reasons: all inert nouns were indeclinable in that period (30), therefore no diclensional formant in them was possible.

The quantity of the thematic forms in neuter was also increased by some inert nouns which were not derived from the thematic inertive/accusative form of the fientive nouns, but were thematized by analogy with nouns involved in that process.

4.0. All these questions are discussed by J. Kuryłowicz in his "Inflectional Categories". He connects the development of case

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[22] J. Kuryłowicz, who also points to the identity of cases, explains the penetration of the nasal formant in *o*-stems by the subordination of these stems to the pronominal *o*-stems, where nom. *-os*: nom.-acc. inanim. *-od*, acc. anim. *-o* were older than nominal *-o* in all cases (19: 210). Such assumption, however, is absolutely unmotivated, not to speak that there are no real grounds (except tradition) to reconstruct, e. g. neutr. Gr. τό, Sl. *to* as descending from **tod*. In Lithuanian as well we have no **-d*: *tai*.

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inflections in various stems with gender (animate or inanimate), stress and apophony (all as if in accordance with our principles) and with the role of pronominal morphology. Thus he writes: "The split between the neuter and the animate gender may be retraced to the period of the accentual rearrangement of the nominal paradigms which took place in early IE." (19:207). So then it becomes impossible to speak about the original distribution of all lexics in animate (more correctly—fientive) and inanimate (inertive). Thus Kuryłowicz's view disagrees with our view from the very start. Even the method he uses to back up his basic theses is unacceptable for us. He states that "the *-s*-nominative has existed in the pronoun before its spread in the animate gender" (19:209), even though the motivation is as follows: ". . . the formal opposition between the two genders is often rooted in the pronoun (demonstrative, interrogative). A limitation of the primitive scope of the neuter, even its disappearance in the noun, may still leave certain traces of its former existance in the (substantivized) adjective and in the pronoun. I. e., if the neuter survives in the adjective, it also continues to be distinguished in the pronoun. Finally, it may be restricted to the pronoun only. Cf., on the one hand, Lith. or Spanish /. . ./. On the other hand, French has *ce* : *celui*, *celle*, and *quoi* : *qui*, but no trace of the neuter in the substantivized adjective" (19:208–209). In other words, a kind of universal is suggested: a phenomenon is mostly conservative there, where it has originated. If the horse tramway had appeared first in England, England must have been the last country to abandon these means of communication. And vice versa: as this transport survived in Tbilisi up to the II World war, it means that Georgia was the motherland of the horse tramway. It seems that here we have to do with the false applying of synchrony in the diachronic method.

4.1. J. Kurylowicz defines gender as "a *semantic* category motivated 1. by special derivational (or inflexional) elements; 2. by the sense of the root in certain primary nouns. It is only *inferred* 3. from the form of the attribute (or the anaphoric pronoun) in all the other cases". Further he writes: "But whereas in groups 2 and (partly) 1 gender is both *semantic and*

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presupposed by the form of the attribute or of the anaphorical element, it is only presupposed in the rest of cases. From the descriptive point of view, the latter group must be considered as having lost its semantic motivation. /. . ./ this seems to be at the same time the historical (diachronic) explanation of grammatical gender" (19:32–33).

Here is no need to say that gender is a kind of a broader universal which unifies all possibilities of the lexical-grammatical grouping of nouns. Therefore in order to define gender as a linguistic category, one must confront it with the other means of the lexical-grammatical grouping. Only after the general definition is obtained, it becomes possible to pass to the concrete manifestations of gender, e. g. in IE. J. Kuryłowicz avoids this procedure. It seems to be typical for many great Indo-Europeanists to look at all non-Indo-European languages as second-rate and to treat the IE. material in such a way as if it were sufficient both to solve all linguistic problems and to define all linguistic categories. As a rule examples from other linguistic families merely illustrate already finished deductions. Thus the existence of the semantic-grammatical category of class is usually forgotten by Indo-Europeanists while they speak about grammatical gender. Nevertheless the category of class exists in many Caucasian and African languages and is motivated 1. by special derivational elements; 2. by the sense of the root. The single item by which it is not really motivated in any instance is 3—"the form of the attribute": if class does exists in a language, not a singular instance may be found, in which class would not be expressed by morphological formant and might be seen only from the corresponding attributive form. The sense of the root automatically conditions the appearance of the corresponding class indicator. Consequently if definitions 1,2 are common both to class and to gender, they are suited for defining neither of them. If so and if there are no more items of the semantic-grammatical grouping of nouns in all languages of the world except class and gender, the definitions 1, 2 are no definitions at all. In order to define the gender category, the distinctive feature of the latter must be found. And it is, of course, the definition 3, named diachronic by its author. So the IE.

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gender is mere morphological category and its connection with semantics is only an irrelevant feature in its definition. Thus among the grammatical categories one must distinguish between those with semantic burdening and those that have lost it. The change of the structural typology causes the substitution of the semantic-grammatical category of class by the grammatical category of gender.

4.2. Typological investigation of various languages shows that gender does not appear in systems where the single form expressing the direct object does not develop. Thus we have no gender and no case of the direct object (accusative) in Paleo-asiatic, American Indian, West and East Caucasian, Basque, Kartvelian, Sumerian, Elamite, Proto-Hittite, Bantu, Sudanese, Austronesian, Australian, Papuan, nor (if no speaking about non-systemic relics) in modern English, Middle and modern Iranian, in the part of the modern Indo-Aryan languages, notwithstanding the fact that in some of them rich declension may be represented (cf. 10 cases in Chukchi, 35 cases in Dido). Side by side there is accusative as the single form of the direct object and there is the category of gender in Baltic, Slavic (except modern Bulgarian with relics), Germanic (except modern English), Anatolian, Greek, Sanskrit, Old Iranian, Old Akkadian, Old Canaanite, Amorite, Ugaritic, Arabic, Ancient Ethiopic, though in course of the *transforming* of the system with gender and accusative to that without gender and accusative the case form of the direct object is first to be lost (cf. the development in English, Scandinavian, Romanic, Bulgarian, Albanian, Phoenician, Hebrew, Aramaic, Berbero-Libyan, Kushitic, Chad). As a rare anomaly of such development Old Armenian may be pointed to. As for Tokharian, Uralic, Dravidian and Altaic, their accusative is not a single case form marking the direct object and depends on the sensibility or animateness of this object. Thus accusative, when it is the single form of the direct object, may be treated as an implication of gender (see 29). Therefore it may be assumed that the development of gender out of the category of class is a morphological process which redistributes the stems according to the form of the direct case.

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In the fientive structure the direct object is indicated by the form of the subject of state, i. e. by inertive, The bare stem inertive may later serve in creating the accusative form, while the fientive structure decays. This form also indicates the subject in the former class of inert nouns, where the new subjective case (nominative) is formed on the basis of inertive as well. Thus the nominative and accusative forms appear identical. Their non-coincidence in those nouns inherited from the fientive class stimulates further morphological differentiation of the two new lexical-grammatical groups—the "strong" (nominative differs from accusative) and the "weak" (the both coincide) genders—by their corresponding concord with the attributive parts of speech. The earlier division of the lexical-semantical classes of nouns into subclasses, characterized by their supplementary features (stem determinatives as *-r*, *-l*, *-n*, *-w*, *-y*, *-s*, the place of intonation, apophony, etc.), enforces the formation of the declensional types of stems. Thus gender is a grammatical category which is determined *morphologically* (first and foremost —by declensional models, cf. IE. *o*-models' systemic relation to the common gender, *ā*-model's relation to feminine, *s*—to neuter, etc.[23]), and *not* semantically (as classes).

Consequently in all languages with the declensional gender we have the material connection between the form of accusative and the "weak" gender form. In IE. it is the primary identity of the accusative form of the common gender with the nominative accusative form of the neuter. In Semitic it is the indentity of the accusative in *-a* with *-a* as the real marker of feminine (see 29).

4.3. At the intermediary stage, when fientive and nominative sentences coexist, "strong" gender words functioning as objects of action do not yet differ in their form from the nominative-accusative of the "weak" gender words. Nevertheless the former inert nouns used in the nominative begin to agree with the verb in the same way as do the "strong" gender nouns in nominative, i. e. they may become subjects by the fientive verb and take a direct object. This causes the contamination of the "weak"

[23] Accordingly we introduce the term *declensional gender*.

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gender by the "strong" one. "Weak" gender stems together with some "strong" gender stems, which have passed into "weak" stems, form a new gender Σ. This is the basis for the appearance of the system with three genders. The traditional explanation about the split of the IE. common gender into masculine and feminine is not sufficient, because of the evident connection not only of some feminine lexics with neuter ones (Skt. *jīvitam* : Lat. *vita*, neuter plural formations as collective "feminine"), but also of some masculine lexics with that of neuter (e.g. Gk. ζυγός and ζυγόν). In IE. the gender Σ was transitional, the common-"strong" and non-common-"weak" genders mixing. While the unmixed part of the common gender remained proto-masculine, part of the derivational models of the gender Σ, by joining the laryngeal determinative *-H_2, split half-and-half: derivations meaning collectivity produced plural neuter forms (τὰ πλευρά), while the remaining derivations accomodated themselves to the declensional model of proto-masculine (i. e. received endings gen. *-*s*, acc. *-*m*/-*n*) and turned into feminine forms (*vita*, ἡ πλευρά). Part of the common gender nouns also passed into feminine (Hitt. *ḫaššaš* : Lat. *ara*, Skt. *rathas* : Lat. *rota*). The gender Σ models without *-H_2 produced singular neuter forms (*jīvitam*, τὸ πλευρόν) (see 24)[24].

4.4. J. Kuryłowicz treats the development of genders in IE. quite differently, but he does not explain the main thing: the reason for the connection between the oxytonic, or barytonic, paradigms and the "animate", or "inanimate", semantics. According to him such connection was motivated by the exterior factors (19:207–208). What was the reason for the phonetic merger of the accentual columns of the strong and weak case forms in the oxytonic paradigm (cf. this work, 2.2, 3, 7 above)? For what purpose must one case have been strong and the other one weak? And if it is the connection with the distinguishing of animateness-inanimateness, for what reason could such incomprehensible complications have arisen in the primitive nominative system? It will be useless to search for the answer to these questions in

[24] The development of the athematic feminine forms is explained by J. Kuryłowicz (rf. 19: 215).

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the "Inflectional Categories". Why the paradigms of the demonstrative pronouns must have differentiated from those of nouns from the very beginning and why the difference between the animate and inanimate in accusative singular was originally only in pronouns, remains obscure. It seems that the only argument for J. Kuryłowicz to assume the pronominal origin of case endings is pointing to such authorities as F. Specht and J. Schmidt (19:211).

5. We see that the assumption of the nominative character of the PIE. structure enormously complicates all explanations (cf. 19: Ch. VIII, IX). Their simplicity by assuming the fientive structure is already a good argumentum pro. How naive is such "nominativizing" by reconstructing PIE., resp. IE. syntactic models may be seen from the following conjectures of W. P. Lehmann:

"Stage A /g^{w}*Λγws* g^{w}ΛmtΛy/ [g^{w}eγws g^{w}ṃti] "the cow is
Stage B /g^{w}ˈˈΛγws g^{w}ΛmtΛy/ [ˈˈg^{w}eγws g^{w}ṃti] coming"
Stage C /g^{w}eγws g$^{w}_{e}$m-tyη/ [ˈˈg^{w}eγwz g^{w}ṃ-ti]
[. . . .]
Stage E /ˈˈg^{w}o·ws g^{w}m-tyη/ [ˈˈg^{w}o·wz g^{w}ṃ-ti]" (21:188).

(I don't envy our honourable ancestors who spoke this splendid language! It is obvious that of all the mankind Mr. W. P. Lehmann is the single man who still enjoys abilities to pronounce this, God bless him!)

Thus the closeness of accusative forms to neuter ones loses its sense from the very PIE.! We dare reinterpret these reconstructions in accordance with all said above:

Stage A /k'ΛwΛ k'ΛmΛ/ [k'åwå k'åmå]
Stage B /k'ΛwΛ k'ΛmΛ/ [k'åwə k'åmə]
Stage C /k'áwə k'ámə/ [k'a̋wə k'a̋mə]
Stage D /k'aw k'ám-t(h)a/ [k'a̋w k'a̋mtha]
Stage E /k'āws k'am[t(h)]/ [k'a̋wz k'a̋mth]

At the stages A, B, C, D we have the inertive sentence of the fientive typology. These stages may be described in accordance to

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the type of the corresponding fientive construction: stages A, B ("Nostratic", or "Borealic" = (pre-)PIE.)—the fientive construction of the verbal type, stage C (complex parent language: IE. + x + y)—the fientive construction of the mixed type; stage D (IE.^{x} + IE.^{y})—the fientive construction of the nominal type[25] (in the verbal form of the inertive sentence inertive enclitic **-tha* "id" may appear, which in previous epoch was possible only in the fientive sentence by fientive verbs where it indicated the direct object); stage E (separate IE. dialects)—nominative construction (the subject by the intransitive verb may receive *-s* only in the nominative epoch).

So in this instance as well we see a mistake made by a scholar who does not want to take into consideration either the history of the nominative linguistic structure, or the evidence of the non-Indo-European languages, which may shed light upon this history. Otherwise it would be clear for him that in PIE. "the coming cow" (as for the stages A, B, the meaning might have been also "the cow is standing"—cf. the root *QWM* in Semitic) by no means could be expressed in the same way as e. g. "the cow calving a calf".

6.1. We have discussed the processes that had caused the appearance of the IE. thematic and athematic declensional patterns. As for verbs, the origin of the thematic vowel in them must be the same as in nouns. Owing to the processes of reduction, the primary stems (originally not differentiated from the nominal ones) appeared as variants with the ending *-ε/-*a* or without it. Later the former variants were specialized for some group of verbs while the latter were specialized for the other verbs. In this way the stem ending became morphemized in a shape of thematic vowel before the personal endings.

6.2. As for the apophony *ε—*a*, in the earliest epoch no difference existed between verbal and nominal forms. Therefore it seems quite natural that *a*-vocalism is represented in causative forms (cf. Gk. φέβομαι, but causative φοβέω) which undoubtedly descend from the fientive forms.

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[25] See fn. 20.

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7. Thus our view on the origin of the IE. vocalism suits to explain all the main models of IE. stems without breaking the basic principles of the laryngeal hypothesis with its statement of the PIE. syllabicity. There is strong criticism being directed against this hypothesis (cf. 22; 36) and it is undoubtedly J. Kuryłowicz's retrogression (cf. 18:38), which has provoked this criticism. O. Szemerényi, who has made his aim to rehabilitate the traditional Indo-Europeistics of Brugmannian era, declares the typological impossibility of the monovocalic languages. This declaration is based on his criticism of W. Allen's and A. Kuipers' works on the monovocalism of Kabardian and Abaza (38:75–81). But as D. Job has recently shown, the structural analysis testifies against the critics of W. Allen and A. Kuipers (14:52–57). After all, however, it makes no difference whether the monovocalic structure does really exist there or somewhere else, or not, since the probability should not be excluded that the development from the prehistoric syllabicity to the historic polyvocalism is a diachronic universal connected with the origin of human language. Such a universal must have been in force during milleniums and cannot be tested in so recent periods as after the emergence of the first written documents in Old Egyptian, Sumerian or Hittite.

One argument of O. Szemerényi may be rejected already relying on the principles of this article. Thus O. Szemerényi alleges the necessity of the existence of PIE. */o/ beside */e/ on the single possible reconstruction of the root "cow" as *g^wow-, but not as *g^wew- (38:70). Of course, one can hardly reconstruct a laryngeal in this root. But in accordance with our view, the types **CεS* and **CaS* must have been only facultative dublets in the fientive nouns (where the timbre of the fientive marker was able to be generalized on the root) and thus the predominance of one of them is a result of historical chance.

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Abbreviations

Bl.	Baltistica, Studies in Baltic Linguistics. Vilnius.
EKP.	Érgstivnaja konstrukcija predloženija v jazykax različnyx tipov. Leningrad 1967.
GL.	General Linguistics. Pennsylvania.
IF.	Indogermanische Forschungen. Berlin, New York.
KSG.	Konferencija po sravniteljno-istoričeskoj grammatike indoevropejskix jazykov. Moscow 1972.
KNJ.	Konferencija Nostratičeskie jazyki i nostratičeskoe jazykoznanie. Tezisy dokladov. Moscow 1977.
LP.	Lingua Posnaniensis. Warszawa, Poznań.
Matsne.	Izvestija Akademii Nauk Gruzinskoj SSR, Serija jazyka i literatury. Tbilisi.
NTS.	Norsk tidskrift for sprogvidenskab. Oslo.
TJ.	Toxarskie jazyki. Moscow 1959.

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Typology and the Indo-European Prepositions

I. Historical Background

In recent years a new approach to historical reconstruction has achieved prominence in diachronic studies. This new approach is, of course, 'typological syntax'. Typological syntax is itself principally an outgrowth of Greenberg's landmark Basic Word Order typology study of universals (1963), and it has been embraced by a fair number of linguists working in the field of syntactic change. This support and enthusiasm for typology by historical syntacticians is not surprising: after all, syntax has always been the stepchild of historical-comparative grammar. Up to the time when the Basic Word Order typology was first adopted by diachronic linguists, there was in essence no unified model even remotely adequate for the treatment of the mechanisms and principles of syntactic change. The best accounts by far, at least for the Indo-European languages, were framed in the format of traditional grammar. However these early handbooks, most of which have never been surpassed in pure scholarship and devotion to detail, lack the theoretical perspective necessary for a unified treatment of syntactic change. And in the lengthy period between the appearance of the great Indo-European handbooks and the late 1960's, it appears that trends and developments in synchronic syntactic theory had a minimum overall effect on historical syntactic theory. European structuralism had little impact; and American structuralism, with its emphasis on discovery procedures, especially for phonemics, was even less helpful in matters relating to syntax.

The impact of generative grammar on historical linguistics is a familiar topic, and I will take no time reviewing it here. But

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there is one interesting curiosity from the TG. period in historical linguistics (roughly 1965 to 1973) which stands out: despite the fact that the foundations of TG are in syntax, and that the theory was most closely attended to and worked out in the syntactic component, the chief—perhaps the only—effect of TG. on historical linguistics was in the field of phonology. It must be conceded, judging from the scope and quantity of problems of syntax which were addressed in the TG. format, that the overall contribution of this model to our understanding of syntactic change must be judged as less than impressive. (The work of Closs-Traugott might be judged differently, but her work focuses on changes from one well-known stage of English to another, and not with reconstruction).

So by the time Lehmann began applying the Basic Word Order typology model to the investigation of changes in syntax (e. g., 1969, 1971, 1973, and culminating in 1974), the entire field of historical linguistics was in serious need of a framework in which to analyze syntactic change. Generative grammar had failed to live up to its advance billings, and a large number of historical linguists welcomed the typological model as a fresh, new starting point.

But how has typology fared? Is it all that it was supposed to be? Recent studies in the historical syntax of Indo-European strongly suggest that typological syntax is simply inadequate by itself as a model of change (see esp. Watkins 1976; I should point out that Watkins' work has always exemplified the best in Indo-European scholarship, no matter what theory is in current vogue).

The typological model is essentially based on the implicational relations which are said to hold between various structural configurations within a language. Some structures are "harmonic" or "consonant" with others. When harmony is found, explanation has been provided; disharmony, on the other hand, suggests either a changing language or a contact situation. In effect, the method is foolproof, since it has so many escape hatches available to explain away exceptions to the expected patterns. As Jeffers has pointed out (1976: 987), the typological method contains two fundamental flaws:

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a) It requires that proto-languages be typologically consistent. Since no such living language has ever been found, we are only fooling ourselves if we think of proto-languages in this way.

b) It is circular. Constructions which are harmonic with the supposed reconstructed proto-language are considered archaic and demand no synchronic explanation. At the same time these very constructions are used to support the initial hypotheses concerning the basic typological character of the proto-language.

II. The Indo-European Prepositions

In this section I hope to make a further contribution to the weakening of the (simplistic) use of typology by showing that one of its predictions is in error, and that certain developments in the IE. languages which have been explained by implicational ordering can be better explained by more "conventional" means. I use the term "simplistic" above to stress the point that not all typological studies aimed at reconstruction are misguided. On the contrary, I would single out the excellent, probing and theoretically sophisticated work of Paul Friedrich (esp. 1975, 1977) as representative of the good use to which typology can be put if properly and judiciously applied. I hope only to show here that typology alone is simply not sufficient in itself to explain phenomena of syntactic change.

It is generally, though by no means universally agreed that PIE. was postpositional (but see Friedrich 1977: 468–70). The evidence for a postpositional PIE. comes from a variety of sources, but chief among them are Hittite, which is exclusively postposing (cf. J. Friedrich 1974), and Vedic Sänskrit, where postposing is the general rule, though preposing is frequently found as well. Other support comes from unexplained (and therefore) relic forms in Greek and Latin. A brief sampling of the evidence follows:

(1) Hittite (cf. J. Friedrich 1974: 129—30)

É-ri *anda*
'*in* the house'

šuḫḫi *šarā*
'*on* the roof'

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(2) Sanskrit	sa dēvó dēván
(cf. Delbrück 1911:	*práti* paprathē pṛthú.
776–77)	'The god extends himself wide *behind*
	the gods.'
	tásmād mádhya *práti* pašúr váriṣṭhaḥ
	'For that reason a cow is fattest
	with respect to the middle.'
(3) Greek (Homeric)	'Ιθάκην κάτα
	'*down towards* Ithaca'
	τοῦ μ'ἔκ φασί γενέσθαι
	'*from* whom they say I have been
	begotten'
(4) Latin	quo*cum*, me*cum*
	'*with* which, *with* me'

In a strong programmatic statement, Paul Friedrich (1976) has criticized the postpositional view of PIE. as relying excessively on Sanskrit and Hittite, while ignoring the fact that Latin, Greek, and most of the other stocks are overwhelmingly prepositional. Friedrich suggests a plausible alternative to the accentual arguments for postposing (recently restated by Hock 1975: 95–6) preferring to view the evidence as favoring preposing, or at least ambivalence in the placement of what he calls 'locative auxiliaries' (1977: 468–70). More generally, he makes a methodological point which must be heeded by historical linguists of every persuasion, but especially typologists: simply because a constituent is more marked (e. g., the postposed *-cum* of *mecum*), this does not necessarily mean that it is more archaic, and 'therefore a precious "residue" of an earlier unmarked form. We have to recognize that innovating forms often have a marked status for centuries, even millenia, and that, on the other hand, stylistically marked constructions are not necessarily the reflex of formerly unmarked constructions' (1976: 210–11; his emphases).

No matter how we decide the issue of pre- or post- posing in Indo-European, it is clear that no solution will please everyone; the evidence is sufficiently ambiguous that the "correct" solution may be more a matter of taste than hard facts. If

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PIE. was SVO., as Friedrich suggests, then there are inescapable disharmonies with the postpositional stocks like Hittite and Vedic. If it was SOV., as Lehmann proposes (e. g., 1974), then there are disharmonies with the prepositional stocks. Furthermore, if SOV., PIE. must also have been internally inconsistent because of its system of preverbs. A language which is SOV., and postpositional should not allow preverbs. To illustrate with Latin and Greek, if the language was postpositional (cf. *mecum*, Ἰθάκην κάτα), then the appropriate and harmonic parallel structure with verbs would be something like **venireper* (for *pervenire*) or *ἐίμιπερι (for περίειμι). But no such post-verbal construction is recoverable or even suggestible for PIE. So regardless of how we look at it, the language must have been typologically inconsistent. From this the conclusion seems inescapable that we cannot explain the purported shift of the original IE. postpositions to prepositions as resulting from the changing pattern of SOV. to SVO. word-order (Lehmann 1974: 212). For one thing, as Friedrich points out, there is ample evidence that PIE. was SVO; secondly, if the language was already inconsistent typologically, why should we expect this major reshuffling to promote conformity here?

I would like to suggest that the rise and development of the IE. prepositions in fact had nothing whatever to do with word-order or word-order change. I believe that another mechanism was at work here, which I will now outline.

It is commonly known that the IE. pre- and post-positions are largely derived from earlier particles (though some come from case-endings). These particles supplemented case-forms in specifying the semantic and syntactic functions of nominal forms. Lehmann contends (1974: 212–13) that some of the early IE. particles existed in close association with verbs, and these were often the same particles which were used (he claims) as postpositions. He illustrates these two uses with Hittite *piran* 'before, in front':

(5) (= L 50) kuiš LUGAL-ua-aš piran ēšzi
who king's front he-sits
'whoever sits before the king'.

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Here Lehmann contends that he correct bracketing of *piran* is as a postposition: [*LUGAL-ua-aš piran*].

(6) (= L 52) nu ANA ^{D}IŠKUR Ù DINGIR.LÚ^{MEŠ}-ŠU
Ptc. to weather-god and his-male-gods
ANA ^{D}UTU Ù DINGIR.LÚ^{MEŠ}-ŠU píran
zikizzi
to sun-god and his-male-gods front
he-lays-in-turn
'He lays in turn [the slices of bread] for the weather god and his male gods and the sun-god and his male gods'.

Here Lehmann suggests that the proper bracketing of the particle *piran* is as a preverb: [*píran zikizzi*]; cf. also A., 101: κατ' ἄρ' ἕζετο 'down he sat him'.

But one should note that in many cases the analysis of particle or adverbial elements is open to question and alternate interpretation. As is pointed out by Smyth (1920: 366) 'it is often impossible to decide whether the prepositon [= adverb, P. B.] belongs to the verb or noun. Thus, A, 439: ἐκ δὲ Χρῡσηὶς νεώς βῆ may be either 'Chryseis went out of the ship' or 'Chryseis went-out-from (ἐξέβη) the ship'. I am not suggesting that Lehmann's interpretation is incorrect, but rather that such evidence is frequently unanalyzable.

The particles in question here had rather free movement characteristics, but the unmarked position was before the verb; if marked they are placed in clause-initial position (see Watkins 1964). Lehmann feels that 'in the course of time, preverbs in unmarked position came to be combined with their verbs, though the identity of each element is long apparent in many of the dialects' (1974: 213). Preverbs which for some reason remained uncombined came to be treated as adverbs. Thus the different positions of these various particle elements in the different stocks eventually led to different word-classes.

It appears that Lehmann's scheme, while possible, is deficient in several crucial respects. First, and principally, it provides no motivation for the combination of particle and verb. Were there no restrictions or constraints governing this union? Why did

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some combine, while others did not? Secondly, this analysis fails to account for the morphological identity of preverbs and pre- and post- positions. There is a morphological transparency between these forms which could not have resulted from such a seemingly accidental process.

As an alternative, I would like to suggest the following: adverbs, whatever their remoter origins (i. e., case-endings, nouns, or particles), preceded the verb in the IE. sentence. These adverbs became attached to the following verb by this rule:

(7) $\# \rightarrow + / \begin{bmatrix} \langle + \text{ adv} \rangle \\ \langle + F_1 \rangle \end{bmatrix} ___ \langle + \text{ verb} \rangle \langle + F_1 \rangle$

$\langle F_1 \rangle$ in (7) refers to the collocation of semantic and syntactic features which determine the compatability of a given adverb with a given verb: this is the necessary condition for a union. For example, Lat. **circumscire* 'known around' unacceptably combines two elements (cf. Gk. *περιγιγνώσκω—Skt. *pari*+*jñā* has the meaning 'carefully observe, find out'); Lat. *circumire* 'go around' is perfectly acceptable (cf. Gk. περίειμι, Skt. *pári*+*i*).

Rule (7) represents a case of "junctural tightening", and I would suggest that this tightening reflects the relaxed phonological features characteristic of allegro style. By this rule, the word-boundary separating the adverb and verb is weakened to a morpheme boundary, and the free adverb becomes a bound preverb. I propose that the process outlined in (7) represents the first stage in the creation of the IE. prepositions. The grammatical effect of this rule is the addition of the feature $\langle +$ preverb$\rangle$ in the lexical specification of adverbs, as illustrated here with **prō*, *perō* 'before, forward':

(8) **prō*, *perō* $\begin{bmatrix} \langle + \text{ adverb} \rangle \\ \langle + \text{ preverb} \rangle \end{bmatrix}$

Recall that the original function of the particle/adverb was the supplementation of the cases in specifying the syntactic and semantic functions of nominal forms. I believe that the examples below preserve the syntactic independence of preverb and verb:

(9) Plaut., Pseud. 1, 2, 25:
Te cum securi caudicali praeficio provinciae.

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'And I appoint you with the axe in charge of the chopping department'.

(10) Caes., B. G. 6, 12: Omnibus Druidibus praeest unus.
'One man is in charge of all the Druids.'

(11) Dem., 18. 236: περιγένεσθαι ἐμοῦ
'to surpass me'

(12) (Smyth 1920: 367): ἐκβῆναι τῆς νεώς
'to go-out-from the ship'

What is noteworthy and relevant about (9)–(12) (and many more could be provided) is the fact that the preverbs in these sentences are governing their own objects, independently of the verb. In (9), for example, *prae-* governs *provinciae*, while *facio* governs *te*. In (11), ἐμοῦ is governed by the preverb περί-, not by γένεσθαι. I believe that Latin verbs like *praesum*, *praeficio*, etc. represent an archaic verbal type, one which maintains the syntactic independence of preverb and verb.

Once the junctural tightening outlined in (7) has made the union of adverb and verb possible, the process as follows: as the language evolves, the need to mark finer semantic detail increases with the expansion of the lexicon. To achieve these finer semantic specifications within the verbal system, the system of adverbs is exploited. By elaborating and expanding the adverbial system, it is possible for the language to avoid wholesale suppletion while refining the semantics of verbs. Suppletion, especially in large quantities, increases opacity and reduces learnability, and is therefore avoided. To illustrate (here with Latin), let us assume a root 'come' (*venire*); in order to express such concepts as 'come around', 'come to', 'come through' and so on, it would be thoroughly uneconomical to create a new root in each instance. Thus the adverbs are used instead, joined to their following verbs by rule (7), and eventually relexified as whole units:

(13) circum
ad
per
in
de } + venire

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Such a technique of lexical expansion causes a minimum of disruption within the syntactic and lexical systems.

It is apparent from all accounts that the IE. cases served a fairly wide range of semantically and syntactically distinct functions. It is clear also that languages with overlapping case-functions are typically unstable and develop techniques to more explicitly express syntactic and semantic details than is conveniently allowed when such functional ambiguity is present (see Jamison 1976). There are several obvious ways in which a language might achieve this "strengthening" of the nominal system (see Kuryłowicz 1960). One way would be simply to add more cases, keeping case-functions distinct. This is apparently what many of the Finno-Ugrian languages have done, since only five cases are reconstructible for Proto-Finno-Ugrian, while some of the modern languages have many more, ranging from Lapp with eight to Hungarian with twenty-one (Hodge 1970: 2).

Another way to achieve this strengthening would be to introduce pre- or post-nominal modifiers which supplement and augment the already existing case-markers. But there is no reason to believe that such modifiers should be created de novo: they are already available in the preverbal system. Since most of the qualifiers which modify verbs can modify nouns as well (e.g., locative, temporal, benefactive), the addition of the prepositional function to the adverb-preverb is quite easily accomplished within the lexical system, all the while maintaining morphological transparency between the forms. To accomplish this, preverbs are copied from their preverbal position into prenominal position, as shown in quasi-transformational notation in (14):

(14) X Preverb Verb Noun Y ⟩
1 2 + 3 4 5
1 2 + 3 2≠4 5

(This scheme assumes an SVO. order, though it could be utilized for SOV. as well by realigning term 4). This now leaves us with the following lexical specification for our exemplar **prō, perō*:

(15) **prō, perō* $\begin{bmatrix} \langle + \text{ adverb} \rangle \\ \langle + \text{ preverb} \rangle \\ \langle + \text{ preposition} \rangle \end{bmatrix}$

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I submit that the process represented by (14) constitutes the necessary intermediate stage in the development of the IE. prepositions. It is supported by the following examples:

(16) Caes., B. G. 1, 35: Caesar milites *trans* Rhenum *trans*duxit.
'Caesar led the soldiers across the Rhine.'

(17) Cic., Fam. 12, 23, 2: legiones *ad* urbem *ad*ducere
'to lead the legions to the city'

(18) Thucy. 1. 137: ἐκβῆναι ἐκ τῆς νεώς
'to go from the ship'

Exactly how long this hypercharacterized construction existed productively is by no means clear, but at the least, (16)–(18) suggest its one-time presence. It is obvious that such a construction was actually doomed from the outset, simply because it resulted in an over-specified, redundantly marked construction which was susceptible to reanalysis and simplification. And eventually, in the final stage of the development of the IE. prepositions, the preverb can now be omitted, since the preposition can now serve as the sole modifier:

(19) X Preverb Verb Prep. Noun Y
1 2 + 3 4 # 5 6
1 3 4 # 5 6

Of course, (19) represents the familiar stage of all the late IE. dialects, where prepositions appear freely without accompanying preverbs, or with preverbs other than their own twin. I will list only a few examples by way of illustration:

(20) Cic., Verr. 2, 5, 30: Illos duci *in* carcerem jubent.
'They order them to be led to prison.'
(not *inducere*)

(21) X., A. 1,8,28: καταπηδήσας ἀπὸ τοῦ ἵππου (not ἀποπηδήσας)
'leaping down from his horse'

These later developments, however, are not possible until the prepositional system has been independently established and can stand without the accompanying preverbs.

III. Conclusion

The foregoing has been an attempt to provide an account of the IE. prepositions which is not dependent on typological im-

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plications. I believe that this analysis provides a suitable, internally consistent and internally motivated alternative to the proposal that the IE. prepositions came about as a result of the changing word-order pattern from SOV. to SVO. At the same time I believe I have provided some support for Friedrich's proposals concerning PIE. word-order, though this was not my primary goal[1].

IV. Postscript

In addition to the literature cited above dealing particularly with the IE. prepositions, there has been considerable debate in recent years over what is actually a more basic issue, viz.: why should a language with a well-developed and fully functional case-system develop prepositions at all? The issue has been taken up, for example, by Lakoff 1972, and Vennemann in a long string of papers (e. g., 1973, 1975). Vennemann's overall position has been strongly criticized by Klein (1975), which should be consulted for details.

In concluding this paper, I would like to offer several observations on the process of prepositional development in general, with special reference to Vennemann's suggestions outlined in his "An Explanation of Drift" (1975). Vennemann's basic position is that all phonological change is destructive (see 1975: 292 et seq.): '(7) the dominant types of phonological change are neutralizing and reductive'; '(8) Every morphological system is destroyed in time by phonological change'; (9) As a substantive S-O marking system is eroded by phonological change, word order syntax must react to compensate for the ambiguities and perceptual complexities arising in a consistent verb-final language'. The change of XV to VX order, according to Vennemann, results in a rise in the use of prepositions, 'since their S-O morphologies are non-uniform and not dependable' (1975: 295).

The material from the Indo-European prepositional system analyzed above provides, I believe, counterevidence to Venne-

[1] I would like to thank Raimo Anttila, Martti Nyman, Paul Friedrich and William R. Schmalstieg for their valuable comments on earlier drafts of this paper, with the customary exculpations.

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mann's suggestions, at least for early IE. Vennemann's proposals may be correct for Germanic, but the evidence of other IE. stocks like Slavic, Latin and Sanskrit, where case-systems and prepositions existed side-by-side for several millenia, surviving countless reductive phonetic changes, suggests that phonology alone will not provide ample impetus for the demise of a case-system and the rise of compensating prepositions. I would suggest, in fact, that the pre- or post- positional systems of a language must already be established before reductive phonetic changes can destroy meaningful, productive and necessary distinctions in a language. And it appears that the evidence of the Finno-Ugrian group, especially Hungarian, proves that just the reverse might happen. I hope to take up this issue in more detail in a later paper.

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Alteuropas nordpontische Namenlandschaft

Welcher Sprache jene älteste Namenschicht entstammt, die sich im nördlichen Einzugsbereich des Schwarzen Meeres freilegen läßt, ist strittig geblieben. Wolfgang P. Schmid erklärte 1966 den Dnjestr und den Dnjepr zu Kronzeugen für die Priorität eines iranischen Stratums, dem auch der Don, der Donjez und der Südliche Bug zugehören sollen[1]. Dem habe ich 1973 entgegengehalten, daß es sich, wie klare Namenübereinstimmungen zeigen, vielmehr um einen Teil „Alteuropas" handelt[2]. In seiner Rezension meines Buches, die „eher eine Auseinandersetzung als eine Besprechung" sein will, gibt Sm. sein iranisches Grundstratum preis[3]. Aber diesem Zugeständnis folgt sogleich die Gegenkritik. Es sei verfehlt, von einer alteuropäischen Namenlandschaft mit bestimmten Sondermerkmalen zu sprechen, wo offensichtlich nur ein Stück Normal-Alteuropa vorliege. Zum anderen hätte ich verkannt, daß der nordpontische Gürtel mit iranischen Umbenennungen durchsetzt sei, als deren wichtigste Sm. *Dnestr* und *Dnepr* in Anspruch nimmt. Somit rettet er aus seiner früheren Version gerade diejenigen Namen, die ihn seinerzeit auf ein iranisches „Uraltstratum" gebracht hatten, und frisierte sie zu nachträglichen Einsprengseln in einer nichtiranischen Schicht um. Als völlig unfundiert schiebt er schließlich

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[1] Wolfgang P. Schmid, Alteuropa und der Osten im Spiegel der Sprachgeschichte (Innsbruck 1966) = Innsbrucker Beiträge zur Kulturwissenschaft, Beiheft 22 (im folgenden: Aeur.).

[2] Gottfried Schramm, Nordpontische Ströme. Namenphilologische Zugänge zur Frühzeit des europäischen Ostens (Göttingen 1973) 24–32 (im folgenden zitiert als: Str.).

[3] W. P. Schmid in: IF. 81 (1976) 435–444, im folgenden abgekürzt als Rez. Ich berücksichtige außerdem die — mir freundlicherweise erteilten — brieflichen Antworten auf einige Kontrollfragen zu Sm.s Rez. (zitiert als: br.) und seinen Beitrag „Die Ausbildung der Sprachgemeinschaften in Osteuropa" zum Handbuch der Geschichte Rußlands Bd. 1 (Stuttgart 1978) 104–121 (zitiert als: Ausb.).

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meine These beiseite, in der griechischen Namenüberlieferung habe sich eine kostbare Spur des Kimmerischen erhalten.

Wenn ich mich daranmache, Sm. auf den folgenden Seiten in allen wesentlichen Punkten zu widerlegen[4], dann in der Hoffnung, die Namenphilologen für den — bisher stiefmütterlich behandelten — Nordsaum des Schwarzmeeres zu interessieren, der aus dreifachem Grund ihre besondere Aufmerksamkeit verdient. Einmal erscheinen mir die ältesten Namenverhältnisse nirgendwo sonst in Europa über so weite Entfernungen hinweg ähnlich ungeklärt. Zum anderen läßt sich gerade hier die Alteuropa-Problematik auf ein neues Gleis schieben. Sm. hält die Untergliederung der weiten alteuropäischen Namenlandschaft erst für eine Forschungsaufgabe von übermorgen, weil es zunächst darauf ankomme, „diese Landschaft zu konstituieren und durch möglichst gründliche Materialsammlung abzusichern" (br.). Mir scheint es dagegen schon heute an der Zeit, die Eigenart der Teilräume zu diskutieren. Und schließlich empfiehlt sich der Nordpontos-Gürtel als Erprobungsfeld für eine Methodenverfeinerung. Über keinen anderen Teil Europas sind ja so viele Völkerstürme hinweggebraust. Nur wenig altes Namenerbe hat sie überdauert und das wenige trägt die Spuren einer Weitergabe von Volk zu Volk. Die Rekonstruktion von Namenstammbäumen wird dadurch erschwert, daß hier untergegangene Sprachen ins Spiel

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[4] Im Detail verdanke ich Sm. mehrere wertvolle Hinweise. So übernehme ich gerne, daß der Name des Pruth zu lit. *purvas* „Sumpf, Dreck", *purlyti* „vergießen" u. a. gehört (Rez. 438) und daß die Donau auf eine Urprägung *Dāneu̯io-* zurückgeführt werden sollte. Der *i*-Stamm, den ich — wohl wissend, daß er durch die Wortbildung überlieferter Sprachen nicht gedeckt wird — angesetzt hatte, erklärt sich aus meiner Orientierung an den überlieferten Nachfolgeformen Τάναις „Don", pannon. Δανούβης, rumän. *Dunăre* „Donau" und *Dъně-* in den slawischen Namen von Dnjestr und Dnjepr: Sie alle lassen im Gegensatz zu lat. *Dānuvius* an ihrem Ausgang kein *-o-*Element erkennen. Wie ich zugeben muß, besagt das aber noch nichts über die Urprägung. Ob es sich empfiehlt, darauf hin meine Ansätze **Dānowi-pros* und *-stros* in der Fuge zu ändern, mögen die Spezialisten beurteilen. — Wenn Sm. (Rez. 400) *-o-* in aruss. *Bogъ* „Südl. Bug" nicht mit Str. 8, 22 auf *-ъ-* zurückführen möchte, so finde ich meine Auffassung durch den Frühbeleg *Bъgъ* zu a. 922–39 im Komissionnyj Spisok der 1. Novgoroder Chronik bestätigt.

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kommen. Die Forschungsdiskussion dürfte in diesem Bereich nur vorankommen, wenn aufs genaueste geprüft wird, welche Argumente die Wahrscheinlichkeit für sich haben. Auf Beispiele von Argumentationen, in denen die Glaubwürdigkeit von Behauptungen gewogen werden soll, soll es mir im folgenden vor allem ankommen.

I.

Beginnen wir mit den iranischen Einsprengseln, die nach Sm. einen erheblichen Teil der nordpontischen Stromnamen ausmachen sollen. Wenn Abaev aruss. *D(ъ)něprъ* auf ein nordiran. **Dan-apra-* „Tiefffluß" zurückführte[5], dann hielt Sm. diese Deutung deshalb für schlagend, weil mit ihr ein perfekter Anschluß an Wortschatz und Kompositionsweise einer bekannten Sprache, nämlich des Ossetischen, gelungen schien. In diesem iranischen Idiom, in dem das Skytisch-Sarmatische fortlebt, finden sich die Appellative *don* „Fluß", *arf* „tief" und — als archaischer Restbestand — auch Zusammensetzungen mit vorangestelltem Determinatum[6]. Sm. (Aeur. 7) ging den von Abaev eingeschlagenen Weg weiter und stellte *D(ъ)něstrъ* als „Großfluß", altnordiran. **Dan-(ə)stura-*, zu oss. *don* und *ystyr*. Das Argument, daß die beiden Prägungen, lägen sie wirklich

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[5] V. I. Abaev, Istoriko-ėtimologičeskij' slovar' osetinskogo jazyka Bd. 1 (Moskau 1958) 63.

[6] Verfehlt scheint mir, wenn Sm. ossetische Bildungen wie *aefsūrğ* u. ä. „Aschenlösung" *āp* + *sūraka-* „Wasser" + „stark" Rez. 439 den gutindogermanischen, freilich in den Tochtersprachen unterschiedlich häufig vertretenen *armstrong*-Komposita wie anord. *hárfagr* „haarschön", griech. ποδώκης „fußschnell" zurechnet. Denn hier handelt es sich doch nicht um eine „durch ihre Wasser (oder hinsichtlich ihres Wassers) starke Flüssigkeit", sondern um ein „Starkwasser". Eindrücklich zeugt für die Natur der nordiranischen Kompositionsweise der Namenbeleg für die Stadt Theodosia auf der Krim, der sich beim Anonymus Periplus Ponti Euxini findet: Ἀρδαβδα τουτέστιν ἑπτάθεος. Zu Unrecht emendiert Vasmer, Schriften I 165 in *Αβδαρδα, wo es nichts zu emendieren gibt. Für *Ard-avda*, das zu osset. *ard* „Eid" (avest. *arəta-* „heiliges Recht") und *avd* „sieben" stimmt, liegt die Abwegigkeit einer Einordnung als *armstrong*-Kompositum wohl auf der Hand. Von den idg. Sprachen scheint mir nur das Albanische überzeugende Parallelen zu enthalten.

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zugrunde, eigentlich zu ganz anderen russischen Nachfolgelautungen hätten führen müssen, räumt Sm. durch eine Folge von Kunstgriffen aus. **Don-* (< **Dan-*), die regelgerechte slawische Wiedergabe des Anfangsliedes, hat nach Ausweis des litauischen FlNs *Doniepras* tatsächlich bestanden, ist dann aber — parallel zu iran. *sata* < slaw. *sъto* „100" — zu *Dъn-* > *Dn-* geworden. Weil es das Gegenstück *sъto* gibt, macht das Verhältnis zwischen *Dan-* und slawisch *Dъn-* dem leicht zufriedenstellenden Sm. „keine Schwierigkeit"(!). Und wenn sich in *Dъněstrъ*, *Dъněprъ ein -ě-* ergeben konnte, das doch durch die angeblichen iranischen Vorformen in keiner Weise nahegelegt wurde, dann weiß Sm. auch hier Rat: Die Slawen haben eben die beiden Lehnflußnamen mit ihrer Vokabel *dunaj* kontaminiert (Ausbild. 117). Dabei wird — wider alle Wahrscheinlichkeit — unterstellt, *dunaj* sei nicht etwa der zum Appellativ umfunktionierte Donau-Name, sondern ein gutslawisches Erbwort. In einem Zirkelschluß, der schmunzeln macht, erweist sich Sm.s Prämisse flugs deshalb als richtig, weil sie den Zweck, dem sie dienen soll, auch tatsächlich erfüllt: „Daraus ergibt sich, daß das Slavische schon vor Übernahme des Donau-Namens ein *dunaj*' gekannt haben muß, was dessen Bedeutung: unter anderem auch 'Pfütze, Bach' wahrscheinlich macht"[7].

Ich halte die Denkschritte, mit denen Sm. die vermeintlichen iranischen Ursprünge mit den überlieferten Formen harmonisieren möchte, für samt und sonders brüchige ad-hoc-Konstruktionen. *Doniepras*, das uns eine ältere slawische Entwicklungsstufe verraten soll, ist willkürlich aus einer ganzen Reihe von litauischen Zeugnissen herausgepflückt, in denen der Dnjeprname auf kleinere Flüsse und Wiesen übertragen erscheint. Die

[7] Die Versuche von Karol Zierhoffer, Nazwy miejscowe północnego Mazowsza (Breslau 1957) 162f. und Marian Jurkowski, Ukraińska terminologia hydrograficzna (Breslau . . . 1971) 22f., *dunaj* als genuinslawische Bildung (nach Z.: eine vorslawische Ableitung von **dheu/dhou-* „fließen, laufen") zu erklären, überzeugen mich nicht. Daß Namen bedeutender Ströme für Bäche und Flüsse wiederverwendet werden, ja daß sie — besonders in weiterem Abstand on dem betreffenden Strom — zur Bezeichnung von „Fluß, Bach, Pfütze" dienen, läßt sich so vielfach belegen, daß eine andersartige Herleitung von *dunaj* unnötig erscheint.

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übrigen Belege aus Litauen zeigen durchweg *Dune-*[8]. Danach wird der Einzelgänger *Done-* nichts als eine Entgleisung sein, wie sie für Fernexporte von Namen mit ungewöhnlicher Lautung und zudem bei Flußnamen mit kurzem Bekanntheitsradius geradezu typisch sind. Oder sollten wir nun etwa aus dem immerhin viermal neben achtmaligem *Dunajus* bezeugten lit. *Dunojus*[9] folgern, die slawische Vorform von *Dunaj* müsse **Dunoj* gelautet haben? Wie es zu der Lautung *sъto* „100" kam, ist bis heute umstritten geblieben. Wollte man mit Sm. durch dieses eine Wort eine russische Sonderentwicklung von *o* > *ъ* belegen, dann könnte man von Stund an jede beliebige Sonderentwicklung plausibel machen: irgendein Einzelbeispiel wird sich im XYZ-Archiv der ungelösten Etymologien schon als Stütze anbieten. Und wie sollte man glauben, die Slawen hätten Dnjestr und Dnjepr auf *dunaj* ausgerichtet, wo der Vokalismus von *Dunaj* und Sm.s Rekonstruktion **Donĕ-* doch weit auseinanderklaffen? Die letzte der Ungereimtheiten hat Sm. noch nicht einmal einer Erklärung für wert gehalten: Wie konnte in jener gotischen Nachfolgeform, die von *Danastrus* bei Ammianus Marcellinus und byz. Δάναστρις gespiegelt wird, das *-u-* von **Dan(ə)stura-* spurlos abhanden kommen? (Meine Antwort: weil es dieses *-u-* niemals gegeben hat).

In genau umgekehrter Richtung wie Sm. muß man vorgehen. Statt die beiden auffälligsten Schwierigkeiten von *D(ъ)nĕstrъ* und *D(ъ)nĕprъ*, und das in dieser Position ganz ungewöhnliche *ĕ*, durch Kunstgriffe zu entschärfen, gilt es, gerade diese Laute zu Angelpunkten einer Neuinterpretation zu machen. Aus dieser Einsicht ist meine eigene Deutung herausgewachsen, die in den Belieferern der Slawen keine Iraner, sondern ein untergegangenes, altbodenständiges Volk der ukrainischen Waldsteppe sieht. Seine Sprache hatte, wie ich annehme, teil an einem Wandel von *ā* zu *o*, dessen erste Etappe *ā* > *ō* sich für weit auseinander liegende Punkte des karpatisch-balkanischen Bereiches nachweisen läßt und somit offenbar für einen Großraum typisch

[8] Die litauischen Formen entnehme ich dem Aufsatz von A. Vanagas, Lietuvių kalbotyros klausimai 8 (1966) 173–184, auf den sich auch Sm. stützt. Die Übersetzung verdanke ich Petras Nevulis.

[9] Ebd. 178f.

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war (Für Norddakien ist auch die Fortentwicklung zu *o* belegt, wenn Δοκίδαυα bei Ptolemaios III 8,4 wirklich „Dakerort" bedeutet). Das rätselhafte slaw. *-ě-* < *-ai-* führe ich über einen ebenfalls südosteuropäisch belegbaren Wandel auf *-aŭi-* zurück. Damit wird es zum Indiz, daß die Anfänge beider Namen ursprünglich mit der sowohl Donau wie Don zugrundeliegenden Prägung übereinstimmten.

Zu meinem Schluß, daß Abaev und Sm. Unrecht haben, kann man übrigens auf einem viel kürzeren Wege gelangen. Hätten die Skythen oder Sarmaten wirklich — zu einem Zeitpunkt, der nach Sm.s Präzisierung (Ausb. 117) vor ihre *-pr-* > *-rf-* Metathese fallen müßte — die beiden Ströme Dnjestr und Dnjepr umbenannt, dann wären in den Gebrauch des von Osten kommenden Türkvolks der Petschenegen, das wir im 10. Jh. in der ukrainischen Steppe antreffen, doch wohl die neuen Namen eingegangen. Statt dessen verwendeten die Petschenegen zwei Lautungen, die griechisch als Τροῦλλος, Βαρούχ wiedergegeben werden und mit großer Wahrscheinlichkeit die beiden uralten, von den Skythen bereits vorgefundenen Prägungen fortsetzen. Die These von der Umbenennung geht also an einer unserer wichtigsten Quellen, *De administrando imperio* von Kaiser Konstantin Porphyrogennetos, achtlos vorbei. Dagegen ist meine Alternative, daß *Dъněstrъ* und *Dъněprъ* am Waldsteppenabschnitt von Dnjestr und Dnjepr altbodenständig sind und sich im Zuge von Namenausgleichvorgängen schließlich bis zur Quelle und bis zur Mündung vorgeschoben haben[10], durch ein reiches Vergleichsmaterial gedeckt. Eine Umtaufe, wie Abaev und Sm. die annehmen, fand bei Strömen nur äußerst selten statt. Einen Namenausgleich zwischen verschiedenen Laufabschnitten muß man dagegen für jeden Strom annehmen, nur daß dieser Prozeß in den meisten Fällen bei Einsetzen unserer Überlieferung bereits abgeschlossen war.

[10] Der Satz: „Will man auf Abschnittsnamen hinaus, muß man die Gleichzeitigkeit beweisen können" (Rez. 439), geht in die Irre. Wie sollte man für Namen, die tief in vorhistorische Zeit zurückreichen, beweisen, ob sie gleichzeitig oder sukzessive entstanden sind? Beweisen kann man aber, daß die Alternative von Umbenennungen durch Iraner sich nicht mit der Überlieferung verträgt, was mittelbar für Abschnittsnamen spricht.

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Nun schreibt mir Sm., daß *Dъněstrъ* und *Dъněprъ*, die sich doch nicht anders, denn als Komposita deuten lassen, eben durch ihre Bauweise als jüngere Prägungen ausgewiesen würden. Im alteuropäischen Stratum, das gerade durch unkomponierte Bildungen gekennzeichnet werde, sei für sie kein Platz. Das kommt davon, wenn man sich „Alteuropa" als uniformierte Archaik vorstellt. Warum sollte in einer Teillandschaft und noch dazu in einer Randregion, die vielleicht viel später als Zentraleuropa indogermanisiert wurde, nicht auch einmal ein komponierter Flußname verliehen worden sein? Ein Name jüngeren Typs muß eben nicht, wie Sm. offensichtlich unterstellt, in jedem Fall später gegeben sein als ein Name älteren Typs. (Wer wollte etwas aus der bloßen Tatsache, daß drei Kinder Johannes, Karl und Che Guevara heißen, ihr Altersverhältnis erschließen?) Mit aller Wahrscheinlichkeit haben jene Völker, die den alteuropäischen Namenschatz ausbreiteten, bei ihrer über weite Räume und lange Zeiten verteilten Benennungsakten gleichzeitig mit solchen Stereotypen, die sie einem ererbten Reservoir entnahmen, auch Neuprägungen verliehen, um die sie selber den traditionellen Vorrat bereicherten. Es ist, meine ich, an der Zeit, sich die Entstehung der frühen Hydronomie konkreter, geschichtlicher und damit vielfarbiger vorzustellen als das — Sm. einmal als repräsentativ genommen — bislang üblich war.

Nun hat Sm. Aeur. 7 auch den Donjez und in der Rez. 440 noch dazu den Κοῦφις „Südl. Bug." und den Κωφήν „Kuban" als iranische Neubenennungen in Anspruch genommen. Das ist für *Donec* natürlich abwegig, da es sich um eine gut slawische Deminutiv-Ableitung vom Lehnnamen *Don* handelt, die älteres, aus voriranischem Erbe stammendes Ὕργις ablöste. Für die beiden anderen Namen gibt Sm. folgende Begründung: „Sicher ist wohl, daß man Κοῦφις, Κωφήν mit einer iranischen Etymologie ausstatten muß, da ein Zusammenhang mit dem Kabulfluß (aind. *Kubhā*) kaum zu bestreiten ist." Hier häufen sich die Schnitzer. *Kubhā* im Rigveda kann gar nicht den Kabulfluß meinen, da dessen altgriechischer Name Κωφήν mitteliran. *kōfēn* „bergig" widerspiegelt[11]. Wenn Menander Protektor den Kuban

[11] *Wilhelm Eilers/Manfred Mayrhofer, Die Sprache 6 (1960) 11–113.*

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ebenfalls als Κωφήν aufführt, dann sollte man diese Lautung besser nicht etymologisieren, weil es sich um eine Bildungsassoziation handelt: der frühbyzantinische Autor hat das authentische *Kuban-* an den aus der Alexandertradition geläufigen Kabulfluß angeglichen. Κοῦφις hängt in der Tat etymologisch mit *Kubhā* zusammen. Aber das spricht schon allein deshalb nicht für eine iranische Etymologie, weil **kŭbh-* „gebogen" kein arisches Sondergut, sondern indogermanisches Gemeingut ist.

Das Fazit: Sm.s Annahme von iranischen Einsprengseln im nordpontischen Stromnamenschatz hält in keinem einzigen Falle der Nachprüfung stand. Daß es — wenn auch in wesentlich geringerer Zahl als bislang behauptet — unter den kleineren Gewässern, die ja viel häufiger umgetauft zu werden pflegen als Ströme, iranische Neubenennungen bis hinauf an den Rand des Mischwaldes geben dürfte, ist ein anderes Thema, das hier nicht zur Debatte steht.

II.

Meinen Nachweis, das älteste Namenstratum des nordpontischen Gürtels gehöre zu Alteuropa, habe ich durch eine Argumentation zu verfeinern unternommen, die den besonderen Ort der neukonstituierten Landschaft im alteuropäischen Großzusammenhang bestimmen sollte. Wenn die längsten Flüsse dieses Raumes in der ältesten Namenschicht durchweg maskulin waren, während in der zweiten Größenordnung auch die Feminina *Vorskla, Sula* und *Roś* vorkamen, dann verrät sich darin ein klarer Unterschied zur baltisch-slawischen, aber auch zur indo-iranischen Hydronymie. Zugleich ergibt sich — wie ich heute bereits genauer belegen könnte als 1973 — eine wichtige Übereinstimmung mit Südosteuropa (Str. 26f.). Auf ein spezifisch nordpontisches Merkmal, die viermalige Verwendung von *dānu-* „Fluß" in Stromnamen, wird gleich zurückzukommen sein. Zu solchen Aussagen habe ich nun, wie Sm. (Rez. 436) mir vorhält, nur kommen können, weil ich mit meiner Beschränkung auf die neun längsten Flüsse methodisch verfehlte Vorentscheidungen getroffen habe, die meine Ergebnisse „nicht unerheblich beein-

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flussen". Dieser Vorwurf wäre berechtigt, wenn ihn Sm. an die eigene Adresse gerichtet hätte. Denn seine Behauptung von 1966, das älteste nordpontische Stratum sei iranisch, basierte ja auf der Makrohydronomie: kleinere Gewässer wurden nur zur Grenzabsteckung nach Norden herangezogen (Aeur. 7f.). Mich dagegen trifft der vorgebrachte Einwand nicht, denn in meine Behauptung, lediglich die größten Flüsse seien durchweg maskulin gewesen, muß ja wohl auch eine Kenntnis der übrigen Flußnamen eingeflossen sein. Daß es aus dem Namenbestand der kleineren Flüsse noch manches nachzutragen gibt, glaube ich gerne. Allerdings will mir nach Lektüre einer Studie von Trubačev[12] scheinen, daß sich im Hinterland des nördlichen Schwarzmeeres die ursprünglichen Verhältnisse nur sehr unvollkommen freilegen lassen. Denn das alte Erbe dürfte in einem weniger resistenten Material als dem Stromnamenschatz so weit verunklärt sein, daß Rekonstruktionen weitgehend auf morscher Grundlage aufbauen müssen. Vermutlich werden sich noch manche Namenwortübereinstimmungen mit dem Litauischen aufzeigen lassen, an denen Sm. so viel liegt. Aber ich sehe nicht, welche meiner Aussagen, die ihren Gegenstand ja nicht erschöpfen, sondern nur erschließen wollten, durch eine gründlichere Durchforstung des Gesamtmaterials erschüttert werden könnte. Wenn Sm. mir vorhält, meine „histrodanuvischen" Sondermerkmale seien nur die Konsequenz aus einer willkürlichen Auswahl (Rez. 436), dann hat er das nicht bewiesen und wird es auch nicht beweisen können.

Doch nun zu *Dānu-*, das mir als wichtiger Schlüssel zur Sonderart der nordpontischen Namenlandschaft erschien, weil dieses Etymon nördlich des Schwarzen Meeres in den Namen von immerhin vier unter den sechs längsten Strömen vorkommt, während es der ältesten Hydronomie des übrigen Europas vielleicht völlig abging. (Das in England mehrfach begegnende *Donwy*, *Don* u. ä. geht vermutlich auf einen Import von der Donau zurück).[13] Nein, meint Sm. (Rez. 437): *dānu-* sei ein gemeinalteuropäisches Namenwort, denn es kehre ja noch im

[12] O. N. Trubačev, Nazvanija rek Pravoberežnoj Ukrainy. Étimologija. Ètničeskaja interpretacija (Moskau 1968).

[13] Vgl. Wilhelm Nicolaisen, BzN. 8 (1957) 246.

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poln. *Tanew* (→ San) wieder. Mich hat längst gewundert, wie ein Philologe vom Range Vasmers die *Tanew* als eine am Anfang lautverschobene und daher aus dem Germanischen entlehnte Dublette zum Donaunamen verstehen konnte, wo doch mndt. *Dōnowe* zeigt, daß der *ā* > *-ō-* Wandel im Germanischen der *D-* > *T*-Verschiebung vorausging. Ein *Tāneu̯-* mit *T* < *D* aber mit erhaltenem *ā* ansetzen heißt danach eine germanische Unform konstruieren. Um so erstaunlicher, daß Sm. diese verfehlte Deutung wieder aufwärmt, nachdem längst bekannt ist, daß die — von Sm. zum Maskulinum uminterpretierte — genuinslawische *Tanew* mit der Donau nicht das geringste zu tun hat[14]. *Dānu-*, so möchte ich wiederholen, gehört mit aller Wahrscheinlichkeit nicht zu den gemein-alteuropäischen Flußnamenbausteinen. Es darf vielmehr als das wichtigste Leitfossil unter den Namenwörtern der ältesten nordpontischen Hydronymie gelten. Für die Eigenständigkeit der nordpontischen Namenlandschaft innerhalb Alteuropas hat es Signalwert.

III.

Sm.s besonderen Unmut zog ich mir mit der Behauptung zu, im Streifen zwischen Donau und Don habe sich ein kunstvoll gebautes und kunstvoll verklammertes System von Paarbindungen entfaltet: *Donau-Don*, *Sereth-Pruth*, *Dnjestr-Dnjepr*. Dieser Unmut hat sich, wie es scheint, an einem Mißverständnis entzündet. Nach der Rez. 437f. gibt es eine ganze Reihe von Flußnamen außerhalb meines angeblichen Systems, die es verböten, „von einer Paarbindung zu sprechen": der *Tanew*, der Dnjestrnebenfluß *Seret* und andere weitverstreute Bildungen auf *-etos*. Kopfschüttelnd muß ich feststellen, daß Sm. eine Paarbindung nur da anerkennen will, wo es sich um ein exklusives Doppel handelt. Wo immer eine Namenprägung, ja selbst ein

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[14] Das *-a-* ist in die Nom.-Form *Tanew* (statt älterem *Tnew*) erst durch Paradigmaausgleich mit dem Gen. *Tanwi* und den übrigen Kasus geraten. Zugrunde liegt *Tъn-*, s. J. Rieger in: Slavia orientalis 14 (1965) S. 363–365.

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Suffix auch anderswo wiederbegegnet, ist die Hypothese einer Paarbindung widerlegt. Welch absonderliches Vokabelverständnis! Für mich liegen, wie schon Str. 4.1. präzisiert, „Paarbindungen" überall dort vor, wo sich in der Gleichheit oder Ähnlichkeit von Namengebilden die Absicht der Benenner verrät, zwei Örtlichkeiten mit sprachlichen Mitteln aufeinander zu beziehen. Natürlich können Namengebilde oder ihre Bausteine, die irgendwo „gebunden" auftreten, anderswo „ungebunden" wiederbegegnen. Von „Paaren" zu scheiden sind die — von Sm. damit verwechselten — „Dubletten": etwa die beiden erwähnten *Serets* oder *Isar, Iser* und *Isère*. Bei ihnen war kein Brückenschlag, keine Paarigkeit als Ausdruck von Zusammengehörigkeit intendiert. Wer derartige Dubletten gegen meine Paarigkeits-Thesen ins Feld führt, stößt ins Leere.

Nun hat Sm. mein Paar aus *Sereth* und *Pruth* noch mit einem weiteren Einwand angegriffen: ich hätte für den Pruth einen falschen Stammbaum aufgestellt (Rez. 438). Schon die frühesten Zeugnisse für beide Stromnamen lauten Σέρετος und Πυρετός. Jeder Unvoreingenommene wird mir wohl darin folgen können, daß *Puretos* und *Seretos* zwei paarige Namen der ältesten Schicht waren, deren Klangparallelität der Laufparallelität zweier benachbarter Ströme ebenso Rechnung trug, wie das für *Dnjestr* und *Dnjepr* oder, weiter im Westen, für *Sava* und *Drava* bis zum heutigen Tag auf der Hand liegt. Schwierigkeiten macht einzig jener slaw. *Prut* < **Pərǫto-*, dessen Spur erst um a. 950, anderthalb Jahrtausende nach dem Erstbeleg Πυρετός bei Herodot, einsetzt. Ich rechne mit einer slawischen Entgleisung, die aus einer sekundären Paarbindung mit dem *Reut* < **Revǫto-*, einem Nebenfluß des Dnjestrs und Parallelfluß zum Pruth, herrührt. Diesen Ansatz hält Sm. für „nicht möglich" (Rez. 438). Denn das Slawische könne mit *-u-* < *-ǫt-* sehr wohl die ursprüngliche Form des Suffixes, nämlich *-ont-* spiegeln, während Πόρατα (laut Herodot die skythische Entsprechung zu Πυρετός und von mir als regelrechte Umsetzung von *Puretos* zu iran. **Purata-* erklärt) auf eine Variante mit *-ǫt-* zurückgehe. Πυρετός mit seinem -ε-, das weder zum Ansatz *-ont-* noch *-nt-* stimmt, soll nach Sm.s brieflicher Erläuterung eine griechische Anlehnung an πυρετός „Fieber" sein. Daraus folgt, daß ich einer akustischen Täuschung

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aufgesessen bin. Die Suffix-Übereinstimmung zwischen Σέρετος und Πυρετός erweist sich als sekundär und unerheblich, ja sie kam wohl nur bei den Griechen und nicht im Gebrauch der Autochthonen vor. Die Klangparallelität der Urprägungen, die bereits für *Dъněstrъ* und *Dъněprъ* weginterpretiert wurde, verfällt hier — wiederum dem klaren Zeugnis der Erstbelege zuwider — ein zweites Mal dem Stift eines paarfeindlichen Philologen. Natürlich kann ich nicht mit der Retourkutsche kontern, Sm.s verästelter Stammbaum sei „nicht möglich". Möglich ist vieles. Aber sein Ansatz erscheint mir wesentlich komplizierter, wesentlich ballastbeschwerter als meiner. Ich halte ihn, kurz, für falsch.

Wer mir, gegen Sm.s Einspruch, abnimmt, daß vier parallele Zuflüsse des Nordschwarzmeeres, nämlich Sereth, Pruth und Dnjestr, Dnjepr zwei Namenpaare trugen, ja, daß sie — diesmal auch in Sm.s Sinne — von zwei gleichnamigen Strömen, der Donau und dem Don, eingerahmt wurden, der wird sich wohl kaum mehr gegen die in der Tat kühne Annahme sperren, die Urprägungen der rätselhaften Namen *Dnjestr* und *Dnjepr* seien aus den Namen von *Donau-Don* als den Außenachsen und zwei aus *Seretos* und *Puretos* abstrahierten Elementen *sro-* und *pro-* zusammengesetzt worden. Diese Konstruktion, die ich als „danuvisches Namensystem" etikettiert habe, fällt aus den Normen der uns bekannten Namengebungen und zugleich aus der Typologie der indogermanischen Nominalkomposition weit heraus. Und das ist zunächst einmal ein gewichtiger Einwand gegen die Richtigkeit meiner These. Aber ich konnte die Einzigartigkeit des Befundes, den ich glaube, freigelegt zu haben, mittlerweile durch den Nachweis relativieren, daß die Urprägungen für die Theiß samt ihren Hauptzuflüssen und dem benachbarten Temesch einen uralten Fünferverbund bilden, der sich als Vorstufe zum „danuvischen Namensystem" begreifen läßt. Geographisch zusammengehörige Ströme onomastisch aufeinander zu beziehen, hebt sich innerhalb „Alteuropas" nunmehr als Spezifikum eines geographischen Streifens heraus, der sich von Save und Drau im Westen bis zum Don im Osten erstreckt[15].

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[15] G. Schramm, Der Flußnamenverbund von Theiß und Temesch, BzN. N. F. 10, 4 (1975) 60–90.

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IV.

Nicht einmal als „Anregung" will die Rez. 441 meine Gedanken über Τάναϊς „Don" und Ὕπανις „Kuban" gelten lassen[16]. Worum geht es? Die beiden antikgriechischen Formen nahm ich Str. 11.1.–16.36. mit griech. Κιμμέριος: assyr. *Gamirrā* zusammen und verstand die drei hellenischen Lautungen als Spiegel einer Sprache mit Konsonantenverschiebungen und $\bar{a} > a$-Wandel. Wahrscheinlich seien es die Kimmerier gewesen, die den Grie-

[16] Auch in den beiden einzigen sowjetischen Stellungnahmen zu meinem Buch, von denen ich Kenntnis erhielt, wird meine Kimmerier-These abgelehnt: Die Kimmerier seien eher ein Zweig der Thraker als Vorfahren der Armenier gewesen. O. N. Trubačev, Voprosy jazykoznanija Nr. 4 (1976) 40f. will das durch den makedonisch-thrakischen Charakter der einzigen uns erhaltenen Kimmerier-Vokabel ἄργιλλαι „Wohnhöhlen" bei Strabon V 4,5, und durch die thrakischen Namen erhärten, die sich für eine Reihe von Königen des Bosporanischen Reiches nachweisen lassen. G. B. Džaukjan, Istoriko-filologičeskij žurnal Nr. 4 (Erevan 1977) 73–82, dessen Ausführungen mir Hans W. Poll übersetzte, führt darüber hinaus als Indiz für eine ethnische Zugehörigkeit der Kimmerier noch an, daß ihr Name eine thrakische Ableitung von **Kir(s)mar-io-* „Schwarzmeer" sein könne (78). Was ließe sich mit derartigen Methoden alles beweisen! Wenn ein einziges, seiner Bedeutung nach besonders entlehnungsverdächtiges Wort wie ἄργιλλαι, das als zufälliger Rest von einer ganzen Sprache übriggeblieben ist, schon ausreichen würde, deren Stammbaum zu klären, dann würde die rumänische Entsprechung zu ἄργιλλαι, das Substratwort *argeá* „Erdhütte zum Weben", falls es heute das Rumänische mit der gleichen Zufälligkeit allein repräsentieren würde, *für* den altbalkanischen und *gegen* den romanischen Charakter dieser Sprache zeugen. Die Rußlandgoten könnten keine Ostgermanen gewesen sein, weil in der Ukraine nach ihrem Abzug andere als gerade ostgermanische Namen verwendet wurden. Džaukjans etymologischer Einfall scheint mir verfehlt, weil er am *G*-Anlaut von assyr. *Gamirrā* vorbeigeht. Auf jeden Fall sollte man Etymologien, die sich in dieser Güteklasse zu Dutzenden aufstellen lassen, nicht — statt ihre Richtigkeit, soweit möglich, zu beweisen — als Beweismittel für vorgefaßte Ansichten mißbrauchen. Von gründlichem Unverständnis für meine Argumentation und zugleich von ungenauer Lektüre zeugt D.s Bemerkung a.a.O.: „Kennzeichnend ist, daß Schramm nicht einmal den Versuch unternimmt, die kimmerischen Sprachreste mit einer armenischen Etymologie zu versehen, und daß er nicht beachtet, daß der Konsonantenwechsel nicht nur dem Armenischen eigen ist". Für *Tanais* und *Hypanis* konnte und wollte ich den angemahnten Versuch gar nicht unternehmen, da ich den Kimmeriern nur die Weitervermittlung und nicht

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chen diese Namen weitergereicht hätten. Und den drei Lehnlautungen lasse sich ablesen, daß dieses Volk sehr wohl als Vorfahren der Armenier in Frage komme. Der Klotz am Bein, der Sm. Rez. 441 daran hindert, diesen „Gedankenflügen" zu folgen, setzt sich aus den folgenden Argumenten zusammen: a) Τάναϊς mit seinem von russ. < iran. *Don* abweichenden *T-* könne durch ταναός „langgestreckt" beeinflußt sein (Rez. 437); b) der Hypanis gehöre offensichtlich mit baltischen Flußnamen auf *Up-* zusammen; c) wenn Τάναϊς in dem *ă* seiner Anfangssilbe von aind.-avest. *dānu-* (mit *ā*) abweiche, dann nur deshalb, weil der ossetische Lautwandel von *ān* > *an* (> *on*) hier schon vollzogen sei; d) *Kubań* muß eine iranische Neubenennung sein (Rez. 440). (Wer, wie Sm. verschwenderisch mit Umtaufen rechnet, kommt um Probleme, aber auch um Entdeckungen herum). Sein Fazit: Die Klangähnlichkeit, die mir zwischen *Hypanis* und *Kubań*, dem heutigen Namen des gleichen Stroms, doch ohrenfällig zu sein scheint, ist (ein weiteres Mal!) nur zufällig und die Diskrepanz zwischen *D-* und *T-* in der Überlieferung des Dons historisch unergiebig.

Die Alternative, die Sm. damit anbietet, bleibt, so möchte ich meinen, unbefriedigend. Denn sie trägt folgenden drei Befunden keine Rechnung:

1. Die *H-*/*K*-Relation von *Hypanis*/*Kubań* wiederholt sich 500 km weiter westlich in *Hypanis*, wie in der Antike ja auch der südliche Bug hieß, und dessen mittelalterlicher Benennung Κοῦφις. Er wollte glauben, daß die Iraner zwei gleiche (oder ähnliche) Namen mit *Up-*, die sie in ganz verschiedenen Stromgebieten antrafen, hier wie dort durch Neubenennungen mit ein und derselben Basis *Kub-* ersetzt haben sollen, die mit *Up-* weder sinnverwandt noch (eindeutig) klangähnlich ist?
2. Die Konsonantenverhältnisse innerhalb des Quadrates, zu dem sich *Tanais* = *Don* und *Hypanis* = *Kuban* anordnen

etwa die Schöpfung der beiden Namen zugeschrieben habe. Die außerarmenischen Konsonantenverschiebungen wurden Str. 16.1–10 ausführlich diskutiert und lediglich die Koppelung von Konsonantenverschiebung und Egalisierung der Vokalquantität als ein armenisches Spezifikum in Anspruch genommen.

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lassen, zeigen einen Parallelismus, den Sm. als reinen Zufall beiseite schiebt. So viel Zufall ist nicht wahrscheinlich.

3. Wenn nordiran. *dānu-* „Fluß" sich schon zu Herodots Zeiten zu *danu-* entwickelt hätte, dann bliebe unverständlich, wieso das Ossetische vor *n* lediglich ein ehemaliges *ā* (z. B. in *don* < *dānu-* „Fluß", *bon* < *bānu-* „Tag") zu *o* wandelte, während altes *ă*, mit dem *ā* so früh egalisiert worden sein soll, in der gleichen Position als *a* oder *ä* fortlebt.

V.

Mittlerweile dürfte hinter einer Sachauseinandersetzung der Unterschied im Zugriff deutlich geworden sein, der die beiden Debattierer trennt. Sm. geht es primär um die Freilegung von Entstehungsebenen (wie einer alteuropäischen Grundschicht und iranischen Einsprengseln) und beim Ansatz der ursprünglichen Prägungen um „glatte Etymologien". Wie die Interpretation von *Dnjestr* und *Dnjepr* wohl am eindrücklichsten zeigen: sobald er diese gefunden zu haben glaubt, ist er bereit, selbst mit den außergewöhnlichsten Verwitterungen und Verwerfungen zu rechnen, die ihm den weiten Abstand zwischen den erschlossenen und den bezeugten Formen überbrücken. In einem Fall führt er Formen, die unverkennbar in ein und denselben Stammbaum gehören, auf ganz verschiedene Prägungen zurück[17]. Nach meinem Urteil überschätzt er die Variabilität des Materials, das er untersucht, auch darin, daß er ungehemmt mit Umnennungen von Strömen durch eine landnehmende Neubevölkerung rechnet.

Mein Zugang ist ein anderer. Ich bin ein (philologisch ausgebildeter) Historiker und interessiere mich für die Verwitterungen und Verwerfungen, die Namen durchgemacht haben, noch mehr als für ihre Ausgangsformen, die nur dann und wann etwas für die Historie abwerfen. (Das befreit mich natürlich nicht von der Verpflichtung, mit Urprägungen zu arbeiten, die einer sprachwissenschaftlichen Kritik standhalten). Mein Hauptanliegen ist es, in das Feld der Namengeschichte, das bisher gegenüber der vieldiskutierten Namenentstehung vernachlässigt wurde, mehr Ordnung, mehr Methodik, mehr Komparatistik hin-

[17] So Oswald Szemerényi, dem ich das Problem vorlegte.

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einzubringen, damit meine Disziplin, die Geschichte, etwas aus diesem aufschlußreichen Überlieferungszweig lernen kann. Der Umgang mit Stromnamen hat mir immer von neuem bestätigt, daß sie in einer vieltausendjährigen Geschichte Europas nur selten ausgewechselt wurden. Damit dürfte von vornherein die Wahrscheinlichkeit gegen sich haben, wer so verschwenderisch mit Neubenennungen rechnet wie Sm. Ich machte weiterhin die Erfahrung, daß die Makrohydronomie auch in anderer Hinsicht erstaunlich konservativ ist. Wenn man in Lehnstromnamen heimische Appellativa wie *dunaj* „hineingelesen" haben soll, so bleibt das ohne überzeugende Parallele unter den gesicherten Fällen. Eine — etwa auf die Slawen schon nicht mehr übertragbare — Rolle spielten jene Griechen, denen der Südliche Bug und der Dnjepr zunächst nur aus Zwischenlandungen bei der Küstenfahrt vertraut wurden. Dieser verhältnismäßig lockere Anfangskontakt, der sich auf die Mündungen beschränkte, erklärt gewiß ein eigentümliches, und — wie mir scheinen will — einzigartiges „Nest" von Unregelmäßigkeiten: Griech. Ὕπανις „Südl. Bug" hat die — aus byzant. Κοῦφις, petschen. < slaw. Κουβοῦ erschließbare — bodenständige Nachfolgeform von *Kubh-*, einem anderen Namen mit gleicher Basis, aber anderem Suffix gleichgemacht: nämlich dem *Kuban*, dessen kimmerische Entsprechung nach meinem Ansatz **Khupani-* > griech. Ὕπανις lautete. Der Βορυσθένης „Dnjepr" wurde nicht nur den PNen auf -σθένης eingepaßt, sondern offenbar auch — dem Gebrauch der Altanwohner zuwider — vom Mündungslimaen auf den gesamten Strom übertragen (Str. 8. 20f.; 15. 3–5.; 8. 24–27.)[18]. Aber selbst in dieser engabgesteckten Region, wo sich gleich zwei Umprägungen finden, wurde nicht etwa

[18] Ein Stammbaum wird zerhackt, wenn Sm. Rez. 441 meine Annahme gotischer Vermittlung von Ῥῶς „Wolga" bei Agathemeros bestreitet: die Lautung lasse sich auch iranisch erklären und zwar, wie er brieflich erläutert, aus **R(a)uxša-*. Für antik griech. Ῥᾶ „Wolga" rechnet er mit einer ganz anderen iranischen Vorform: **Rava(n)-* oder **Rahā*. Dabei müssen doch Ῥᾶ und Ῥῶς einen gemeinsamen Stammvater haben. Ich entscheide mich jetzt für **Rava-*, weil es avest. *ravan-* „Fluß" gibt und die Wolga mordw. *Rav(o) Rav* heißt. Die Alternative *Rahā*, die ich aus der Literatur übernahm und die Sm. weiterhin für möglich hält, wird man fallen lassen dürfen.

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mehr Verständlichkeit erreicht, um die es bei Stromnamen-Rezeptionen fast nie geht. Wir haben es vielmehr mit einer „sinnlosen" Klanganpassung (an ein PN-Endglied -σθένης) und vor allem mit Nachbarschaftseinflüssen (vom Kuban auf den Südl. Bug sowie von der Mündung des Dnjeprs auf seinen Stromlauf) zu tun. Wer nicht herausgegriffene Einzelnamen „hintrimmt", sondern das gesamte Vergleichsmaterial im Auge behält, wird kaum mit Sm. annehmen wollen, die Griechen hätten sich ein iran. **Danai-* als ταναός zurechtgestutzt, weil sie eine Vokabel mit der Bedeutung „langgestreckt" hineininterpretierten. Ein Volk, das Sagen über die Danaer erzählte und sich in poetischer Sprache selber diesen Namen beilegte, hatte keinen Anlaß, die wohlvertraute Lautung *Danai-* umzuprägen.

Wenn ich den Grund für *D-* statt zu erwartendem *T-* statt dessen in einer Zwischenschaltung des Kimmerischen in jenem Weitergabevorgang gesucht habe, der zu den griechischen Formen führte, dann erst nach langem Umgang mit anderen Stromnamen, die mich lehrten, daß ihre Lautungen gerade dort, wo sie zunächst regelwidrig, sperrig und spröde anmuten, belastbar und für den Historiker aufschlußreich sind. Denn im Unterschied zu Orts-, Flur- und Gewässernamen mit kurzem Bekanntheitsradius, für die Entgleisungen und Umprägungen geradezu typisch sind, erweisen sich Stromnamen bis ins lautliche Detail hinein als beharrsam. „Mutationen", die von einzelnen Benutzern eingeführt wurden, hatten zumeist wenig Chancen, sich durchzusetzen, weil sie durch die große Zahl der übrigen Benutzer rasch wieder rückgängig gemacht zu werden pflegten. Eine „Typologie der Namenveränderung" gedenke ich einem Buch vorauszuschicken, das ich über das Fortleben von antikem Namenerbe in Südosteuropa vorbereite. Der Namengeschichte, die neben den vielerforschten Namenetymologien einen Gegenstand zu eigenem Recht darstellt und mit einer eigenen Methodik aufgehellt werden muß, möchte die verstärkte Aufmerksamkeit der Wissenschaft wünschen.

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A proposito dell'ordine delle „parole" nella lingua del Rigveda

Immagini in cui la "realtà" viene più o meno sensibilmente trasfigurata dalla fantasia creativa poetica, costituiscono il semantismo testuale (= contenuto) sul quale il poeta opera l'interpretazione fonologica (= espressione). La scelta di una certa interpretazione fonologica (tra quelle possibili per un determinato semantismo) e l'ordine delle macroentità (= parole) possono dipendere dall'atmosfera musicale[1] che si vuole creare in corrispondenza a tale semantismo[2].

Nella composizione di un'opera poetica, la scelta delle immagini poetiche del contenuto dipende in (gran) parte dai modelli (di "poesie" già esistenti) che il poeta ha presenti nel momento creativo. La dipendenza da tali modelli può essere così accentuata, che di essi viene ripresa, oltre al contenuto, l'atmosfera musicale (la qual cosa comporta la ripetizione più o meno parziale, nell'ambito del verso, dell'interpretazione fonologica e dell'ordine delle parole peculiari dei modelli in questione); in questa prospettiva va vista e studiata la poesia "formulare" del Rigveda.

Tratto, a questo punto, dell'ordine disposizionale, limitatamente ai gruppi di N+N (in genitivo) e di N+Agg, nei quali la seconda entità è dipendente dalla prima, di alcune coppie di parole rigvediche nell'ambito dell'unità metrica del pada.

Duhitṛ+divaḥ (= "*figlia del cielo*)[3]

Le varianti disposizionali *duhitṛ divaḥ* e *divas duhitṛ* ricorrono normalmente l'una in posizione finale (l'unica "eccezione" si ha

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[1] Rientra qui anche l'allitterazione, per la quale rimando a Sani (1972).

[2] Per quanto concerne l'ordine delle parole, si deve distinguere tra le variazioni, semanticamente nulle, dovute all'atmosfera musicale e quelle, semanticamente non nulle (si vedano in proposito, e. g., Sgall 1969, Li & Thompson 1976), dovute a semantismi differenti.

[3] Epiteto dell'aurora (cf. Renou 1955–1969, III 8).

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nel verso trimetro V 80, 6a), l'altra in posizione non finale (e, volentieri, non iniziale) di pada. Per quanto concerne la seconda variante, *divaḥ* e *duhitṛ* possono essere, eventualmente, separate da altre parole; in tal caso *duhitṛ* può essere, in via eccezionale, collocata in posizione finale di pada (si vedano i dimetri IV 30, 9a; IV 52, 1c).

a) *vājebhir duhitar divaḥ* (I 30, 22b) "con premi, o figlia del cielo".
vy ùchā duhitar divaḥ (I 48, 1b; V 79, 3b; V 79, 9a) "risplendi, o figlia del cielo".
candréṇa duhitar divaḥ (I 48, 9b) "(con luce) splendente, o figlia del cielo".
prāvādyá duhitar divaḥ (I 49, 2d) "aiuta oggi (l'uomo dalla buona fama), o figlia del cielo".
Si vedano anche i dimetri IV 30, 8d; V 79, 2b; V 79, 8b; VI 81, 1b; VI 81, 3a; VIII 47, 14b; VIII 47, 15b; X 127, 8d.
ápa dvéṣo maghónī duhitā́ divá́ (I 48, 8c) "che la generosa figlia del cielo allontani le ostilità".
yásya yóge duhitā́ jā́yate divá́(ḥ) (X 39, 12c) "quando è aggiogato il quale (= carro), nasce la figlia del cielo".

b) *rayíṃ divo duhitar iṣayádhyai* (VI 64, 4d) "(portaci) la ricchezza, o figlia del cielo, affinchè possiamo usufruirne".
uchā́ divo duhitaḥ pratnaván no (VI 65, 6a) "brilla, o figlia del cielo, per noi come prima".
yā́ṃ tvā́ divo duhitar vardháyanty (VII 77, 6a) "te che, o figlia del cielo, (i Vasiṣṭha) rendono vigorosa".
Si vedano anche i trimetri I 71, 5d; I 92, 5d; I 113, 7a (= I 124, 3a); IV 51, 1c; IV 51, 10a; IV 51, 11a; V 80, 5d; VI 64, 5c; VII 67, 2d; VII 78, 4a; VII 81, 5c; X 70, 6a.
divó duhitā́ bhúvanasya pátnī (VII 75, 4d) "la figlia del cielo, la signora della creazione".
divá́ stave duhitā́ gótamebhiḥ (I 92, 7b) "la figlia del cielo è lodata dai Gotama".
ví divó devī́ duhitā́ dadhāty (VII 79, 3c) "la figlia del cielo, la dea distribuisce (beni)".
duhitṛ + sūryasya (= "*figlia del sole*")[4]

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[4] Si veda in proposito Oldenberg, 1909–1912, II 53.

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La variante disposizionale *sūryasya duhitṛ* ricorre in posizione sia iniziale sia mediana sia finale (nel dimetro IX 1,6b) di pada, *quella duhitṛ sūryasya* soltanto in posizione finale.

a) *sū́ryasya duhitā́bharat* (IX 113,3b) "la figlia del sole ha condotto (questo bufalo)".
sū́ryasya priyáṃ duhitús tiró rávam (IX 72,3b) "oltre il caro suono della figlia del sole".
á sū́ryasya duhitá tatāna (III 53,15c) "la figlia del sole ha esteso (fino agli dei la sua fama)".
yáṃ sū́ryasya duhitā́vṛṇīta (IV 43,2d) "(il carro) che ha scelto la figlia del sole".

b) *á vāṃ ráthaṃ duhitá sū́ryasya* (I 116,17a) "sul vostro carro (è salita) la figlia del sole".
Si vedano anche i trimetri I 117,13c; I 118,5b; VI 63,5a.
sūnu- + *sahasaḥ* (= "figlio della forza")[5]
La coppia in questione non è mai collocata in fine di pada.

La variante disposizionale *sahasaḥ sūnu-* ricorre in posizione iniziale oppure mediana dinanzi a parola che comincia in vocale, quella *sūnu- sahasaḥ* in posizione mediana dinanzi a parola che non comincia in vocale (l'unica "eccezione" si ha in VIII 19,7b).

a) *sáhasaḥ sūno trisadhasta havyám* (V 4,8b) "(apprezza) la (nostra) offerta, o figlio della forza, o (dio) dalle tre dimore".
sáhasaḥ sūno nahy ànyád ásty ápyam (X 142,1b) "o figlio della forza, non c'è altra amicizia".
sáhasaḥ sūno áti sá prá śṛṇve (X 11,7b) "egli si fa udire in lontananza, o figlio della forza".
puroḍā́śaṃ sahasaḥ sūnav áhutam (III 28,5b) "(possa tu gradire) la focaccia di riso che (ti) è stata offerta, o figlio della forza".
doṣá́ śiváḥ sahasaḥ sūno agne (IV 11,6c) "(sei) propizio durante la notte, o figlio della forza, o Agni".
Si vedano anche i trimetri V 3,9b; VIII 60,2a.

b) *áchidrā sūno sahaso no adyá* (I 58,8a) "o figlio della forza, (accorda)ci oggi (delle protezioni) indistruttibili".
ihá tváṃ sūno sahaso no adyá (IV 2,2a) "tu, o figlio della forza,

[5] Di norma, epiteto di Agni.

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(nato) oggi, (ti muovi) qui (come) nostro (messaggiero)".
babhrāṇáḥ sūno sahaso vy àdyaud (III 1,8a) "portato, o figlio della forza, hai brillato diffusamente".
nítyaḥ sūno sahaso jātavedaḥ (III 25,5b) "(tu che sei) nato nella famiglia, o figlio della forza, o Jātavedah".
Si vedano anche i trimetri VI 50,9a; VI 1,10c; VI 13,4a; VI 4,1b; VI 5,5b; VI 11,6c; VI 15,3c; VI 13,5b; VI 13,6a; VI 18,11c; VI 20,1d; VI 21,11b; VII 1,21b; VII 1,22d; VII 3,8c; VII 7,7b (= VII 8,7b); VII 16,4c; X 50,6b.

putra- + *sahasaḥ* (= "*figlio della forza*")[6]

Anche questa coppia non compare mai in fine di pada. E' generalizzata (con l'unica "eccezione" in III 46,6a) la variante disposizionale *sahasaḥ putra-*, la quale soltanto una volta (nel dimetro II 7,6c) ricorre in posizione iniziale di pada.

tvám āhuḥ sáhasas putrám aṅgiraḥ (V 11,6d) "essi ti chiamano figlio della forza, o Angirah".
tvám íd dhí sahasas putra mártya (I 40,2a) "o figlio della forza, il mortale ti (invoca)".
úc chocíṣā sahasas putra stutó (III 18,4a) "(brillando) con la (tua) fiamma, o figlio della forza, quando sei lodato".
Si vedano anche trimetri III 14,1c; III 14,4c; III 16,5c; V 3,1c; V 3,6d.

apām + *napāt* (= "*nipote delle acque*")[7]

La variante disposizionale *napāt apām* ricorre soltanto in posizione mediana, quella *apām napāt* in posizione sia iniziale sia mediana sia ("eccezionalmente" in VII 34,15a) finale di pada.

a) *prá vo nápātam apáṃ kṛṇudhvam* (I 122,4c) "date ad Apām Napāt la precedenza (nella) vostra (offerta)".
yéna nápātam apáṃ junáma (I 186,5c) "con il qale (canto) vogliamo mettere in movimento Apām Napāt".
tritó nápātam apāṃ suvṛktí V 41,10b) "con una bella lode (come) Trita (ha lodato) Apām Napāt".

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[6] Di norma, epiteto di Agni.

[7] Due sono gli inni del Rigveda ad Apām Napāt in quanto "spirito delle acque": II 35; X 30. Negli altri inni apām napāt è, per lo più, epiteto di Agni. Per il problema si veda Oldenberg 1917, 119 e sgg.

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b) *apáṃ nápātam ávase* (I 22,6a) "(canta) Apām Nāpāt per ottenere aiuto".

apáṃ nápād yó vásubhiḥ sahá priyó (I 143,1c) "Apām Napāt che insieme con i Vasu come caro (Hotṛ)".

Si vedano anche i trimetri II 31,6d; II 35,1c; II 35,2c; II 35,3d; II 35,9a; II 35,10b; III 9,1c; VI 50,13b; VII 47,2b; X 30,3b; X 30,4c; X 149,2b; X 92,13b.

só apáṃ nápād ūrjáyann apsv àntár (II 35,7c) "questo Apām Napāt che ha un sostanzioso nutrimento nelle acque".

só apáṃ nápād ánabhimlātavarṇo (II 35,13c) "questo Apām Napāt dal colore che non sbiadisce".

ubhe ródasī apáṃ nápāc ca mánma (VI 52,14b) "entrambi i mondi e Apāṃ Napāt (ascoltino) la preghiera".

Si vedano anche i trimetri VII 35,13c; X 8,5c.

pati- + *brahmaṇaḥ* (= "*signore della preghiera*")[8]

E' attestata la sola variante disposizionale *brahmaṇas pati-*; essa ricorre in posizione sia iniziale sia mediana sia (molto più frequentemente) finale di pada.

bráhmaṇas pate tvám asyá yantā (II 23,19a; II 24,16a) "o Brahmaṇaspati, (sia) tu il conduttore di questo (inno)".

bráhmaṇas pate suyámasya viśváhā (II 24,15a) "Brahmaṇaspati, (possiamo essere) sempre (conduttori di una ricchezza) facile da reggere".

Si vedano inoltre i trimetri II 24,14a; II 26,2d; X 67,7c.

iyáṃ vām brahmaṇas pate suvṛktír (VII 97,9a) "questo buon canto di lode è per voi due, o Brahmaṇaspati (e Indra)".

suśévam bráhmaṇas pátiṃ gṛṇīṣe (VII 97,3b) "canto (questo dio) benigno, Brahmaṇaspati".

kṛṇuhí brahmaṇas pate (I 18,1b) "fa, o Brahmaṇaspati, (che lo spremitore di soma sia oggi Kakṣīvant)".

rákṣā ṇo brahmaṇas pate (I 18,3c) "proteggici, o Brahmaṇaspati".

tváyā vayáṃ suvṛdhā brahmaṇas pate (II 23,9a) "grazie a te che ben rafforzi, o Brahmaṇaspati, (possiamo ricevere i beni)".

Si vedano inoltre I 18,5a; I 40,1a; II 1,3c; II 23,1c; II 23,5d; II 23,11c; IX 83,1a; X 155,2c; X 164,4a; X 174,1c;

[8] In quanto dio della preghiera; cf. Oldenberg 1917, 65 e sgg.

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I 18,4,3a; I 40,5a; II 23,17c; II 24,2c; II 24,4a; II 24,5d; II 24,8a; II 24,9b; II 24,11d; II 24,13d; II 25,1d (= II 25, 5d); II 26,4b; VI 75,17c; X 53,9d; X 65,1d; X 173,3d; I 38,13b; II 26,3d; V 46,3c; VII 41,1c; VII 44,1c; VIII 27,1c.

(rayi)pati- + *rayīṇām* (= "*padrone delle ricchezze*")[9]

E' attestata la sola variante disposizionale *(rayi)pati- rayīṇām*. Là dove ricorre il semplice *pati-*, una o più parole possono separare il primo membro della coppia (cioè *pati-*) dal secondo (si vedano, in proposito, i trimetri I 60,5a; III 7,3b). Il secondo membro (cioè *rayīṇām*) è collocato sempre in fine di pada.

agnír bhuvad rayipátī rayīṇám (I 60,4d; I 72,1c) "Agni era il padrone delle ricchezze".

tváṃ hy ási rayipátī rayīṇám (II 9,4c) "tu sei il padrone delle ricchezze". Si vedano inoltre i trimetri VI 31,1a; IX 97,24c.

sá cin nv àsām pátī rayīṇām (I 68,7b) "è soltanto lui il padrone di queste ricchezze".

vayáṃ syāma pátayo rayīṇám (IV 50,6d; V 55,10d; VIII 40, 12d; VIII 48,13d; X 121,10d) "possiamo noi essere signori di ricchezze".

sómaḥ pátī rayīṇám (IX 101,6c) "Soma, il signore delle ricchezze".

Regole analoghe valgono per la coppia[10] *dharuṇa-* + *rayīṇām*: si vedano I 73,4d; X 5,1a; X 45,5a; X 47,2b.

(sam)rājan + *carṣaṇīnām* (= "*re dei popoli*")[11]

E' attestata la sola variante disposizionale *(sam)rājan carṣaṇīnām*; i due membri possono ricorrere (come si ha in I 32, 15c) separati da altre parole.

samrā́jaṃ carsaṇīnám (III 10,1b; X 134,1d) "(lui), il re dei popoli".

prá samrā́jaṃ carṣaṇīnám (VIII 16,1a) "(cantate) il re dei popoli".

rā́jānaṃ carṣaṇīnám (V 39,4b) "(invito) il re dei popoli".

yó rā́jā carṣaṇīnām (VIII 70,1a) "lui che è il re dei popoli".

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[9] Epiteto di Agni e, occasionalmente, di altri dei.

[10] Di norma, epiteto di Agni.

[11] Di norma, epiteto di Indra.

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Regole analoghe valgono per 'a coppia[12] *vṛṣabha* + *carṣaṇīnām*:

nitóśanaṃ vṛṣabháṃ carṣaṇīnắm (VI 1,8b) "(noi avviciniamo il Poeta), il concessore (di ricchezze), il toro dei popoli".

gīrbhír vardha vṛṣabháṃ carṣaṇīnắm (VI 18,1d) "accresci con lodi il toro dei popoli".

(a)jāyathā vṛṣabha carṣaṇīnắm (X 180,3b) "tu sei nato, o toro dei popoli". Si vedano inoltre VIII 96,4d; VIII 96,18a.

pāre + *rajasaḥ* (= "*al limite estremo del mondo* (*in quanto estensione orizzontale*)")

Sono attestate (quasi tutte in posizione mediana di pada; fanno "eccezione" I 33,7b; X 43,5b) entrambe le varianti disposizionali. La variante *pāre rajasaḥ* ricorre soltanto quando uno dei due membri è accompagnato da un aggettivo.

a) *yắ sísratū rájasaḥ pāré ádhvano* (VIII 59,2c) "(questi dei) che sono giunti al limite estremo del mondo, del percorso".

b) *tvám asyá pāré rájaso vyómanaḥ* (I 52,12a) "al limite estremo di questo mondo, del cielo, tu (hai reso la terra contrappeso della tua forza)".

yó asyá pāré rájaso vivéṣa (X 27,7d) "(lui) che ha agito al limite estremo di questo mondo".

yó asyá pāré rájasaḥ (X 187,5a) "(lui) che (è nato) al limite estremo di questo mondo".

dūré pāré rájaso rocanắkaram (X 49,6d) "al lontano limite estremo di questo mondo ho trasformato (Vṛtra) in luci".

śociṣa + *śukreṇa* (= "*con luce splendente*")

E' attestata la sola variante disposizionale *śukreṇa śociṣā*; questa ricorre in posizione finale nei pada dimetri, mediana in quelli trimetri.

ágne śukréṇa śocíṣā (I 12,12a; VIII 44,14b; X 21,8a) "o Agni, con luce splendente".

śukréṇa deva śocíṣā (VI 48,7b) "con luce splendente, o dio". Si vedano inoltre I 45,4d; I 48 14d (= IV 52,7c); VIII 56,5c; X 187,3b.

bhānúḥ śukréṇa śocíṣā vy àdyaut (IX 85,12c) "la fiamma ha brillato con luce splendente".

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[12] Di norma, epiteto di Indra.

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Si vedano inoltre X 45,7d; X 123,8c.

vraja- + *gomat* (= *"il recinto ricco di buoi"*)

La variante *vraja- gomat* ricorre, nel pada, in posizione iniziale (e, una sola volta in posizione mediana, in VI 73,3b, dove da *vraja-* dipende anche l'aggettivo *mah*, che precede quest' ultimo), quella *gomat vraja-* in posizione finale (ed una volta in posizione mediana: si veda VII 27,1d).

a) *vrajáṃ gómantam uśíjo ví vavruḥ* (IV 1,15d; IV 16,6d; X 45, 11d) "gli Uśij hanno aperto il recinto ricco di buoi".
Si vedano inoltre X 25,5d; X 62,7d; VI 10,3d.

b) *gámat sá gómati vrajé* (VII 32,10d) "questo giunge al recinto ricco di buoi".
asmán ava maghavan gómati vrajé (VIII 70,6c) "proteggici, o generoso, nel recinto ricco di buoi".
Si vedano inoltre I 86,3c; V 34,5d; VIII 46,9d; VI 62,11b.

Per quanto concerne gli aggettivi, si deve precisare che essi spesso (come avviene regolarmente per la penultima coppia esaminata) precedono il nome da cui dipendono; si vedano, e. g., *āśu-*, *uru-*, *viśva-*, *hiraṇyaya-*. Quando in un pada due aggettivi dipendono da uno stesso nome, l'uno può precedere e l'altro seguire tale nome; si vedano, e. g., *āyaí sūkténa vácasā návena* (II 18,3b) "con un discorso nuovo, ben detto, affinchè (Indra) giunga", *aniréṇa vácasā phalgvèna* (IV 5,14a) "con un discorso senza forza, che non dice nulla".

Le regole disposizionali, postulate per gli esempi presi in considerazione, permettono di studiare con maggior sistematicità certi aspetti della tradizione formulare rigvedica: sulla base di esse si possono evidenziare ulteriormente i nessi (di tradizione) tra pada aventi contenuto ed espressione (più o meno) parzialmente comuni. Poichè tali regole hanno valore soltanto nell'ambito del pada (in quanto complesso semantico-musicale), ne consegue che esse non possono venire, ipso facto, considerate norme della disposizione delle parole nella lingua[13] parlata quotidianamente dai poeti vedici.

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[13] Considerata artificialmente come "realtà" acronica.

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Prescindendo dagli altri testi letterari indiani, l'ordine delle parole (nel nostro caso limitatamente ai gruppi di N + N in genitivo e N + Agg., nei quali la seconda entità è dipendente dalla prima) della lingua dei tempi vedici può essere restituito assai fedelmente, qualora si operi un confronto di tipo sincronico con una lingua attuale che presenti un sistema morfo-sintattico nominale simile a quello vedico (la qual cosa implica, tra l'altro, per la lingua in questione la cosidetta "frase nominale")[14]. A tal fine è utilizzabile il lituano, che è la lingua indoeuropea attuale più vicina a quella vedica. In lituano i N in genitivo e gli Agg. (in funzione "attributiva") precedono[15], di norma, i N dai quali dipendono; si vedano, da una parte, *mergaitės kasos* "le trecce della fanciulla", *beržo šaka* "un ramo di betulla", *karvės pienas* "il latte di vacca", *sniego baltumas* "la bianchezza della neve" (cf. L. K. G. 185 e sgg.), dall'altra *aukštas namas* "una casa alta", *prie laukų darbų apsukri Monika* "Monica, svelta nei lavori dei campi", *geri vaikai* "dei buoni bambini" (cf. L. K. G. 235 e sgg.; Senn 1966, 357 e sgg.). Nella lingua poetica possono ricorrere (per motivi, per lo più, di atmosfera musicale) disposizioni di parole, le quali non sono "normali" nella lingua parlata e nella prosa; cito esempi dalle poesie[16] di Maironis (morto nel 1932):

a) *Kad, pavirtęs kaip ąžuolas girių puikus* (21) "che, caduto come una superba quercia dei boschi".
Apkaitins giesmėmis dangaus (29) "accenderà con canti del cielo (l'affannato petto)".
Kaip bangos ant marių, kaip mintys žmogaus (30) "come le onde dei mari, come i pensieri dell'uomo".
Daug mergaičių pakrančiais Dubysos (36) "molte ragazze lungo le rive del (fiume) Dubysa".

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[14] Cf. Benveniste 1971, 179 e sgg.

[15] La disposizione differente che, non raramente, si riscontra per i primi nei monumenti in prosa della lingua arcaica è da attribuire all'influsso di modelli polacchi (cf. Senn 1966, 398).

[16] Della raccolta *Pavasario balsai*, conformemente all'edizione pubblicata a Vilnius da Vaga nel 1976. Con i numeri fra parentesi, collocati dopo ciascun verso lituano citato, si rimanda alle pagine di tale edizione.

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Tvinksta, rausta ir dega lyg veideliai mergai čių (46) "si ingrossa, diviene rossa ed arde come volti di fanciulle".
Kaip po langu sužvengia žirgas jauno bernaičio (46) "come sotto la finestra nitrisce il cavallo del giovane ragazzo".
Ir sparnas miego malonaus (50) "e l'ala del dolce sonno".
Jo vardas: gėlė atminimo (58) "il suo nome: fiore del ricordo".
Jog žemė — tai šalis vargų (68) "che la terra (è) una valle di lacrime".
Vien tik ugnys gaisrų (74) "soltanto le fiamme degli incendi".
Nei energijos jaunimo (98) "non (c'è) l'energia della giovinezza".

b) *Ant vieškelio amžių plataus* (16) "sull'ampia strada dei secoli".
Nebijokim vargo kieto (18) "non temiamo la dura fatica".
Ir sušildo krūtinę ir vaidentuvę jauną (20) "e accende il petto e la giovane fantasia".
Ieškos įkvėpimo brangaus (30) "cercherà la cara ispirazione".
Augo uosis gražus (33) "cresceva un bel frassino".
Tarp pakalnių placių (33) "tra ampie valli".
Kam tos uogos raudonos (36) "per chi queste rosse bacche".
Skamba giesmės auksinės (36) "risuonano aurei inni".
Numesk, tėvyne, rūbą seną (42) "getta, o patria, il vecchio vestito".
Nenoriu nei aukso, nei rūbų šilkinių (56) "non desidero né oro, né vestiti di seta".
Veltui ieškai nors vieno atminimo brangaus (97) "invano cerchi un solo caro ricordo".

Il fatto che, nella lingua poetica lituana, si possa avere una varietà di disposizioni analoga a quella del Rigveda costituisce un decisivo punto d'appoggio alla mia *proposta di ipotizzare per la lingua parlata dei tempi vedici le regole disposizionali N(in genitivo) N, Agg. N*, sulla base di un confronto sincronico con il lituano.

La mia ipotesi riceve piena conferma, quando si prenda in considerazione la lingua indiana della fase post-vedica[17], cioè il sanscrito[18].

[17] La prosa vedica non può essere presa in considerazione in quanto risente di influssi della lingua poetica.

[18] Si veda, e. g., Thumb-Hauschild 1959–1960, II 438 e sgg.

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Historical Development of the Ancient Greek Accent System[1]

Ardhamātrālāghavena putrotsvam manyante vaiyākaraṇāḥ
"Grammarians rejoice more over the saving of half a mora than over the birth of a son."
Paribhāṣā 122

Typological classifications of languages in terms of their accentology have been proposed by several scholars (Jakobson (1931); Trubetzkoy (1939: 179–94); Martinet (1954); Garde (1968: 97–139); Kiparsky (1973)). According to Garde, in addition to languages with fixed accent and those with free accent (where accent is "linked with the accentual properties of morphemes") there are at least two sub-types: quasi-fixed—where accent is linked sometimes with word-boundaries and sometimes with properties of morphemes (Polish, Macedonian, Latin); with limited freedom—where accent is linked simultaneously with word-boundaries and with the properties of morphemes (Ancient and Modern Greek). The following paper will demonstrate the usefulness of these principles in the genealogical study of successive états de langue (PIE.—Proto Greek—Ancient Greek dialects).

It is obvious that any analysis of suprasegmental phonology of dead languages faces serious—if not insurmountable—difficulties. In the case of Ancient Greek we are lucky to have a wealth of dialectal inscriptions, examples of a literary usage of various dialects and even descriptions of ancient grammarians, who introduced diacritics for marking accent around 200 B. C. If the analysis of living tone-accent languages depends very

[1] An earlier version of this paper was read at the Third International Conference on Historical Linguistics in Hamburg, August 1977. I am grateful to Dr. B. Newton (Simon Fraser University), Dr. S. Levin (State University of New York), Dr. J. Hewson (Memorial University of Newfoundland) and Dr. H. Andersen (University of Copenhagen) for their comments and criticism.

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much on reading spectrograms, then this empirical technique has to be replaced by philological work on Greek dialectal texts. It is worth emphasizing, however, that classical scholars can enormously profit by drawing parallels with the development of the well investigated living tone-accent languages, namely Serbo-Croatian. In what follows I would like to point out some interesting parallels between the development of the Hellenic and Slavic groups of languages/dialects in the matter of accent.

It is generally agreed that Ancient Greek—or more precisely the Attic-Ionic dialect—was a tone-accent language, very much as Modern Serbo-Croatian or Lithuanian. We may recall that there are two conditions on the status of a "real" tone-accent language (as shown by Trubetzkoy):

i) mobility of accent,

ii) distinctive vowel length.

If any one of these conditions is about to disappear, it may be said that a language/dialect is on the way to becoming a stress-accent type. Thus the transition from the tone-accent type to the stress-accent type may manifest itself in fixation and immobilization of the accent, which is easy to demonstrate for Aeolic (Lesbian) and Doric dialects. (It will be shown that Lesbian fixed its accent purely phonologically, whereas Doric dialects tended to immobilize their accent morphologically). Before turning our attention to the accentual history I would like to make a few comments on the temporal and spatial dimensions of my investigation.

Greek at the time of the earlist alphabetic records (8/7th c. B. C.) is represented by a number of widely differing dialects—some of them very unlike one another. Meillet (1965: 79) commented on their mutual understandability "it must have been difficult for Greeks from different cities, speaking different dialects, if not to grasp the general sense, at least to understand one another exactly". However, all these dialects were certainly recognizable as Greek; it is perhaps significant that Greek sources themselves do never refer to mutual unintelligibility.

We may venture the back-projection of the four principal Greek dialectal groups of the Classical Era to the last centuries

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of the 2nd Millenium—thus we may talk about proto-Aeolic, proto-Doric as rather autonomous dialectal units, and proto-Ionic and proto-Arcado-Cypriot (or proto-"Achaean") of the same period as two closely linked dialectal formations. As shown in works by W. Porzig and E. Risch the most characteristic features of Ionic, such as the fronting and raising of the Proto-Greek $\bar{a} > \bar{æ} > \bar{e}$, are of relatively recent date. Many features of Ionic can be securely dated as post-Mycenaean and indeed many scholars, e. g. Chadwick (1963: 16), have wondered whether there was Ionic differentiated from the common Mycenaean dialect. There is however no doubt about the existence of two Greek dialects in the Late Helladic (= Mycenaean) period, called usually *ti*-group and *si*-group. E. g. the 3rd Pers. Pl.Act. of the thematic verb had the suffix *-onti*, which is preserved in West Greek (φέροντι); East Greek dialects have forms ending in *-si* (which is a result of dental palatalization and simplification of the affricate): Arcadian φέρονσι, Lesbian φέροισι, and Attic-Ionic φέρουσι, Mycenaean /phéronsi/. Thus many classical scholars have accepted the existence by 1400 B. C. of two Greek dialects, respective dialectal groups.

Any investigation into the prehistory of the Greek accent may start with Kuryłowicz's observation (1958: 7) "Pour comprendre l'accent grec il suffit de partir d'un état à peu près védique". In other words, we may imagine that in the PIE. ancestor of Ancient Greek—as in Vedic—the accent moved within word boundaries without phonological constraints limiting the range of positions. For instance, we may recall descriptions of accentual behaviour of various nominal and verbal classes—in terms of morphological categories—as elaborated by Ancient Indian grammarians. Thus grammar of Vedic Sanskrit contains accentual rules of the morphological nature such as: verbs of the Class I (e. g. *bhárāmi*) are stem accented, verbs of Class VI (*tudámi*) are theme accented; on the other hand, verbs of the Classes II, III, V, VII (e. g. *védmi—vidmáḥ*) accent the stem in the Singular of the Present but the termination of the Dual and Plural. Another morphological rule describes the accentual behaviour of all Imperfects—namely, the augment is always accented.

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Freedom of the PIE. accent was restricted in various ways in all daughter languages. (It would be better to say that morphological accentual rules were in their turn almost everywhere replaced by phonological rules). For instance, neither Greek nor Iranian continues the presumably PIE. rule of accenting the augment in historical tenses. Neither did Greek, Iranian or Balto-Slavic verbs continue the trichotomy of movable, stem-accented and theme-accented verbs. It is a pity that we are on safe gound only in Greek, but it is possible to make certain conclusions about Proto-Iranian and Proto-Balto-Slavic from later conditions. The situation in various Slavic languages is explicable from a penultimate fixed stress (with certain morphological restrictions such as possibility of the antepenultimate stress if the root syllable was third from the end, according to Shevelov (1964: 71)). The fixation of stress on the penultimate syllable is assumed by Kuryłowicz also for Iranian and Armenian. In Greek freedom of the PIE. accent was limited by allowing the rule assigning accent to operate only within one of the last three syllable—thus against Vedic 1st Pl. *bhárāmaḥ* (Pres.) and *ábharāma* (Impf.) with the accent on the augment, we find in all Ancient Greek dialacts the accent on the antepenult φέρομεν — ἐφέρομεν. What is remarkable in this development in all Satem languages is the fact that the simple phonological rule replaces the morphologically complicated accentuation of PIE. It might be tempting to talk about "simplification of grammar", but there appear at the same time new complications which I will discuss shortly. It should also be mentioned that the assumption of a period in Common Slavic with fixed stress on the penultimate is by no means secure (in any case it should not be used for establishing "a one more Slavic-Iranian link" as Shevelov (1964: 71) wanted it). Let us remind ourselves of the well-known accentual dichotomy of stem-accented and ending-accented accentual classes of Russian, Ukrainian, Byelo-Russian and Serbo-Croatian dialects[2]. Ancient Greek preserved a more

[2] Of course, the accentuation of verbs and nouns in individual Slavic languages underwent various morphologically conditioned levelings. Shevelov (1964: 74) places the appearence of the paradigms with final

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original state of affairs—in having "normal" verbs accented on the stem, contract verbs on the post-root (= prethematic vowels φιλέω, τιμάω, χρυσόω, as in Vedic Class X *čoráyāmi*); and there are even remnants of the mobile class (e. g. οἶδα, ἴδμεν < **wid—mén*). This accentual trichotomy has been severely limited in Arcado-Cypriot and Aeolic which accent denominative and deverbative verbs as Attic does its athematic verbs (Lesbian φίλημι vs. Attic φιλέω, cf. Attic ὀνίνημι). It is also well-known that only Vedic and Greek among IE. languages preserved PIE. oxytones (in Greek some of them became paroxytones, according to Wheeler's Law), whereas Lithuanian and Slavic languages shifted the PIE. accent from the ultima to the penult (these are cases covered in historical textbooks by Hirt's Law). As far as Proto-Greek is concerned we can reconstruct the stage where the place of accent would be describable by rules such as: oxytones and paroxytones keep their accent distributed columnarly (e. g. *potamós*, *potamósyo* and *dā́mos*, *dā́mosyo*), whereas proparoxytones keep their accent on the antepenult:

Sg.	Nom.	*pherómenos*	Pl.	*pherómenoi*
	Gen.	**pheroménosyo*		**pheroménoōn*
	Dat.	**pheroménoei*		**pheroménosi*
	Acc.	*pherómenon*		*pherómenons*
Sg.	1	*légō*	Pl.	*légomes*
	2	**légesi*		*légete*
	3	**légeti*		*légonti*

This antepenult rule assigning accent in Proto-Greek (and possibly even in its late Mycenaean descendant) operated on syllables—in other words we may assume that both Proto-Greek and Mycenaean were of stress-accent typology. During the process of vocalic contractions Greek started changing its typology toward a tone-accent language. Put simply, after the contractions involving the thematic vowel and the ending proper (i. e. after the loss of a syllable) it becomes more economical to describe the Greek accent in terms of moras. (In what follows,

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stress in the late Common Slavic period, when the jers were reduced to zero and the stress in the verbal paradigm was reinterpreted in both Sg. and Pl. as final.

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I will be using the concept of the mora as defined by Garde [1968: 142], i. e. as "pitch-carrier").

However, we should not forget Martinet's (1954: 75) caveat that the mora does not correspond to a phonetic reality, but it is only a purely analytical device; there are languages in which this concept makes the description of phonology clearer and may be more economical, and there are others in which it does not. In our case, the accentual phenomena of Proto-Greek are adequately described in terms of syllables and morphology; however, starting in Mycenaean, i. e. around 14th c. B. C., and describing the prehistory and history of Classical dialects it is more economical to do so in terms of the mora than of the syllable (or syllabic nucleus). Furthermore, we should keep in mind that even in tone-accent languages such as Ancient Greek, Lithuanian and Serbo-Croatian where a wide range of accentual phenomena is describable in moras, we have to admit quite frequently syllabic and/or morphological criteria in our accentual descriptions. A classical example is Jakobson's version (1937/1962: 263) of the general limiting rule for Ancient Greek: "The span between the accented and the final mora cannot exceed one syllable".

We may recall Vendryes' (1929: 53) and Garde's (1968: 145) attempts to formulate this rule in terms of the last three moras. Garde has to explain the occurrence of forms such as ἄνθρωπος (where the high pitch occurs on the fourth mora from the end (1 + 2 + 1)), which are neatly covered by the rule as formulated by Jakobson. According to Garde in the case of words with short ultima the last three moras are identical with the last three syllables. I believe with Allen (1973: 238) that this assumption implying that a penultimate long vowel in words like ἄνθρωπος should not be counted as a long one is quite unrealistic and I think that Jakobson's formulation is preferable. Perhaps even better formulation is that by Allen (1973: 237) "not more than one mora may follow the contonation" where contonation includes the falling glide and has thus a monsyllabic (in the case of circumflex) or disyllabic form (in the case of acute). The reason is that Jakobson's formulation does not explain why the accent of the Gen. Pl. ἀνθρώπων is acute not circumflex (if it

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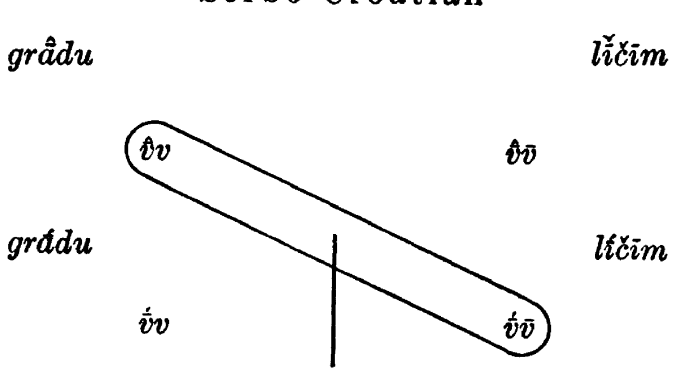
were circumflex the distance between the accented and final moras would be only two moras i. e. one syllable). It may be noted that in this case both Allen's and Garde's formulation are adequate (according to Allen the phonetic glide ends at the end of ultima, that is there is no more left; and according to Garde the accented mora is the third one from the end). However, it is more usual to cover these cases by the so-called 'final trochee' rule, whereby if the high pitch occurs on the penultimate syllable containing a long vowel or a diphthong, it must occur on the first mora if the final vowel is short (i.e. the accent must be



('final trochee' rule
+ general limiting rule)

dêmos ~ dḗmǭ

circumflex and not acute). It is usually assumed that this was only the Attic rule since circumflexed disyllabic words have acute in Doric (Laconian) (e. g. Attic παῖδες vs. Doric παίδες)[3] and in Aeolic (Lesbian) there are no polysyllabic words with the circumflex on the penult (e. g. Attic ἀρχαῖος vs. Lesbian ἄρχαιος). At this point we may notice the cardinal difference between the accent pattern of Attic-Ionic and modern Serbo-Croation dialects. In Attic-Ionic, as a consequence of the 'final trochee' rule,

[3] Examples of Doric accents can be found in Schwyzer (1939: 384): αἴγες, πτώκας, ἐνθοίσα, ἀμόναι, στάσαι (Inf.), ἀνθρώποι.

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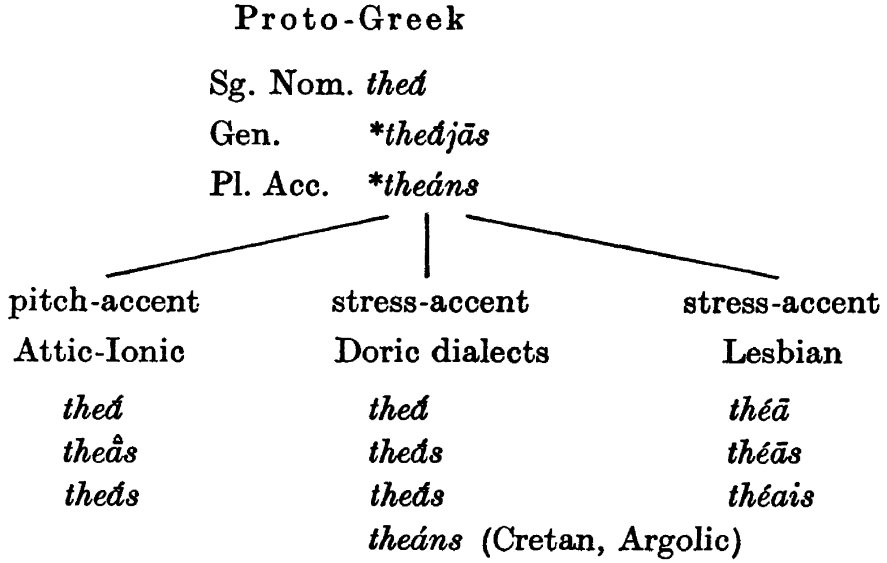
the occurrence of circumflex or acute on the penult is automatically determined by the length of the ultima and minimal pairs such as classical examples from Serbo-Croation (*grȃdu* ~ *grádu* "city" [Dat. vs. Loc.] *lȋčīm* "I paint" ~ *líčīm* "I resemble") are here impossible:

At this point—when the general limiting rule started operating on moras instead of syllables—we may assume that Proto-Greek changed its typology from the stress-accent language toward a tone-accent language. In other words, there appeared contour accents on final syllables after early contractions obliterated the boundary between the thematic vowel and the case or personal suffix. As is well-known this boundary is still recoverable in Sanskrit—contrast the disyllabic ending of the Gen. Sg. Fem. *kanyáyās* "girl" with a dimoric ending of the same declension in Ancient Greek θεᾶς where we presume that even Mycenaean [theâs] (about 13/14 c. B. C.) had a monosyllabic ending. If we want to know when this circumflex became phonemic, we have to ask whether we can date the loss of *-n-* in the Acc. Pl. Consider the minimal pair such as Classical Attic θεᾶς (Gen. Sg.) vs. θεάς (Acc. Pl.) in other dialectal groups:



I think that for Doric dialects we do not have to reconstruct this difference in tone signaling a difference in grammatical function. Certainly not for some of them such as Cretan and Argolic

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which preserved the Proto-Greek cluster *-ns* in the Acc. Pl. Unfortunately, we cannot make any decisive statement about other Doric dialects, especially so-called "mild" dialects (Megarian and Corinthian), which were close geographically to Attic-Ionic and thus conceivably could be influenced in their accent pattern by Attic-Ionic. Incidently, this contrast did not exist even in Lesbian where the Acc. Pl. developed a diphthong *ai*. Furthermore, we may observe that in comparison with Slavic Greek due to its general limiting and 'final trochee' rule lacked the prerequisite for the rise of contrastive tone in middle syllables. There are actually some rare minimal pairs with the contrast acute ~ circumflex on the penult such as:

λῦσαι (Inf.) ~ λύσαι (Opt.) "bind"
οἶκοι (Nom. Pl.) ~ οἴκοι (Loc. Sg.) "house"
μῆτις "intention" μήτις "nobody"

but these are best explained as shown by Kiparsky (1967: 109) by grammatical rules which allot different mora values to the final diphthongs (< λύσειε, < οἰκο+ι) or by postulating a morpheme boundary in underlying representation of μή+τις. Thus, after all, it seems that Garde is right in treating penultimate syllables as monomoric units since—whith these rare exceptions—the phonological opposition of acute and circumflex on them is impossible.

At this point, I would like to emphasize that the traditional view of Ancient Greek as a tone-accent language is rather unsatisfactory. As we have seen in terms of tonemic contrasts we can find pairs involving only ultima in certain dialects, most notably the literary Attic-Ionic and probably insular Ionic dialects (and maybe some "mild" Doric dialects). Aeolic dialects—to judge on the basis of dialect of the island of Lesbos—generalized the recessive accent (i. e. the alternation circumflex ~ acute is possible only on the penult, where no real minimal pairs are possible). So-called "strict" and probably even "middle" Doric dialects were contourless, in other words, their typology was that of a stress-accent language. This can be assumed on the basis of the absence of minimal pairs involving ultima (such as Gen. Sg. vs. Acc. Pl.), as shown above. Another important

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piece of evidence for the stress-accent typology of "strict" Doric can be found in phonetic interpretation of phonomena covered by so-called "Wheeler's" and "Vendryes' Law". Wheeler's Law ("Daktylisch ausgehende oxytona werden zu Paroxytona") covers examples of the shift of accent from the ultima to the penult in words with the heavy antepenult:

Vedic	Proto-Greek	Attic-Ionic	
pēšaláḥ	*poikilós*	*poikílos*	"variegated"
aŋkuráḥ	*aŋkulós*	*aŋkúlos*	"crooked"

Using Allen's theory (1967: 46) of the stress in Ancient Greek (basically, the stress was regulated by the quantity of the ultima, whereas the tone by the length of the ultima)[4] we may notice that in Proto-Greek *poikilŏ́s* there was a positive correlation of the pitch and stress in that the stress and high tone coincided on the ultima (as in Vedic). After the metatony—described by the label of Wheeler's Law—this correlation changed into a negative one (*poikílŏ̂s*) of high tone and stress. This of course is a typical situation arising in tone-accent languages such as Serbo-Croation, where the fundamental frequency of the post-acute syllable is "level or slightly rising" (in the case of long rising accent, Lehiste (1970: 86)). It may be noted that Lesbian dialect with the typical antepenultimate accent shows the positive correlation of the tone and stress *pŏ́ikilos*. Another type of metatony—which presumably took place in Old Attic—is described by Vendryes' Law ("Tout properispomène à antépénultième brève devient en attique proparoxyton") according to which the falling accent on the penult became a rising accent on the antepenult if the antepenult was light. Thus for instance, Old Attic *hetŏ̂imos* showing a positive correlation of high tone and stress (i. e. there was a falling ac-

[4] Allen (1967: 46ff.) summarized his hypothesis concerning the incidence of stress in plurisyllabic words in Ancient Greek in three points:
(a) words were primarily stressed on their last heavy syllable;
(b) a secondary stress fell on preceding heavy syllables if separated from the primary stress by at least one (quantitative) mora;
(c) a (probably weaker) stress fell on an initial light syllable if followed by two light syllables.

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cent and the stress on the penult) developed a negative correlation where the high tone appeared on the antepenult and the post-acute syllable preserved the stress *hétŏimos*. The Lesbian dialect in this case did the same, however Laconian shows the correlation of high tone and stress as Old Attic did (at least in the Plural *hetóĭmoi* to use the actually documented forms). Other types of the correlation may be surveyed in the following chart:

A correlation of tone and stress in Ancient Greek dialects

				Coincidence
(1)	Attic	ἔλεγον	*élegon*	+
	Lesbian	ἔλεγον	*élegon*	+
	Laconian	ἐλέγον	*élegon*	+
(2)	Attic	ποταμός	*pŏtamós*	—
	Lesbian	πόταμος	*pŏtamos*	+
	Laconian	ποταμός	*potamŏs*	+
(3)	Attic	ἀγγέλους	*ăŋgélō̆s*	—
	Lesbian	ἀγγέλοις	*ăŋgélois*	—
	Laconian	ἀγγέλως	*ăŋgélō̆s*	(—)
(4)	Attic	ἄνθρωποι	*ánthrŏpoi*	—
	Lesbian	ἄνθρωποι	*ánthrŏpoi*	—
	Laconian	ἀνθρώποι	*anthrŏ́poi*	+
(5)	Attic	ἀρχαῖος	*arkhá͡ios*	+
	Lesbian	ἄρχαιος	*árkha͡ios*	—
	Laconian	ἀρχαίος	*arkhá͡ios*	+

As this contrastive survey shows there was always a coincidence of the tone and (hypothetical) stress in Laconian (with the exception of (3) which is negative everywhere). Thus we may hypothesize that Doric dialects—at least so-called "strict" dialects such as Laconian—preserved the Proto-Greek stress-accent. For Attic and Lesbian we may notice—in Allen's words (1967: 50)—"a predominantly negative correlation of high tone with stress and a positive correlation of svarita with stress". Those are cases (2), (3), (4) for Attic, and (3), (4), (5) for Lesbian. As pointed out above a similar situation obtains in

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tone-accent languages with a disyllabic type of accentuation such as Serbo-Croatian. A coincidence of tone and stress at word level (i. e. monosyllabic accentuation), which is typical of Modern Greek as a stress-accent language is supposed to be reached by the end of the Greco-Roman period (3rd c. A. D.)[5]. As we have seen, it is very likely that some of the Ancient dialects—namely "strict" Doric dialects in a sense anticipated this stage; or more precisely, some of the Greek dialects were of the stress-accent typology during the whole history. Thus I would like to conclude the whole analysis by raising questions which I believe are more appropriate than "when did Ancient Greek lose its tone system?". Namely, "did stress-accent dialects—especially Doric—contribute in an essential way to the new stress-accent typology of Middle Greek? Could the contact between speakers of Ionic and Doric dialects accelerate the acquisition of stress-accent typology? How important was the anticipation of the stress-accent typology by Classical and Hellenistic Doric dialects for the Middle Greek?" It is well-known—to use Blass-Debrunner's words (1961: 1)—that "on the lips of other Greeks, Attic gradually lost the peculiarities which set it off from all or most other dialects". Of course, we will never know to what extent this statement corresponds to the reality; in other words—how big were the local differences within Koine. Nevertheless, we may venture some conjectures on the basis of Hellenistic inscriptions. First of all, we may recall that during the last three centuries B. C. we encounter not only inscriptions in Attic-Ionic Koine (i. e. Koine based on the tone-accent dialect) but also inscriptions in the so-called Doric Koine (i. e. Koine based on presumably stress-accent Doric dialects) and Northwest Greek Koine (basically a mixture of Attic with West Greek forms similar to the Doric Koine). If the reconstruction of the phonetic substance of the accent pattern of the Doric dialects during

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[5] Allen (1973: 268) mentions evidence from the work of Gregory Nazianzen in whose hymns there is a tendency to single correspondence between accentual peak and strong position. Similar indications of the transition to a dynamic accent can be seen in an anonymous early 4th century Christian hymn (Pap. Amherst, ed. Grenfell & Hunt, I. ii) and in the hymns of Clement of Alexandria.

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the Hellenistic era surpasses our capacities, we may pinpoint at least some Doricisms (or rather "non-Atticisms") in the Hellenistic Greek regarding the place of accent. E. g. in the paradigm of the 2nd Aorist Attic ἔλαβον was ambiguous (it could mean either "I took" or "they took"). whereas in Doric dialects the 1st Pers. Sg. was accented on the antepenult but the 3rd Pers. Pl. on the penult ἐλάβον "they took"[6]. The situation in contract verbs is similar. In Attic verbal inflections the accent always recedes to its full limits but in Doric dialects the accent is typically "progressive" in the 3rd Pers. Pl. It may be noted that both situations are anomalous for different reasons. Attic paradigms are regular phonologically (in that the accent always recedes to its full limits) but irregular morphologically since the plural paradigms have mobile accent. On the other hand, Doric paradigms are regular morphologically in that the accent is immobilized (on the augment in singular and on the root in plural), but irregular phonologically (the accent could still recede one syllable leftwards in the 3rd Pl.).

Hellenistic Greek solved anomalies of both Attic and Doric systems by borrowing the ending of the sigmatic Aorist *-san*[7]. The extra-syllable in ἐλάβοσαν made it possible to keep the accent on the root (as in Doric plural paradigms) satisfying at the same time the recessive accent of verbal system (as in Attic)[8].

To add some sociolinguistic flavour to the analysis, we also have to consider an urban dialectal (and accentual) diversity

[6] See Schwyzer (1939: 384).

[7] According to Hainsworth (1967: 73) ancient writers variously ascribe the Hellenistic type ἐλάβοσαν to Boeotian, Doric, Chalcidian, Alexandrian, οἱ τῇ Ἀσιάνῃ φονῇ and οἱ ἑλληνίζοντες ἐν Κιλικίᾳ.

[8] It is of interest to observe a similar tug of war between phonology and morphology in Modern Greek. In Modern Greek past tenses the augment is lost if unstressed—this happens in the 1st and 2nd Pl. but there are two forms in the 3rd Pers.:

Sg. 1	*égrafa* "I wrote"	Pl.	*gráfame*
2	*égrafas*		*gráfate*
3	*égrafe*		*égrafan* or *gráfane*

The phonologically proper 3rd Pl. *égrafan* is paradigmatically irregular. This irregularity can be liquidated only by the adoption of an extrasyllable *(e)gráfan-e*, which allows all plural forms to have accent on the root.

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(e. g. Athens — Attic, Korinthos — "Mild" Doric, Sparta — "Strict" Doric) against the background of geographical areas with heterogenous speech communities and various social institutions (such as Hellenistic military camps), where the speakers of various dialects had to establish a contact. E. g. we may imagine speakers of Doric dialects and Attic resorting to a type of accentual Koine (with non-phonemic contours on the ultima and uncertainty as to the antepenult/penult accentuation in many verbal and nominal forms) across the border of Ionic Attica and "Mild" Doric Megara, or in the Saronic Gulf with adjacent shores of Attica, Megara, Korinthos and Argolis.

Obviously, attempts to reconstruct these events cannot be cast in sociolinguistic terms of "how many speakers of 'mild' Doric tested well on the distinction between falling and rising accent on the ultima"; a historical linguist has to resort to the phonetic interpretation of inscriptional evidence such as the appearance of a high back Attic /ū/ (< *eo*, spelled OY) or Ionic diphthong /eu/ (< *eo*) in the Doric inscriptions of the last pre-Christian centuries found in the islands of Rhodes and Cos (Buck [1955: 166–7])[9]. Suppose it really means that certain sections of the Doric population adopted Ionic forms such as καλεῦντες and/or Attic form καλοῦντες and dropped their native καλέοντες. Here we have the case where the dimoric monophthong /ûˉ/ and the diphthong /êu/ with the falling tone in Attic and Ionic correspond to a disyllabic sequence /eo/ in Doric. The correspondence of dimoric monophthong /úˉ/ and the diphthong /eú/ to the disyllabic sequence /eo/ in Doric obtains for instance in the Gen. Pl. :

	Attic	Insular Ionic	East Aegean Doric
Pl. Nom.	καλοῦντες /ûˉ/	καλεῦντες /e͡uˉ/	καλεοντες /eo/
Gen.	καλούντων /úˉ/	καλεύντων /eú/	καλεοντων /eo/
Pl. Nom.	ἄνθρωποι		ἀνθρώποι
Gen.	ἀνθρώπων		ἀνθρώπων

[9] ποιεῦντα Ialysos. 4th or 3rd c. B. C. IG. XII. i. 677. Schwyzer 284.
ποιεῦνται Lindos. 3rd c. B. C. IG. XII. i. 761. Schwyzer 285.

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May we hypothesize that the speakers of East Aegean Doric dialects borrowed even the Attic-Ionic falling and rising tones? The alternative answer, of course, could be that they did not and that they could carry over their native habit of accenting the same syllable in both Nominative and Genitive Pl. as in ἀνθρώποι ~ ἀνθρώπων. Thus we may imagine that in the speech of Doric-Ionic bidialectals the above words could be realized without the falling and rising tone: /kalū̃ntes/ ~ /kalū̃ntǭn/ i. e. with the coincidence of high tone and stress on the penult. Since examples of a negative influence of the bidialectalism and bilingualism on the tone-accent system from various living languages are relatively numerous[10], I am inclined to hypothesize that the Middle Greek stress-accent typology was reached much earlier than the 4th c. A. D. In certain areas—such as Doric Aegean islands and Peloponnese—we may assume that this could be the case in the last pre-Christian centuries.

In any case, the outcome of these processes is fairly well-known—Middle Greek became a stress-accent language and we may note the similarity with the accentual development of the East Slavic languages. Both groups eliminated the length distinctions in the vowel system but in general did not shift its stress-accent from the position of the ancient tone-accent. Thus Modern Greek stress-accent occurs on the same syllable as bore either the circumflex or the acute in Ancient Greek. Even the ancient accentual shift ἄνθρωπος ~ ἀνθρώπου—predictable by the general limiting rule—is continued by the modern phonologically unpredictable shift [ánθropos] [anθrópu]. Similarly, the contemporary distribution of Russian word stress is considered to be a relatively good indicator of the original

ποιεύντω Halasarna (Kos.) 3rd or 2nd c. B. C. Ditt. Syll. 614[3] 1023. Schwyzer 253.

αἰρεύμενοι Antimachia (Kos). 3rd or 2nd c. B. C. DI. 3720. Schwyzer 254.

[10] I know of two synchronic parallels: Ivić (1958: 261) reports that the Ikavian dialect of Istria undergoes drastic changes in its tone-accent typology as a result of Italian-Serbo-Croatian bilingualism of its speakers. For Lithuanian spoken in Philadelphia in a non-tone-accent language environment Robinson (1968: 206) found that the prosodic feature of pitch is nondistinctive.

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accentual position. In marked contrast with the Slavic and Hellenic group of languages, Modern Indo-Aryan languages lost the ancient tone-accent without trace and replaced it by a stress-accent, the location of which is quite unconnected with that of the ancient melodic accentual peak.

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Cohen, Gerald Leonard, ***Origin of -"n" in Greek 1 pl. act. "-men"*** , Indogermanische Forschungen, 84 (1979) p.107

Origin of *-n* in Greek 1 pl. act. *-men*

Abstract

A solution is proposed here for a long-standing problem in IE. morphology, viz. the origin of *-n* in Greek 1 pl. act. *-men*: this *-n* can be plausibly derived from the **-ṃ* (> *-m* after a vowel) that I have earlier reconstructed for Greek 1 pl. mid. (*-metha* < **-medhwṃ*).

The origin of Greek 1 pl. act. *-men* (e. g. *paideu-o-men* = we teach) has long been unclear. Buck (1933: 245), for example, says: 'The *-men* of Attic-Ionic agrees with Skt. secondary *-ma* except for the final *n* (of uncertain source).' Brugmann (1878: 151) writes: 'Von allen Aktivsuffixen ist dieses das Schwierigste', and several leading Indo-Europeanists express their uncertainty through the testimony of silence; they mention Greek *-men* but then pass over the *-n* here without comment (Schwyzer 1939: 662–663; Meillet 1964: 173, 270—'inorganic' *-n*; Meillet-Vendryes 1960: 330). Cf. also Hirt (1928: 148: 'Die Erklärung ist schwierig.') and Szemerényi (1970: 217) who mentions Hittite 1 p.p. *-weni* but remains non-committal about the connection.

Several theories have been set forth on *-men*, but none has won much support. According to one hypothesis, Greek 1 p.p. *-men* is of PIE. origin and corresponds to Hittite 1 p.p. (SE) *-wen/-men*[1]. This view appears in Kronasser (1956: 170), but apparently no other scholars have endorsed it. It is by no menas certain that the Hittite forms go back to PIE., and without the Hittite forms, there is no supporting evidence for the PIE. origin of *-n* in Greek *-men*. The uncertainty of the Hittite situation is expressed by Sturtevant (1951: 141) who after citing the Hittite 1 and 2 pl. endings says: 'But I am not sure of the precise history of the several forms . . .'

[1] The Hittite form is always *-wen* unless preceded by *-u-*, in which case *-men* appears.

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Mention may also be made in passing that a PIE. origin of Greek *-men* was seen by J. Schmidt (1878, 1879, 1880, 1881) who reconstructed 1 p.p. **-mam/*-man* and argued that this form became *-ma* (e. g. OI. *bharema*) in post-tonic position and (Greek) *-men* in tonic position. Schmidt's hypothesis was attacked as ad hoc by Osthoff (1881: 290) and then set aside by Indo-Europeanists. Its only importance now seems to be that it was the first hypothesis to consider the possibility of *-n* < **-m* in Greek *-men*.

The other theories on *-men* view *-n* here as arising from a development within Greek. Schleicher (1871: 231, 652) made the first attempt, with his unsatisfactory suggestion of 'ephelcystic *n*'. Schleicher believed that final consonants in Greek could disappear (therefore: 1 pl. *-mes* > **-me*) and that a new (ephelcystic) consonant could be tacked on (therefore: **-me* > *-men*). But no explanation is offered for the appearance of *-n* here; it is assumed to materialize out of thin air. Later scholars have mercifully ignored Schleicher's suggestion, and its only value now is as a quaint relic of early scholarship on IE. morphology.

A second hypothesis was set forth by Brugmann (1916: 618), who was clearly concerned with not having to reconstruct an additional ending for 1 pl. act.:

'The necessity of reconstructing PIE. **-mem* for Greek *-men* can be avoided by the following hypothesis: there existed in Proto-Greek 1 du. **-wen* with *-n* from *-ton*, *-tan* (2 du., 3 du.), and *-men* arose by analogy with this lost **-wen*. There would then be no need to bring a PIE. nasal ending into discussion.'

This suggestion has also been ignored by the scholarly community, and it is not hard to see why. **-wen* is nowhere attested and has no evidence to support its existence other than 1 pl. *-men*. And the thought of having to ultimately derive the *-n* of a 1 pl. form from the *-n* of a 2, 3 du. ending probably struck most Indo-Europeanists as a bit far-fetched. Note should be made that Brugmann often gave free rein to his imagination in *Grundriß* . . . when dealing with recalcitrant problems. We clearly see in Brugmann's suggestion on Greek *-men* that the spirit from his Young Grammarian days was still very much alive.

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A third hypothesis on the Greek origin of *-n* in *-men* appears in Walter (1923: 18; 1924: 357) and is seconded by Hirt (1928: 148): 1 pl. *-men* < *-me*, reformed after the infinitive in *-men*. No other scholars have endorsed this hypothesis, though, perhaps because there are no other instances known of an infinitive influencing the form of a personal verb ending.

A fourth hypothesis appears in Sommer (1918: 65, note 3), and is attributed by him (in the forward to the second edition, p. VIII) to Rudolph Helm, although no bibliographical reference is given for Helm's suggestion. The hypothesis is that *-n* in *-men* arose by analogy with 1 sg. act. (SE) *-n*, e. g. *epaideu-on* = I taught. Rix (1976: 243–244) supports this view, which does seem to be the best of all the hypotheses set forth. But weaknesses can be spotted even here. It is not clear that changes in 1 pl. were in fact motivated by 1 sg., since there do not seem to be any parallels in other languages to support such an assumption. And as Walter (1924: 357) points out, the only tenses with *-on*, *-en* in 1 sg. are the imperfect and second aorist; this limited use of *-n* in 1 sg. seems to diminish the possibilities of the ending spreading from 1 sg. to 1 pl.

Another Possibility

I would now like to advance a hypothesis of my own: *-n* in *-men* is derived from the **-m* I have earlier (Cohen 1975) reconstructed for 1 pl. mid. A few words of background are necessary here:

The central observation of my 1975 article is that a 2 pl. ending can be tacked onto a 1 pl. ending; such a development is already recognized for Cypriot Greek *-mente* and the Russian 1 pl. imperative *pojdëmte* (= let's go). I then argue that if the same sort of development is assumed for Greek and Indo-Iranian, the door is thereby opened for a solution of several long-standing problems concerning the origin of med.-pass. verbal endings. For example, Greek *-tha* in 1 pl. *-metha* can be reconstructed back to **-dhwm̥*, which in its *-o-* grade is PIE. **-dhwom* (OI. *-dhvam*). Greek **-methom* first arose (as 1 pl. mid; it is attested

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rarely, in its later from *-methon*, and only after its shift to 1 du.). Later, the zero grade (**-methm̥*) gave rise to *-metha*.

It was either the syllabic sonant **-m̥* of **-methm̥* or the *-m* of **-methom* that spread from the med.-pass. over to the active, resulting in *-men*. If linguists had previously been aware that *-a* in *-metha/-mestha* probably came from **-m̥*, I'm sure the hypothesis I have suggested here would have been advanced long ago.

As for the possibility of levelling between active and med.-pass. forms, cf. the *-e* of 2 pl. med.-pass. *-sthe*, which is already recognized as deriving from the *-e* of the active *-te* (2 pl.).

It is also possible that the above discussion on the shift of a 2 pl. ending to 1 pl. can be used to shed light on the very difficult problem of the origin of *-n* in Hittite 1 pl. *-men/-wen* and 2 pl. *-ten*. The problem here is that no firm starting point has been established for the *-n* in either ending. Is *-n* in Hittite *-ten* of PIE. origin, as Burrow (1965: 308) has suggested—by relating Hittite *-ten* to Vedic 2 pl. *-thana*?[2] Or does Hittite *-ten* derive from **-te*, with *-n* spreading from 1 pl. *-men/-wen*, as Neu (1968:124) has proposed? And if Neu is correct, what is the origin of *-n* in the 1 pl. form? (He does not relate it to Greek *-men*, although he does regard Hittite *-men* as 'old' (p. 123) and hence presumably deriving from PIE.).

My work on verbal endings has made me very much aware that a 2 pl. ending may spread to a 1 pl. ending, and I will make this observation the central point in my attempt to disentangle the problems cited in the above paragraph. Such a move leads me to consider Burrow's hypothesis—that Hittite *-ten* and Vedic *-thana* are cognate—as a view which harmonizes well with my own approach. If these two forms are in fact cognate, I would state that Hittite 1 pl. *-men/-wen* seem to present one more instance of the spread of a 2 pl. ending to a 1 pl. ending; the *-en* here is apparently derived from 2 pl. *-ten*, although whatever originally followed Hittite *w-/m-* in 1 pl. dropped out after *-en* was added.

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[2] The traditional view prior to Burrow's work was that Vedic *-thana* consisted of *-tha-* and a *-na* particle (e. g. Brugmann 1916: 624).

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One can, of course, argue that the opposite shift proposed by Neu (1 pl. ending tacked onto a 2 pl. ending) is also possible, but I would urge great caution here. First, this shift is nowhere unambiguously attested, whereas the shift of a 2 pl. ending onto a 1 pl. one is clearly attested at least in Russian *pojdëmte* and Cypriot Greek *-mente*. And secondly, Neu's proposal rests on the assumption that the *-n* in Hittite *-men/-wen* is of PIE. origin, and such an assumption is currently, at least, very shaky.

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Three Homeric epithets: αἰγίοχος, διιπετής, κορυθαίολος

αἰγίοχος

At the end of his great edition of the Works and Days, 1978, M. L. West deals in an excursus with some names and epithets of gods. He discusses αἰγίοχος in pp. 366–368, and I first give a summary of his discussion. The traditional explanation of αἰγίοχος is wrong on grounds of formation and sense. The form with this meaning would be αἰγιδοῦχος or at least αἰγιδόχος. Even though it is now generally thought that the second element is related to ϝέχω, and not to ἔχω, the translation 'bearing the aegis' persists. The translation is not even appropriate to the contexts, since the aegis is more often associated with Athena than with Zeus. We should analyse αἰγίοχος as αἰγί-ϝοχος, 'riding on a goat' or 'driving a goat-chariot'. But Zeus has nothing to do with goats; αἴξ should therefore be understood as the name of a bird. In Germany and Lithuania the flight of a snipe known as 'Heaven's Goat' or 'God's Goat' presages a storm. Aristotle speaks of a bird called αἴξ; this is not itself a snipe, but, on the assumption that at some point the word αἴξ was applied to the snipe, poets might have conceived of Zeus, the storm-god, riding in his chariot behind the αἴξ.

So much for West's argument. I cannot share his belief that he has arrived at a more plausible interpretation of the word αἰγίοχος than any previously offered. It is, of course, a purely subjective matter whether the picture of Zeus riding through the air drawn by a team of snipe is credible or not. But, whatever one may think of the conclusion, the steps leading to it may well be called into question. It is unfortunate, though not in itself fatal to the argument, that the word αἴξ meaning a type of bird is not attested until Aristotle. More disquieting is the fact that when αἴξ, 'type of bird', is finally found it means a bird of a kind other than the one postulated. I cannot help thinking that it

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would be possible to account for almost any shift in meaning if one allowed oneself such immense latitude.

Nevertheless, I am glad that αἰγίοχος has come up for renewed discussion, since I believe that modern interpreters of the word have taken a wrong turning. I can show this best by considering West's remarks about the form and use of the word.

So far as the form is concerned, West's objection to αἰγίοχος in the sense 'aegis-bearing' is not cogent. When he says that a word bearing this sense 'would be αἰγιδοῦχος', he is in fact assuming that all the principles of word-formation in the Homeric Kunstsprache are sufficiently well understood to enable us to predict what the form required in any situation will be. But we are very far from having reached that state of knowledge. What evidence is there that the stem of αἰγίς was αἰγίδ-, and could never be anything else? It is true that when Homer uses oblique cases of the word the stem always appears as αἰγίδ-; but in the case of αἰγίς, as in that of some other words, we have no means of knowing whether the form with or without -δ- is the original one. To take two actual examples: the accusative of ἔρις appears both as ἔριδα (ζ 92 etc.) and as ἔριν (γ 136 etc.), that of ὄπις as ὄπιδα (ξ 82 etc.) and ὄπιν (φ 28). It is impossible to say whether the stem of these words 'is' ἐρι- or ἐριδ-, ὀπι- or ὀπιδ- respectively. Thus the presence in Homer of accusative αἰγίδα and dative αἰγίδι gives no guarantee that in other contexts the stem might not appear as αἰγι-.

West rightly says that the second element of αἰγίοχος is to-day usually associated with ϝεχ-/ϝοχ- and not with ἔχω; he is right, again, to draw attention to the fact that, if its second element is indeed ϝοχ, αἰγίοχος cannot very well have the meaning 'aegis-bearing'. It is in this regard, I submit, that the false parallel has been drawn. Because γαιήοχος contains the element ϝοχ, it is considered that αἰγίοχος must contain it as well. I do not see the force of this argument[1]. I associate the second element not with ϝοχ- but with ἔχω and see the compound αἰγίοχος as having been formed on the basis of Homeric expressions such

[1] Or, rather, assertion; Bechtel certainly does not argue the point, but simply states it, Lexilogus zu Homer, 1914, 17.

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as αἰγίδ' ἔχουσ' (B 447), αἰγίδ' ἔχων (O 361), ἔχε δ' αἰγίδα (O 308), αἰγίδα χερσὶν ἔχ' (O 318); compare also αἰγίδ' ἀνέσχεν (χ 297).

If, then, the traditional meaning of αἰγίοχος can be restored by dissociating it from formations like γαιήοχος, is the sense 'bearing the aegis' repugnant to Homeric usage? I suggest that it is not. Athena certainly 'bears the aegis' more often than Zeus does; and, in one of the scenes of fighting in the Iliad, Apollo too takes part bearing the aegis (O 229, 308, 318, 361). The association of Athena and Apollo with the aegis can be accounted for on the simple ground that those gods often come down to earth and participate in the battles of men, and so naturally they have more occasion to carry, and to brandish, the aegis. The places in which αἰγίοχος is used disclose the manner in which this epithet came to be applied to Zeus, and not to Apollo or Athena. The intimate connexion between these gods and 'aegis-bearing Zeus' is brought out in the following lines:

αἴ κέν μοι δώῃ Ζεύς τ' αἰγίοχος καὶ Ἀθήνη (Θ 287).
ὃν περὶ κῆρι φίλει Ζεύς τ' αἰγίοχος καὶ Ἀπόλλων (ο 245).

It is noteworthy that, out of the 52 other occurrences of αἰγίοχος, no fewer than 21 describe Athena as the daughter of aegis-bearing Zeus:

αἰγιόχοιο Διὸς τέκος (A 202, B 157, E 115, 714, Θ 352, 427, K 278, Φ 420, δ 762, ζ 324).
θύγατερ Διὸς αἰγιόχοιο (E 815).
κούρη Διὸς αἰγιόχοιο (E 733, Θ 384, K 553, γ 42, 394, δ 752, ν 252, 371, ω 529, 547).

The close co-operation between Zeus and his children has been responsible for the transference of the idea of 'aegis-bearing' from them to him.

διιπετής

In the Homeric poems, this epithet is applied only to rivers: to the Spercheios (Π 174), to 'a river' in a simile (P 263), to the Scamander (Φ 268, 326), to the Aigyptos (δ 477, 581), and to the river near which Odysseus rested after his arrival in Phaeacia

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(η 284). Neither the meaning nor the derivation of the word is known for certain.

The meaning was a matter of dispute in antiquity. Some scholars thought it meant 'bright, clear' (comparing Eur. Bacchae 1267), others 'falling from Zeus'. These opinions are set out in the scholia on Π 174:

> διιπετέος· λαμπροῦ. καὶ Εὐριπίδης· λαμπρότερος ἢ πρὶν καὶ διιπετέστερος . . . (ΣT)[2].
> διιπετέος· ἤτοι ἀπὸ Διὸς πεπτωκότος, διὰ τὸ ὑπὸ τῶν ὀμβρίων ὑδάτων πληροῦσθαι τοὺς χειμάρρους, ἢ διαφανοῦς (ΣA).

Of these explanations, 'falling from Zeus' has found the greater favour among modern writers. The correctness of the interpretation cannot, however, be confirmed unless it is proved that the first element of the compound, διῑ̄-, has the function of an ablative; and it is in the ambiguity of this element that the difficulty of analysing the word chiefly resides. Does διῑ̄- arise from 'metrical lengthening'[3], is it a form of Διεί[4], or has it to be explained in some quite different way? M. Treu thinks that διιπετής cannot mean 'falling from Zeus' or 'fallen from heaven'. He regards διῑ̄- as a spelling of διαι-, basing his observation on Alcman fr. 3. 67 (Page), where a star is described as ὠρανῶ διαιπετής; the whole compound he regards as a derivative of διαπέτομαι, 'fly through, traverse', and believes that it was only later re-interpreted, by false etymology, as Διί + πέτομαι[5].

H. Lüders, who, like Treu, associates -πετ- with πέτομαι, 'fly', takes a different view of the first element. This he thinks is locative and concludes that the compound means 'flying in heaven'. By means of this explanation, Lüders reconciles the Homeric occurrences of διιπετής with its use at h. Hom. Ven. 4:

> οἰωνούς τε διιπετέας καὶ θηρία πάντα

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[2] The rest of the scholion is corrupt: see Erbse's edition ad loc.

[3] As Schulze thought, Quaestiones Epicae, 1892, 241.

[4] So W. F. Wyatt, Metrical Lengthening in Homer, 1969, 237 n. 3.

[5] Glotta 37 (1958) 260–275. Alcman's διαιπετής and its relationship to διιπετής are investigated further by R. Renehan, Rhein. Mus. 115 (1972) 93–96.

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and also with the Vedic concept of rivers flowing by command of Varuṇa and flying through the air like birds[6].

Rüdiger Schmitt agrees with Lüders that in origin διιπετής can have meant only 'flowing in heaven'. He thinks that, as a result of Lüders' argumentation, this is shown to have been a word which descended from Indo-European poetry; but its original sense had disappeared by the time of Homer, who employed it as a meaningless epithet of rivers[7].

H. Humbach assumes the presence of two distinct words: διοπετής (re-fashioned from διαι-), meaning 'fallen from heaven', and διειπετής, the first element of which is identical with the stem of διερός and, in consequence, means 'swiftly hurrying'[8].

It seems necessary to agree with Treu that διιπετής cannot mean 'falling from heaven', since there is no convincing way in which the element διῑ- can indicate 'direction from'. At the same time, Treu's explanation, though possible, cannot be regarded as certain, based as it is entirely upon the one passage in Alcman. The hypothesis remains without verification, mainly because of the serious objection brought by Humbach, namely that, while 'sent from Zeus' could be expressed in Greek by (ἐκ) Διός, the phrase 'existing in Zeus' cannot be rendered by (ἐν) Διί[9]. Humbach's own suggestion is the most persuasive so far offered, giving a plausible sense to διιπετής both in Homer and in the Homeric hymn and, above all, avoiding the unpleasant necessity of regarding it as a 'meaningless epithet'. Although no final judgment can be made, I think it reasonably certain that διιπετής has nothing to do with Διός and that the application of this word to rivers in Homeric poetry does not, in itself, indicate that rivers were felt to have their origin in Zeus[10].

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[6] Varuṇa I, ed. L. Alsdorf, 1951, 141–143. This view is endorsed by V. Pisani, Lingue e Culture, 1969, 361–362.

[7] Dichtung und Dichtersprache in idg. Zeit, 1967, 234–236.

[8] KZ. 81 (1967) 276–283; cf. O. Szemerényi, Journ. of Hellen. Stud. 94 (1974) 144–145. For διερός, 'quick', see especially P. Ramat, Quad. dell'Ist. di Glottologia Bologna 7 (1962) 23–33.

[9] KZ. 81 (1967) 278.

[10] Or, I should say, not all rivers, since at Ξ 434 Zeus is said to have begotten the Xanthos.

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κορυθαίολος

The helmet of Hector is discussed by D. L. Page in his History and the Homeric Iliad, 1959, pp. 248–251. Page infers from the restriction to Hector 'in the traditional language' of the epithets κορυθαίολος and χαλκοκορυστής that in the tradition the Greek helmet was not made of metal and that Hector, alone among the Greek and Trojan heroes, wore a bronze helmet. The meaning of κορυθαίολος as 'shaking the helmet', 'moving the helmet quickly', though frequently given by the authorities (Ebeling, Bechtel, Frisk, LSJ), must be wrong, because the helmet was never so habitually shaken as to call for a special descriptive epithet; hence κορυθαίολος can mean nothing else than 'with flashing helmet'.

Exactly what Page means by 'the traditional language' is not explained with the requisite clarity. Several times in the Iliad the helmets of Greeks, as well as those of Trojans other than Hector, are said, or are implied, to be made of bronze. On the Greek side, we are told that the Locrians did not possess bronze helmets (N 714); from which observation we may surely infer that, if they had had helmets, these would have been made of bronze. Again, Poseidon refers to the 'all-gleaming' helmets of the Greeks (Ξ 372). The Greeks who stand over Patroclus' corpse have 'bright helmets' (P 269). The helmets brought out of the Greek ships were 'gleaming brightly' (T 359). And Achilles has a 'bright helmet' (X 314). On the Trojan side, the warriors Damasos (M 184), Dolops (O 535), and Demoleon (Υ 398) all go to battle with bronze helmets. On both sides, there is seen the gleam from shining helmets (N 341). Are all of these allusions to bronze helmets couched in non-traditional language? And, if they are, how is their language to be distinguished from the formula containing κορυθαίολος?

Page's objection to the meaning 'shaking the helmet' cannot be sustained. In the first place, his contention that an epithet with this meaning would not have evolved in the epic language is disproved by the use of κορυθάιξ at X 132 to describe Enyalios; and the second part of that compound is formed from the stem of ἀίσσω, 'move vigorously'.

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Hooker, J. T., *Three Homeric epithets: "aigiochos, diipetes, korythaiolos" [Greek]* ,
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In the second place, it does not follow that, because the only hero to be given the epithet κορυθαίολος is Hector, this epithet could not be applied to other heroes. The point has been expressed so well by Miss Gray that I simply quote her remarks:

> One traditional association of epithet and noun need mean no more than that the epithet was at some time appropriate to the whole category, as the traditional description of Peleus as ἱππήλατα does not imply that the other heroes were unskilled charioteers but that skill in driving chariots was characteristic of all heroes[11].

In the third place, an analysis of the word κορυθαίολος shows that a meaning 'shaking' cannot be excluded for the second element. The word αἰόλος itself, in many of its Homeric uses, presents a meaning which seems to embrace both that of 'bright, flashing' and that of 'moving quickly'; for instance when αἰόλος is applied to armour (E 295), a shield (H 222, Π 107), wasps (M 167), or a snake (M 208.) But on other occasions the meaning is, without question, 'moving quickly'. Thus we have a horse who moves his hoofs quickly (T 404), writhing worms (X 509), and a darting gadfly (χ 300). These three examples suffice to show that the original sense of αἰόλος was 'moving quickly'; and, for all we know, the word always has this meaning in Homer[12], since there is no Homeric passage in which it must mean 'gleaming'[13]. But if in Homer αἰόλος always may, and sometimes must, mean 'moving quickly', it follows that no attempt to associate κορυθαίολος exclusively with the secondary meaning of αἰόλος is likely to carry entire conviction.

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[11] D. H. F. Gray, Class. Quart. 61 (1947) 117 n. 1.

[12] A. Ronconi discusses the post-Homeric use of the word, in the light of Homeric usage, at Stud. It. di Fil. Class. 14 (1937), 249–251. The Homeric passages are examined in the course of Erna Handschur's Vienna dissertation, Die Farb- und Glanzwörter bei Homer und Hesiod, 1970, 111–112.

[13] A similar transition in meaning from 'move quickly' to 'glitter, gleam' is seen in Latin micare.

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Planudea

1. Introduzione

Concludendo un rilevante saggio[1] sulla teoria casuale localistica[2] a suo giudizio rintracciabile già in Massimo Planude, il Robins alcuni anni or sono sottolineava la possibilità che costui ed altri grammatici bizantini riservassero per il futuro delle sorprese non sottovalutabili in rapporto anche alle teorizzazioni metalinguistiche attuali[3]. Provando noi interesse per il binomio grammatica classica / ipotesi localistica planudea, intenderemmo ora indugiare brevemente su alcuni punti soprattutto del Περὶ συντάξεως[4] che ci pare possano destare un qualche interesse in

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[1] R. H. Robins, The Case Theory of Maximus Planudes, in: L. Heilmann (ed.), Proceedings of the Eleventh International Congress of Linguists (Bologna 1974) 107–111.

[2] Nella sua formulazione odierna essa è opera del semanticista inglese J. M. Anderson, il quale ha dedicato numerosi saggi ed opere al problema. Tra di essi possiamo ricordare: J. M. Anderson, Ergative and Nominative in English, Journal of Linguistics 1968, 1–32; Id., The Grammar of Case: Towards a Localistic Theory (London-New York 1971), che si può considerare il testo fondamentale; Id., An Essay concerning Aspect: Some Considerations of a General Character arising from the Abbé Darrigol's Analysis of the Basque Verb (The Hague-Paris 1973), alla riscoperta delle teorie dell'inizio dell'Ottocento utilizzabili ancor oggi, mutatis mutandis; Id., Maximi Planudis in memoriam, in F. Kiefer-N. Ruwet (eds.), Generative Grammar in Europe (Dordrecht 1973) 20–47, il cui titolo può ingannare poiché si fa solo un breve accenno al grammatico bizantino ed agli studiosi della Philological Society di Londra.

[3] "Very probably Maximus and some other Byzantine grammarians deserve further reexamination in the light of ancient scholarship and modern theory" (p. 110).

[4] ΜΑΞΙΜΟΥ ΤΟΥ ΠΛΑΝΟΥΔΗ, Περὶ συντάξεως, in: L. Bachmann (ed.), Anecdota Graeca II (Leipzig 1828) 105–166. Diciamo "soprattutto", perché faremo qualche riferimento anche all'altra opera grammaticale del medesimo Planude, il Περὶ γραμματικῆς διάλογος, reperibile sempre nel testo del Bachmann, alle pp. 3–101. La biblio-

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coloro che ricercano con la dovuta cautela e senza apriorismi nell'antichità delle anticipazioni intuitive — sia pure parziali — e dei "precursori" rispetto alle formulazioni contemporanee[5].

2. Il proposito di "speculare" sulla lingua

Si può dire che in tutte le epoche si sia sentito il desiderio di speculare sulla lingua, a partire da alcuni grammatici classici

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grafia specifica relativa a questi due trattatelli è nulla; tra quella più generica, dopo accurate ricerche abbiamo potuto reperire solo il breve (e non eccessivamente approfondito) paragrafo di C. Wendel, Maximus Planudes, RE. XX, 2 (1950), col. 2209.

[5] Ciò significa che non intendiamo assolutamente — e lo sottolineiamo — ricercare "anticipazioni illuminate" ad ogni costo, ma cercare di cogliere la specificità ed il sottile rapporto d'implicito legame che talora può rapportare speculazioni di epoche differenti. Contro forzati collegamenti si pronuncia giustamente R. Simone, Storia linguistica e storia della linguistica, in: Società di Linguistica Italiana, Teoria e storia degli studi linguistici I (Roma 1975) 111–150.

Tra i grandi studiosi che hanno già affrontato questa via della riscoperta — sia pure talora non obiettivamente, a giudizio del Simone — possiamo ricordare relativamente al cartesianismo Chomsky (N. Chomsky, Cartesian Linguistics. A Chapter in the History of Rationalist Thought, New York-London 1966) e le giuste rettifiche del Rosiello (L. Rosiello, Linguistica illuminista, Bologna 1967); con riguardo alle ipotesi portorealistiche il Brekle (H. E. Brekle, Semiotik und linguistische Semantik in Port-Royal, IF. 69, 1964, 103–121; Id., Die Bedeutung der 'Grammaire générale et raisonnée' — bekannt als Grammatik von Port-Royal — für die heutige Sprachwissenschaft, IF. 72, 1967, 1–21) e le riserve mosse dal Simone (R. Simone (ed.), Grammatica e Logica di Port-Royal, Roma 1969, "Introduzione", VII-L); e per gli speculativi medioevali a Bursill-Hall (G. L. Bursill-Hall (ed.), [Thomas of Erfurt] Grammatica speculativa, with translation and commentary by G. L. B.-H., London 1972, "Introduction", 117–126) ed altri suoi studi citati in bibliografia al predetto volume, e a Salus (P. H. Salus, The Modistae as generative grammarians, in: L. Heilmann (ed.), Proceedings . . ., cit., pp. 117–120).

Del resto lo stesso Robins nelle sue due opere più famose (R. H. Robins, Ancient and Mediaeval Grammatical Theory in Europe, London 1951; Id., A Short History of Linguistics, London 1967; trad. ital.: Bologna 1971) aveva tentato di rapportare alcune osservazioni di grammatici antichi o d'età comunque anteriore alla nostra alle ipotesi d'oggi.

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antichi (Varrone soprattutto) fino alle recenti ipotesi di "grammatica universale" e di "logica intensionale". Una caratteristica propria della grammatica antica greca e latina è invece quella di dare la preminenza più all'aspetto descrittivo, tassonomico e puramente classificatorio dei fenomeni linguistici, rispetto ad una complementare (o prioritaria) riflessione che sappia anche sollevarsi al di sopra dei puri fatti nel tentativo di giustificarli o generalizzarli[6]. Si verifica poi raramente che un grammatico classico dichiari esplicitamente di voler teorizzare sui dati linguistici. Ciò è invece coglibile nel Περὶ συντάξεως planudeo, dove a p. 112,5–8 si sostiene di voler investigare (ζητεῖν) in rapporto all'ordine (τάξις) dei casi, ai generi, ai tempi ed alle parti del discorso. Infatti — aggiunge l'autore — δεῖ καὶ ἡμᾶς περὶ αὐτῶν θεωρῆσαι καὶ ἐξετάσαι[7].

Il passo è significativo — oltre che per il tema relativamente al quale oggi il grammatico bizantino è conosciuto, l'ipotesi

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[6] Tali eccezioni riguardano l'originalità di Varrone (D. T. Langendoen, A Note on the Linguistic Theory of M. Terentius Varro, Foundations of Language 1966, 33–36; R. H. Robins, A Short History . . ., cit., p. 74 soprattutto; F. Murru, L'accusativo in Varrone, Durius 1980); e quella dei teorizzatori del cosiddetto *octavus casus* (su cui vedi: F. Murru, Alcune questioni filologico-linguistiche a proposito dell'*octavus casus*, Glotta 1978, nn. 1–2, 144–155; id., Due ulteriori definizioni dell'*octavus casus* nei grammatici latini, ibid. 1979, 155–157).

[7] In realtà dalle nostre ricerche è emerso tuttavia che Massimo Planude in questo segue da vicino Prisciano. Infatti il grammatico latino dice: *sic etiam de ordinatione casuum et generum et temporum et ipsarum partium orationis solet quaeri. restat igitur de supra dictis tractare, et primum de ordinatione partium orationis, quamvis quidam suae solacium imperitiae quaerentes aiunt, non oportere de huiuscemodi rebus quaerere* (Inst. gramm. XVII § 12, GL. III p. 115). In ciò eravamo stati parzialmente aiutati da C. Wendel, art. cit., col. 2209, il quale rimanda al Buttmann (A. Buttmann, Des Apollonios Dyskolos vier Bücher über die Syntax, 1877, pp. XXVI–XXXII, da noi non trovato), che diceva che "das Werk des P. nicht unmittelbar auf Apollonios lehnt, sondern geradezu eine Rückübersetzung aus dem 17. Buche der auf Apollonios fußenden Institutio Grammatica Priscians darstellt, in der nur für das Griechische ungeeignete Partien ausgelassen und die lateinischen Beispiele durch griechische ersetzt sind". Però il Wendel non si pronuncia né a favore né contro la predetta ipotesi, che a nostro parere è certo vera in generale.

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stativo-direzionale — per il fatto che viene introdotta la nozione di τάξις, sulla quale ritorneremo nel § 5. Inoltre l'utilizzazione dei due verbi θεωρεῖν ed ἐξετάζειν — non ricorrenti in nessun altro grammatico greco[8] — fa sì che venga sottolineato almeno in teoria il carattere non puramente espositivo della presentazione grammaticale di Massimo Planude. Ciò evidentemente non toglie che in seguito — messi da parte i buoni propositi — lo studioso medioevale ritorni propriamente a seguire le sue fonti, in una presentazione puramente scolastica ed informativa.

3. La struttura minimale della frase

Il tema nella linguistica attuale è naturalmente legato al problema dei gruppi (o sintagmi) minimali frasali, i quali possono essere soggetti ad espansioni per quanto concerne il SN, il SV od entrambi. Esso è stato affrontato direttamente dal tassonomismo bloomfieldiano, e più specificamente dal funzionalismo martinetiano.

In Massimo Planude si pone l'accento piuttosto sul problema dell'accettabilità logico-sintattica dell'enunciato nei confronti dell'eventuale omissibilità (l'odierna *deletion*) di una sua parte. A p. 112, 24–25 il grammatico bizantino sostiene che la frase (ὁ λόγος) risulta priva (ἐλλιπής) di qualcosa, e quindi inaccettabile, qualora non sia formata nella struttura minimale dalle sue due parti essenziali e necessariamente onnipresenti, l'ὄνομα ed il ῥῆμα. Ciò sarebbe esemplificabile in: Ὁ αὐτὸς ὑπολισθήσας

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[8] Relativamente ad asserzioni di carattere generale; su punti specifici invece il verbo ζητεῖν compare in Apollonio Discolo, Περὶ ἀντωνυμίας, GG. II 1 p. 70, 9–10 e 73, 18; ἐξετάζειν ricorre in Apollonio Discolo, Περὶ ἀντωνυμίας, GG. II 1 p. 79, 13, nel Περὶ συντάξεως, GG. II 2 p. 178, 11. Θεωρεῖν non è attestato in nessun passo dei Grammatici Graeci. Invece i tre verbi sono molto utilizzati da Aristotele, in accezione logico-filosofica: vedi H. Bonitz, Index Aristotelicus, Berlin 1870, s. vv. ἐξετάζειν (p. 260), ζητεῖν (p. 309) e θεωρεῖν (p. 328). E Massimo Planude dà l'impressione nei suoi due trattatelli grammaticali di tenere presente un poco la logica peripatetica, oltre a Prisciano. D'altra parte si osservi che a sua volta il grammatico latino dichiara di seguire come fonte Apollonio Discolo ed Erodiano (cfr. GL. III p. 195).

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σήμερον ἔπεσεν (con assenza di ἄνθρωπος); e in: Ὁ ἄνθρωπος ὑπολισθήσας σήμερον (con mancanza di ἔπεσεν)[9].

Mentre l'ὄνομα ed il ῥῆμα non sarebbero omissibili, ciò è possibile per il patticipio (μεσοχή: p. 112, 30sgg.) — ὁ αὐτὸς ἄνθρωπος σήμερον ἔπεσεν —, per la preposizione (πρόθεσις) e per il pronome (ἀντωνυμία): in relazione a quest'ultimo il Planude è costretto poi precipitosamente ad ammettere che esso può risultare anche indispensabile — per esempio in ἐγὼ τρέχω — sulla base di una giustificazione poco semantica e molto etimologica, considerando il pronome come elemento ἐν τῇ χώρᾳ τοῦ ὀνόματος (p. 113, 1)[10].

Ciò che manca non poco alla esposizione del filologo bizantino (ed alla sua fonte latina) è il fatto che non venga riservato nessun cenno alla relazione tra frase "essenziale" e contesto precedente, che talora potrebbe comportare anche la regolata trascuratezza o dell'ὄνομα o del ῥῆμα: per esempio in italiano in *chi l'ha detto? Io.*

4. L'ordine delle parole

In greco — secondo il grammatico medioevale — l'ordine di sequenza "normale" è quello che prevede la serie ὄνομα → ῥῆμα

[9] Però nel Περὶ γραμματικῆς διάλογος (p. 93) il Planude sostiene che è possibile talora omettere — come sottintesi concettualmente — o il nome o il verbo. Così si ometterà l'ὄνομα γῇ in una frase del tipo σπείρειν τῇ γῇ, il verbo in σοφοὶ γὰρ τὰ ναυτικά con θεωροῦσι sottinteso. Approfittiamo dell'occasione per notare che le due operette planudee presentano talora delle analogie non indifferenti su singoli aspetti grammaticali trattati in entrambe.

[10] Purtroppo anche sull'aspetto trattato in questo paragrafo Massimo Planude segue da vicino Prisciano: *si tollas nomen aut verbum, imperfecta fit oratio; sin autem cetera subtrahas omnia, non necesse est orationem deficere, ut si dicas: 'idem homo lapsus heu hodie concidit', en omnes insunt partes orationis, absque coniunctione, quae si addatur, aliam orationem exigit. ergo si tollas nomen aut verbum, deficiet oratio, desiderans vel nomen vel verbum, ut si dicam 'idem lapsus heu hodie concidit' vel 'idem homo lapsus heu hodie'; sin subtrahas adverbium, non omnino d.ficiet oratio, ut 'idem homo lapsus heu concidit'. nec non etiam participium si adimas, neque sic deficiet, ut: 'idem homo heu hodie concidit', nec si praepositionem et interiectionem: 'idem homo concidit', nec si etiam pronomen: 'homo cecidit'. non tamen dico, quod non etiam ex pronomine et verbo perfecta constat oratio, cum dicimus 'ego ambulo, tu ambulas'. tum enim constat perfectio orationis, cum loco nominis sumatur pronomen et vim nominis compleat ordinatione* (GL. III p. 116).

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(p. 113, 7sgg.). Questa asserzione, ricavata dall'osservazione diretta dell'uso linguistico, è certo esatta, ma solo se considerata percentualmente in quanto è noto che le lingue flessive possono ammettere dei *word-orders* differenti[11]. I criteri espliciti che vengono portati a comprova dell'asserzione da parte del Planude sono però del tutto logico-nozionali (cioè extralinguistici), con riferimento all'identità bidirezionale οὐσία = ὄνομα. La qualità essenziale dell'ὄνομα sarebbe infatti quella dell'ἐνέργεια o del πάθος, la quale determinerebbe la disposizione del nome in rapporto al verbo, il quale — per riflesso — esprimerebbe l'ἐνεργεῖν o il πάσχειν. Il ragionamento è tratto tale e quale dalla logica aristotelico-peripatetica e si ritrova anche in Prisciano[12].

Sarà pure inutile da parte del linguista odierno andare alla ricerca di "Agentivi" o di "Pazienti" *ante litteram*, del tipo di quelli enunciati da C. J. Fillmore[13], per il semplice e lampante

[11] Sul tema si veda inizialmente: R. T. Lakoff, Abstract Syntax and Latin Complementation, Cambridge (Mass.) 1968, 100–101; H. Pinkster Review Article di Lakoff 1968, Lingua 1971, 392–393; sempre utile è J. H. Greenberg (ed.), Universals of Language, Cambridge (Mass.) 1966, 76–89. Nell'ambito della filologia classica: P. Linde, Die Stellung des Verbs in der lateinischen Prosa, Glotta 1923, 153–178; J. Marouzeau, L'ordre des mots en latin (Paris 1953); R. Kühner-C. Stegmann, Ausführliche Grammatik der lateinischen Sprache (Leverkusen 1955) II, 597sgg.; A. Ernout-F. Thomas, Syntaxe latine (Paris 1964) 161; J. B. Hofmann-A. Szantyr, Lateinische Syntax und Stilistik (München 1965) 401–406. Lo stesso Marouzeau ha dedicato più di una decina di articoli al tema (su "Revue des études latines", "Lingua", "Latomus").

[12] *Ante verbum quoque necessario ponitur nomen, quia agere et pati substantiae est proprium, in qua est positio nominum, ex quibus proprietas verbi, id est actio et passio, nascitur* (GL. III, p. 116).

[13] Ricordiamo: C. J. Fillmore, The Case for Case, in: E. Bach-R. T. Harms (eds.), Universals in linguistic Theory (London-New York 1968) 1–88; Id., Types of Lexical Information, in: D. D. Steinberg-L. A. Jakobowits (eds.), Semantics (Cambridge 1971) 370–392; Id., Some Problems for Case Grammar, in: C. J. Fillmore (ed.), Working Papers in Linguistics, n. 10, Department of Linguistics, Ohio State University (1971) 245–265; Id., The Case for Case Reopened, in: P. Cole-J. M. Sadock (eds.), Syntax and Semantics. Vol. 8: Grammatical Relations (New York 1977) 59–81. Si veda pure: F. Murru, Il latino e la 'grammatica dei casi': un *excursus* ed alcune proposte in relazione al problema della subordinazione, IF. 82 (1977) 142–154.

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motivo che questi ultimi rientrano nel settore semantico metalinguistico, mentre l'attività o la passività planudee sono palesemente referenziali al pensiero.

5. Osservazioni sulla presunta analogia di successione tra serie avverbiale locale e serie casuale

Riteniamo opportuno riportare due brevi passi del Περὶ συντάξεως, prima della discussione.

1) Εἰ ζητοῦμεν τὸν τόπον, ἐν ᾧ τι γίνεται, τριχῶς ποιοῦμεν τὴν ἐρώτησιν, ποῦ ἢ πόθε; καὶ πῇ ἢ πόσε; καὶ πόθεν; (p. 121, 28–30).

2) Καὶ σημειωτέον ἐνταῦθα ὅπως κατά τινα φυσικὴν ἀκολουθίαν αἱ τρεῖς αὗται ἐρωτήσεις, τὸ πόθεν καὶ ποῦ καὶ πῇ, τὰς τρεῖς πλαγίας ἐκληρώσαντο πτώσεις· τὸ δὲ πόθεν τὴν γενικήν, τὸ δὲ ποῦ τὴν δοτικήν, τὸ δὲ πῇ τὴν αἰτιατικήν (p. 122, 23–27).

Il Robins ha giustamente evidenziato il ragionamento analogico avanzato dal Planude tra avverbi interrogativi di luogo e casi della declinazione[14]. Come per gli avverbi si presenterebbe la serie nozionale "movimento da" → "stato in" → "movimento verso", così pure avverrebbe per la declinazione nominale (genitivo → dativo → accusativo): e l'identità si baserebbe sulla successione nozionale identica movimento da → stato in → movimento verso. Relativamente alla sequenza avverbiale, il Robins (pp. 109 e 110) sottolinea il fatto che — a parte l'evidenziazione dell'ipotesi localistica (su cui rimandiamo al paragrafo successivo) — il Planude sarebbe originale per il fatto di allontanarsi dal suo maestro ideale, Apollonio Discolo, il quale invece proponeva la serie avverbiale stato in → movimento verso → movimento da. Ed in realtà parrebbe che le cose stiano effettivamente in questi termini, se ci si basa su quanto asserisce lo studioso inglese, e se si considera solo il secondo brano (da lui menzionato).

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[14] In Prisciano non si trova il riferimento diretto ai casi: *localia et temporalia adverbia omnes generaliter species localium vel temporalium tam adverbiorum quam nominum sibi subiungunt, quae tam interrogativa sunt quam relativa, quattuor differentias habentia: ad locum, ut 'quo', in loco, 'ubi', de loco, 'unde', per locum, 'qua'* (GL. III, p. 133). Si noti che nel grammatico latino c'è l'ordine: direzionalità verso → stativitá → direzionalità da → direzionalità attraverso.

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Se invece prendiamo in considerazione anche il primo, ne ricaviamo una impressione differente: il grammatico bizantino segue anche il testo di Apollonio Discolo[15] finché il suo ragionamento non viene in contrasto con lo scopo prefisso, quello di mostrare l'analogia di sequenza nozionale tra serie avverbiale di luogo e serie casuale declinazionale. A questo punto egli interviene palesemente sul testo e lascia spazio ad una delle sue "intrusioni", in modo da aggiustare le cose. Ma egli non si accorge più dell'antinomia tra i passi di poco distanti.

In questo caso quindi è la necessità e non tanto l'originalità a manifestarsi.

6. Massimo Planude parla in termini espliciti di ipotesi localistica?

La domanda potrebbe apparire assurda a chi abbia letto quanto dice Hjelmslev[16], le generiche asserzioni di Anderson[17] ed anche lo specialistico articolo del Robins. Ma prove concrete di fatto non ne sono state finora recate. Infatti il Robins (pp. 107–108) si limita a riportare il passo 2 del paragrafo 5, nel quale — come si è visto — si stabilisce solo un'analogia, non si dice esplicitamente che il dativo indica nozionalmente la permanenza in un luogo, e così via. Ne deriverebbe che quanto il Robins porta avanti è una costruzione arbitraria?

Ciò non è certo esatto, ma sarebbe stato opportuno analizzare il testo greco con maggiore attenzione, esattamente tra il primo ed il secondo brano citati nel paragrafo precedente. A p. 121,

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[15] In particolare: τὰ τοπικὰ τῶν ἐπιῤῥημάτων τρεῖς ἔχει διαστάσεις, τὴν ἐν τόπῳ, τὴν εἰς τόπον, τὴν ἐκ τόπου (GG. II 1 p. 201). Qualche cenno allo distinzione tra gli avverbi locali (con la successione: stato in luogo → moto verso luogo → moto da luogo) è già reperibile negli *Scholia* alla Τέχνη γραμματική di Dionisio Trace: rimandiamo in proposito all'*Index rerum et vocabulorum* di GG. I 3, s. v. τοπικὰ ἐπιῤῥήματα, p. 368, col. 1.

[16] L. Hjelmslev, La catégorie des cas, 1e partie, Acta Jutlandica (Aarhus, 1935) 11–12. Il testo dello studioso danese, difficilmente reperibile in Italia, è stato da noi consultato all'Istituto di Glottologia dell'Università di Bologna.

[17] In effetti il linguista inglese, pur vedendo in Massimo Planude un precursore, non gli ha mai dedicato uno studio specifico.

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31–32 si legge: ὧν [degli avverbi di luogo] τὸ μὲν ποῦ ἢ πόθε μόνην καὶ στάσιν ἐν τόπῳ δηλοῖ; a p. 122, 3–5: τὸ δὲ πῇ, ἢ πόσε καὶ πόθεν, καὶ ἄμφω κίνησιν σημαίνει· ἀλλὰ τὸ μέν, τὴν εἰς τόπον, τὸ δέ, τὴν ἐκ τόπου[18]. Sulla base di questo semplice chiarimento e del secondo passo planudeo (sempre del § 5), si può ricavare che i casi sarebbero relazionati funzionalmente a concetti locali nel seguente modo:

dativo ↔ ἐν τόπῳ ↔ STATO
accusativo ↔ εἰς τόπον ↕ VS.
genitivo ↔ ἐκ τόπου ↔ MOVIMENTO[19]

La stessa sequenza precedentemente esaminata degli avverbi interrogativi (sulla linea di Apollonio Discolo) implicitamente comporterebbe la scissione (sia pure funzionale) tra caso stativo e casi non-stativi[20]: tale successione è poi normalizzata *exabrupto* per identità con la sequenza casuale della declinazione.

Dall'analisi del paragrafo precedente e del presente si ricaverebbe dunque un atteggiamento ambivalente di Massimo Planude: da un lato egli resterebbe legato al suo modello più probabile[21] sul tema degli avverbi, dall'altro se ne allontanerebbe

[18] Per quanto abbiamo ricercato con cura tanto nella collezione dei Grammatici Latini quanto in quella dei Grammatici Graeci, non ci è stato possibile trovare dei precedenti nell'antichità classica. La formulazione del Planude dovrebbe quindi essere originale almeno per il problema specifico della serie casuale e dei suoi valori semantici spaziali; ciò ovviamente non varrà per l'analogia con la serie avverbiale locale interrogativa.

[19] Il tema della distinzione tra κίνησις e στάσις si trova anche nel Περὶ γραμματικῆς διάλογος, p. 27sgg. Bachmann, dove si sostiene che alcune preposizioni (παρά, περί) a seconda del verbo a cui si accompagnano possono indicare l'una o l'altra. L'analogia tra stattività/movimento casuale e stattività/movimento nei SP era già stata giustamente accennata dal Robins, art. cit. 108, il quale tuttavia non fa riferimenti né al Περὶ συντάξεως né tanto meno al *locus* qui menzionato.

[20] L'accomunare genitivo ed accusativo entro il nodo κίνησις è confortato implicitamente da un passo del Περὶ γραμματικῆς διάλογος, p. 29, dove sotto il verbo βαδίζειν si accomunano i due casi uniti a preposizioni: ἐκ τῆσδε τῆς πόλεως εἰς τήνδε βαδίζειν τὴν πόλιν.

[21] Cioè prevalentemente Prisciano e — tramite lui — ad Apollonio Discolo, ma indirettamente. Si ricordi poi che Prisciano insegnò grammatica nel VI secolo d. C. a Bisanzio.

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per brevi escursioni speculative (stativität vs. non stativität), le quali però vengono subito fatte rientrare nel *corpus* della presentazione descrittivistica.

7. Conclusione

Concludendo, si può asserire che il Περὶ συντάξεως di Massimo Planude presenta senza dubbio alcune suggestioni che non possono non richiamarci implicitamente talora ad alcuni criteri della linguistica attuale. Naturalmente si dovrà anche ammettere che esse risultano sporadiche, a mo' di illuminazioni momentanee, non inquadrate né inquadrabili a posteriori in un ragionamento meno banalmente e strettamente legato per un verso alla logica aristotelico-peripatetica per l'altro al piatto descrittivismo della grammatica antica[22].

Se quindi non ci pare onestamente avanzabile l'ipotesi di vedere nel filologo medioevale un eccezionale precursore al di sopra dei suoi tempi, ciò non significa che alcune sue osservazioni ed asserzioni non possano essere considerate probanti e degne di attenzioni rispetto agli sviluppi attuali della linguistica.

L'importante sarà di non voler ad ogni costo strumentalizzare il pensiero di Massimo Planude ai nostri contemporanei o viceversa, così da produrre o la svalutazione di questi ultimi (= il loro pensiero era già stato espresso) o un'eccessiva evidenziazione dei medesimi (= l'antico è arretrato)[23].

Nell'ambito della storia della linguistica ci pare più prudente e scientificamente più corretto restare ancorati al livello puramente espositivo (e commentativo), limitandosi a cogliere e a notare le identità e le divergenze tra i poli del parallelo tematico considerato diacronicamente.

Siamo in ogni caso d'accordo con il Robins sulla necessità di approfondire maggiormente lo studio dei testi dei grammatici bizantini — cominciando con l'allestire edizioni critiche adeguate

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[22] Il problema dell'*imitatio* di un modello condiziona fortemente il grammatico medioevale, il quale non osa allontanarsi troppo dal modello.

[23] Vedi R. Simone, art. cit. 140–145.

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e commenti opportuni al passo con le nostre conoscenze della linguistica —, senza però voler fare di essi dei genî *ante litteram* ad ogni costo[24-25].

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[24] Quando scrivevamo il presente contributo non avevamo potuto consultare per motivi contingenti (rilegatura del volume) il saggio assai prezioso di J. Pinborg, Classical Antiquity: Greece, in: T. A. Sebeok (ed.), Current Trends in Linguistics, Vol. 13 (= Historiography of Linguistics), The Hague-Paris 1975. Ivi a p. 87 (2.3.3: "The number of case") si legge: "according to Hjelmslev the localist interpretation of the cases given by Planudes is systematically the must satisfactory case-theory of Greek grammar. According to this, the dative is the unmarked case, whereas the accusative and the genitive are marked for approach and with drawal, respectively. This theory, however, is certainly older than Planudes. It goes back at least to Heliodoros (Grammatici Graeci I 3, 549, 23f. 8th century) and possibly goes as far back as Apollonios Dyskolos (cf. Grammatici Graeci II 3, 68). But there is no reason to connect it with the Stoics." E a p. 121 (3.8: "A note on Byzantine grammar"): "the theories of case, for which praised Byzantine grammarians, in the main have classical models". Relativamente al primo passo abbiamo da osservare il fatto che forse il Pinborg avrebbe potuto fornire un *addendum* sull'indicazione dell'articolo del Robins. Inoltre — ci sia consentita l'osservazione — in un lavoro altamente specialistico come quello inserito nei Current Trends non avrebbe guastato qualche ulteriore osservazione su M. Planude, senza fermarsi a Hjelmslev, che aveva tutto l'interesse a scoprire ipotesi metalinguistiche a sostegno della sua idea di teorizzazioni precorritrici già nell'antichità. Quanto al passo attribuito ad Eliodoro, si veda la nota 15.

[25] A complemento bibliografico di alcune note, procediamo ad alcuni addenda. Relativamente alla nota 6, ricordiamo che su Varrone è recentemente uscito il libro di D. J. Taylor, Declinatio. A study on the linguistic theory of Marcus Terentius Varro, Amsterdam 1975; quanto ad ulteriori possibili indagini su grammatici antichi e medioevali (Planude e no), ci permettiamo di rinviare ai nostri scritti seguenti: Alcune questioni sul vocativo e sulla ipotizzata teoria localistica di Massimo Planude, Paideia 1978, 23–33; A proposito della teoria localistica: un excursus storico, Vichiana 1978, 366–383; Sull'origine della teoria localistica di Massimo Planude, L'antiquité classique 1979, 82–97; Minima planudea, HL. 1980, fasc. 1–2; Sulla sequenza dei casi nella teorizzazione grammaticale greca, ΕΛΛΗΝΙΚΑ 1980; Ancora sulla teoria localistica, Vichiana 1979, fasc. 3; Due note

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di storia della linguistica antica ["κύβοι, γραφεῖον e πτῶσις"; "Cesare, l'ablativo e un abbaglio di Lersch"], MSS. 1980; Due precorrimenti sui casi: nota di storia della linguistica, Acta classica 1980; Prisciano e il sesto caso "greco", Eirene 1980; Aspetti antropologici e linguistici sull'origine del linguaggio in un grammatico bizantino del XIII–XIV secolo: Giovanni Glykys, Relazione presentata al XIV° Congresso Annuale della Società di linguistica italiana, Lecce 1980; A propos du septimus casus, Eos 1980, fasc. 1; Les cas et la linguistique ancienne: le septième, Actes de la Session de Linguistique et de Littérature d'Aussois, settembre 1979, Paris 1980 (Ecole Normale Supérieure); Alla riscoperta dei grammatici dimenticati: Massimo Planude, RSC. 1979, fasc. 2–3; A proposito della recente "Sammlung griechischer und lateinischer Grammatiker", ibid. 1979, fasc. 2–3. Più in generale, molti precorrimenti sui casi [segnacasi e funzioni, in alcuni linguisti italiani sconosciuti] sono reperibili in F. Murru e G. Pessolano Filos, Alla riscoperta della didattica del latino in Italia nel Settecento e nell'Ottocento, "Piccola Biblioteca n. 34" della "Nuova Rivista Pedagogica", Roma 1980.

In rapporto alla nota 13, ci sia consentito segnalare i nostri ulteriori studi: Tra didattica e linguistica contemporanee: studio di un modello semantico-generativo per l'insegnamento del latino, Studi italiani di linguistica teorica ed applicata 1977, 601–628; La "grammatica dei casi" e il sistema dei complementi dell'italiano, VR. 1977, 17–27; La "grammatica dei casi" e l'insegnamento del latino, in G. Proverbio (ed.), La sfida linguistica. Modelli grammaticali e lingue classiche, Torino 1979 (Rosenberg & Sellier), 281–309; La "grammatica dei casi" e il greco antico: prospettive applicative e glottodidattiche, Studii şi cercetări lingvistice 1979, 337–352; Entre philologie classique et linguistique théorique et appliquée: l'accusatif de qualification et la "grammaire des cas", Revue Roumaine de Linguistique 1979, 591–604 [marzo 1980].

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Morphosyntactic Motivation in Reconstructed Words: Latin *tranquillus*

There is a common feeling among linguists that there is much, perhaps too much, room for speculation in etymological studies. Only guesses can be made about how a given word or family of words has developed. This feeling is of course quite justified but it should be recognized that the problem of attaining historical realism is analogous to that of making a psychologically realistic description. Despite the ontological differences, there is a speculative element in both.

Attempts to render a grammatical description compatible with psychological or with historical reality are bound to be based on reasoned guesses. The speculative element can be reduced by working out heuristic principles that increase the rational element. In descriptions professing to aim at psychological realism various kinds of "external" or "substantive" evidence (cf. Campbell 1974; Skousen 1975; in general, Zwicky 1975: 154–5) constitute a set of such heuristic principles. In diachrony, historical realism consists in and hinges on those pieces of contextual information (serving as "external evidence") that can be gathered by means of a careful philological analysis (see esp. Maher 1977a: 64–70); but more grammatically oriented heuristic principles have been put forward as well (see Szemerényi 1977; cf. Anttila 1972: 331–2).

The present paper will be concerned with the important principle that words reconstructed must not contradict the known rules of derivational morphology (Szemerényi 1977: 297–306). More specifically, I shall show by means of a case study that conceptual-semantic match and absence of phonological difficulties are not enough for establishing an etymological relation between two or more words compared. The reconstructed protoword must also be morphosyntactically motivated. This is to say:

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Each morphemic constituent reconstructed to build up a morphologically complex protoword must contribute to the semantic raeding (the "protomeaning") of the said word. The synchronic counterpart of this principle is that a new word is created to meet some communicative need by making use of the semiotic possibilities of the linguistic signs available in the language system. In Maher's (1977a) terms, there is no tradition in a reconstructed word, only creation. A protoword, just as a newly coined word, is the beginning of a tradition.

The case to be presented in this paper involves the etymology of the Latin adjective *tranquillus* 'quiet, calm'. It will be made evident (§§ 1–4) that the principle of morphosyntactic motivation, while necessitating the rejection of the received etymon, leads to new discoveries concerning the Indo-European relations as well as possible Latin internal developments of the word in question. The concluding section (§ 5) is devoted to a discussion of the prehistory of the Latin preposition *trans*.

1. The implausibility of *quies*

The common semantic denominator of the uses of the adjective *tranquillus* is 'quietness', and the adjective *quietus* seems to be its synonym; e. g.,

potin ut animo sis {*tranquillo* / *quieto*}? (Plaut. Merc. 890) (Plaut. Most. 396)

'can't you be in a quiet mood?'.

Hence, there is nothing strange in the fact that *tranquillus* has usually been considered to derive from the same Indo-European source as *quies*, *-etis* 'state of rest' (LEW.[3] 700; Muller Jzn 493; Pok. 638; Vasmer II 420). *Quies* also serves as a convenient quick etymology of *tranquillus* in current dictionaries (e. g. Bianchi & Lelli 1974: 1062; Dvoreckij 1976: 1024). According to LEW., the basic form is **trans+quil+nos* 'ganz ruhig'. As far as phonology is concerned, instruments for turning this representation to *tranquillus* are provided by current handbooks (but on *trans+qu-*, see § 5), but this etymological solution breaks down on morphosyntactic difficulties. According to LEW., *trans+* in **trans+quil+nos* is used in the meaning of French

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très, but notice that nothing in the use of *tranquillus* in historical Latin suggests intensification. This is not contrary to expectation, because the intensifying expression type *très*+ADJ. is a French innovation (Thielmann 1887: 386; Falk 1940). Clearly, the proposed etymon **trans+quil+nos* 'throughout calm' fails to meet the morphosyntactic exigencies of reconstruction.

The customary etymology also fails to pay heed to selectional considerations. *Tranquillus* is primarily a meteorological adjective; i. e., it is "'tautologously" (in the sense of Maher 1976: 330) colligated with *mare* 'sea' (e. g. Plaut. Cist. 15; Poen. 507) and partakes in navigational metaphors (e.g. Plaut. Cas. 26; Poen. 355; Ter. Phorm. 689; Lucr. 5,11–12; Cic. Off. 1,24,83; etc.). Its colligation with *animus* (etc.) to denote a calm state of mind results from a metaphorical extension (cf. Plaut. Poen. 753 *tranquillitas* [viz. *animi*/M. N.] *euenit quasi naui in mari* 'there is calm now, just as for a ship at sea'). The association of *tranquillus* with *quies*, Goth. *hveila*, etc. (cf. Pokorny 638) rather implies the opposite development, but this is highly improbable in view of the fact that meteorological expressions easily proliferate metaphors of emotional state; e.g. Lat. *serenum* 'fair weather'; *serenus* 'clear, bright' (meteorol. > emotional); Finnish *sees* 'serenus' (meteorol. > emotional; cf. Hakulinen 1966); *tyven, tyyni* 'tranquillus' (meteorol. > emotional); Greek *γαλήνη* 'calm, stillness of the sea' and *εὐδία* 'fair weather; calm' metaphorically of 'serenity of mind'; Taillardat 1963: § 337).

2. Reconstructing the original meaning of *tranquillus* (and what can be gleaned from it)

2.1. Semantically, the etymology based on *quies* is a "lectio facilior" because it sticks to what is "folk-etymologically" most obvious. Sometimes the heuristic starting points provided by folk-etymological associations are valuable indeed (cf. Benveniste 1954 as well as Maher 1977a: 51–78 on *aqua* 'water'/*aquila* 'eagle'/*aquilus* 'dark'/*aquilo* 'northwind'; Deroy 1975 on *obliviscor* 'I forget'/*(ob)livius* 'obscure'/*lividus* 'livid, leaden-colored'; Oleson 1976 on *urina* 'urine'/*urinator* 'diver'; Nyman 1977 on *caput*, *-itis* 'head'/*capillus* 'hair of the head'; etc.), but

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they must be compatible with various kinds of external evidence provided by philological considerations. Despite its prima facie plausibility, the etymology **trans+quil+nos* must be jettisoned because it lacks situational motivation. And fortunately, it can be substituted for by another etymological tradition leading from Bréal (1883, 1884) through the various editions of DELL. (which copies from Bréal & Bally 1886: 401) to Borgeaud (1967, 1969).

It was pointed out by Bréal in an oral presentation (1883) that linguists are too prone to base their etymological solutions on usual word meanings without too much paying heed to such "exceptional" instances that might, as linguistic fossils, offer a glimpse at how a given word was used at an earlier state of the language. In other words, Bréal made a plea for he "palimpsest effect" of language.[1]

As a case in point, Bréal proposed that the original meaning of *tranquillus* was not 'calm', but rather 'transparent'. As a piece of evidence he (1884: 346) quoted a passage from Pliny's Naturalis Historia. For expository purposes, let us quote this passage in extenso:

> "Cinaediae inueniuntur in cerebro piscis eiusdem nominis candidae et oblongae euentuque mirae, si modo est fides praesagire eas habitum maris *nubilo colore aut tranquillitate*" (Plin. Nat. 37, 153).

According to Pliny, *cinaediae*, i. e. white oblong stones found in the brain of the fish named *cinaedus*, "foretell" conditions

[1] Palimpsest denotes a parchment or papyrus which previously carried another text. Often this removed text can be dimly seen between the lines of the text. In the same vein, "every language, as indeed all natural systems ... are palimpsests" in that they "embody 'overlaid functions'" (Maher 1977a: 33). These overlaid functions, i.e. "imprint[s] of different eras, different authors, different languages, dialects, styles, registers, of paradigmatic variants analogically extended to new ones" (Maher 1977b: 5), are comparable to the vestigial traces, in a palimpsest, of the previous text which is seen or can be made more or less visible by means of various techniques such as ultraviolet rays, etc. The veritable τέχνη γραμματική, or the art of writing grammars, consists in the ability to detect and assess the "palimpsest effects" of language.

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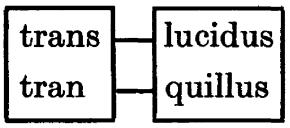
at sea *nubilo colore aut tranquillitate* ('according to whether they are dull in color or transparent'). In this context, *tranquillitas* can mean only 'transparency', and this Bréal takes to be the original meaning.

Bréal made an excellent guess. Firstly, it explains the prefixal *tran+* by specifying with a credible function. This satisfies the requirement of morphosyntactic motivation:



'transparent'
'transparent' > 'calm'

Secondly, the semantic shift 'transparent' > 'calm' is a natural result of a shift of focus from the transparency of sea to the calm of sea. The fact that waters are transparent when calm (cf. Cic. Tusc. 5,6,16 *maris tranquillitas intellegitur nullā ne minimā quidem aurā fluctus commovente*) provides the original context of situation which is, furthermore, indexically (metonymically) shown by the interpretation given to the behavior of the *cinaediae*: calm (in nature) = transparency (in stone). Cf. also Plin. Nat. 37, 66 *dos eorum* [i. e. *smaragdorum*] *est in colore . . . imitante tralucidum maris* 'the asset (of emeralds) consists in their color which reproduces the transparency of sea-water'. As a piece of evidence for the original meaning 'transparent' Borgeaud (1969: 146) cites Plaut. Most. 417 *ut liqueant omnia et tranquilla sint* 'so that everything will clear up and quiet down'.

2.2. Unfortunately, however, if we look up the relevant passage in any modern edition of Pliny's "Natural History", we do not find the expression *nubilo colore aut tranquillitate* in the current text. The modern editors (Jan & Mayhoff 1897; Eichholz 1962; Saint-Denis 1972) are unanimous in writing (*praesagire eas habitum maris*) *nubili uel tranquilli*. This seems to ruin Bréal's crucial argument, because as an adjective attribute of *mare*, *tranquillus* would of course have its usual meaning 'calm'.

The reading *nubilo colore aut tranquillitate* was the lectio vulgata until codex Bambergensis (10th c. A. D.) was detected by Ludwig von Jan in 1831. This manuscript is the oldest and most valuable source of the six last books (32–37) of Pliny's Naturalis Historia, and therefore its reading *nubili uel tranquilli* (which

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occurs nowhere else) has been accepted by most editors without further ado. It seems, however, that this deductive replacement of *nubilo colore aut tranquillitate* (other manuscripts, with slight variations) by *nubili uel tranquilli* is a mistake resulting from a servile adherence to the editorial rule that " il faut s'écarter le moins possible du *Bambergensis*, seul survivant de l'*ordo uetustior*" (Saint-Denis 1972: 21).

In the passage at issue, *nubilus* and *tranquillus* are most probably intended as terms of a polar oppositon. So, if *mare tranquillum* means 'a calm sea', the syntagm *mare nubilum* must mean 'a stormy sea'. But this is improbable. Clouds impair the lighting (e. g. at sea). Therefore it is natural that the co-presense of clouds and gloom in nature is diagrammatically reflected in language as the polysemy [1: 'cloudy' — 2: 'gloomy; dull']. If *mare nubilum* was acceptable at all (as far as I can tell, this Plinian passage would be the only attestation of this colligation), it must have referred to the (impaired) illumination at sea. Given this interpretation, *mare tranquillum* would then have denote to the sea in a bright weather. It is important to note, though, that while ABSENCE OF WIND (or, more generally, ABSENCE OF A DISTURBING ELEMENT) is an inherent, emic feature in the semantic structure of *tranquillus*[2], BRIGHTNESS OF WEATHER is only an occasional, etic feature in the "semantic reading" of this lexeme; i. e., it is logically possible that the sea is calm when the sky is overcast. I do not wish to deny the possibility of re-weighing an etic feature as emic on the basis of a concrete context of situation but in *tranquillus* the meaning 'clear, bright' is unattested. On the other hand, I find it more justified than not to maintain the polarity assumption.

To sum up, the only virtue of *nubili uel tranquilli* appears to consists in the fact that it is the reading of Bambergensis. But even this asset must be counterpoised by the fact that "B[ambergensis] . . . . in spite of its superiority is sometimes careless

[2] *Caelum serenum* denotes a bright sky but *caelum tranquillum* means 'a windless sky'; cf. Plin. Nat. 2, 192 *caeloque adeo tranquillo ut uolatus auinum non pendeant subtracto omni spiritu qui uehat.* Metaphorically the wind is *fluctus aeris* 'a wave of air' (Plin. Nat. 2, 114).

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and facile'' (Eichholz 1962: ix). It seems to me that *habitum maris nubili uel tranquilli* is an example to this effect. The colligation *maris nubili* is in any case unparalleleled, and the whole expression is somewhat enigmatic. Despite whether one has etymological preoccupations or not, the reading *nubilo colore aut tranquillitate* must be preferred because it makes more sense; and also in Plin. Nat. 9, 107 *nubilus* is colligated with *color. Tranquillitas*, which is attested in all other manuscripts, but B, can mean only the opposite pole, viz. 'transparency'.

2.3. The basic form **trans+liquillus* suggested by Bréal (1884: 346) is not implausible semantically (cf. *tra(ns)+lucidus*; both *+liqu+* and *+luc+* can be taken as parallels meaning 'to be clear'). However, the syllabic reduction **trans(li)quillus* is difficult (but cf. *sembellus* from *semi+libellus*). Bréal's suggestion is also morphologically improbable, for it is rather artificial to argue for a reconstructed diminutive of a reconstructed adjective **trans+liquus/+liquis*.

A more probable suggestion has been made by Borgeaud (1967) who assocites the element *-quillus* to **k^{w}(e)is+* (cf. OIr. *ad.cí* 'he sees' < **k^{w}is+et*; *cíall* 'sense, perception' < **k^{w}eis+lā*) or **k^{w}(e)it+* (cf. Skt. *citrá+* 'bright, excellent' < **k^{w}it+lo+*; *citti+* 'thinking, understanding'; etc.), i. e. *+s/t+* -extended variants of the Indo-European root **k^{w}ei+* 'worauf achten' (Pokorny 636–7). Thus, Borgeaud concludes that the historically underlying from of *tranquillus* is either **trans+quis+los* or **trans+quit+los*. Semantically this seems plausible enough, and I shall accept it without reservations until § 4, where a perhaps better alternative will be considered. But whichever alternative will be preferred, both potentially involve the same phonological problem which will be dealt with in the next section.

3. Phonological Derivation

3.1. As Borgeaud admits, his etymological solution involves problems of phonological order. In (1967), Borgeaud remains in principle undecided whether *tranquillus* derives from **trans+quis+los* or **trans+quit+los*, whereas in (1969), he definitively assumes **trans+queis+los* as the historically underlying form and concludes: "Il s'ensuit que *l* géminé, dans *tranquillus*, provient

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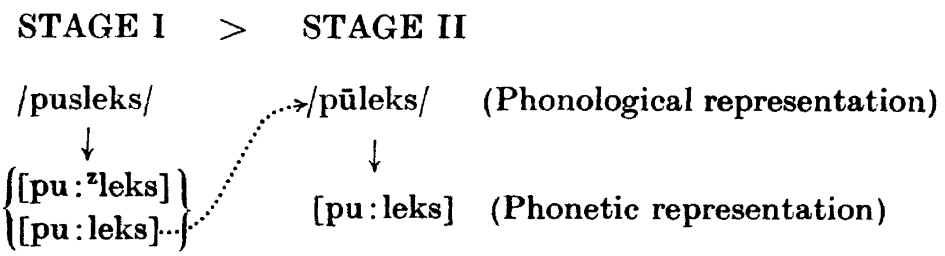
de la diphtongue *ei+sl*, qui donnait une syllabe surlongue + *l*, donc *īl+l*, parallèlement aux cas de **vei(c)s-la* > *vīlla* et **pau(c)s-lom* > *paullum* (cf. *pauxillum*). La règle de ces cas me paraît bien être: les diphtongues *au* et *ei* devant le groupe *sl* (> *zl*) dégagent, après assimilation de *z* à *l*, une syllabe surlongue finalement notée par *ll*. Il convient donc de prononcer: *tranquīllus*'' (1969: 145).

Borgeaud relates the sequence *ll* (< *sl*) to the preceding diphthongs *au* and *ei* but this formulation is flatly contradicted by *ancīle* 'shield (esp. as a symbol of the prosperity of Rome)' which must derive from **anceisle* (which is, in turn, what results from the application of Medial Vowel Reduction to **am(bi)+cai(d)+sle*; on the relation of *ancīle* to *caedo*, see Leumann 1977: 208). From a general point of view, I see no compelling reason to assume that the sequence *sl* was not treated like *sm* and *sn*[3] in Latin. So, *sl* did not develop to *ll*, rather *Vsl* was changed to $\overline{V}l$, and if *sl* was preceded by a long vowel or a diphthong, *s* (> *z*) disappeared tracelessly (cf. Szemerényi 1954: 35–40)[4], e. g. **puslex* > **puzlex* > *pūlex* 'flea'; or, with more linguistic sophistication:



It is here assumed, and made explicit, that **puzlex* and *pūlex* used to be phonostylistic variants, until the casual speech variant *pūlex* was lexicalized (cf. Nyman 1978a: 74–75).

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[3] On the relation of *pesna* (Festus 222, 25 Lindsay) to *penna* 'wing' (< **petna*) see Hamp (1973) which solves Leumann's (1977: §. 211c) aporia.

[4] However, I do not agree with all of his phonological-etymological derivations; e.g., *ūilis* 'cheap' scarcely derives from **wes+lis*; at least, it is scarcely parallelled by *fīlius*. And *suspīcio* 'suspicion' and *dēlīnio* 'to smooth down; to cajole' ought not to be cited as evidence for the sound law ē > ī / __Ci (see Nyman 1978b).

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Nowadays it is commonly held that *rāllum* 'scraper' derives from **rād+lom*, not from **rād+slom*; but *paullus* 'litte, small', *aulla* 'pot', and *vīlla* 'estate' are sometimes cited as evidence for *sl* becoming *ll* (or *ksl* > *ll*). However, I am inclined to side with Thurneysen's (1907) suggestion that *paullus* derives from **paur+los* (cf. *parvus* < **pauros* by metathesis; cf. Gk. παῦρος) and probably also *aulla* from **aul+lā* (cf. *alvus* 'belly, womb' from **aulos*; Gk. αὐλός 'tube'); cf. also Szemerényi 1954: 40; Leumann 1977: 208. *Villa* remains problematic, but it is definitely misleading to suggest that the phonological problem met with in the derivation of *vīlla* from the alleged **vi(c)slā* is paralleled by the case of *tranquillus*. The most natural parallel of *+qu(e)is+los* is of course *dis+l-* (e.lg. *dis+luo* > *dīluo*; **puslex* > *pūlex*, and other instances not involving a historically underlying *Csl*. Therefore, **trans+qu(e)is+los* can become only **tranquīlus*.

3.2. Borgeaud closed his paper (1967) with the following words: "Est-il interdit de poser, comme alternative, une forme **trans-quit-los*?". This question has a slightly iconoclastic tone because it is commonly held that the sequence *tl* can result only in *cl* (e. g. Leumann 1977: 153–4; Sommer & Pfister 1977: 173). Indeed, this change is well-attested, and this is presumably why it has not been (considered) feasible to apply the rule *-tl-* > *-ll-* though, in principle, this change would scarcely be less expectable, or phonetically less natural, than the well-attested parallel change *-dl-* > *-ll-*. In the connection of the etymology of *capillus* 'hair of the head' (Nyman 1977: 177–79), I took notice of the fact that all instances of *-ll-*, from *-dl-*, involved a morpheme boundary between *d* and *l* (e. g. **sed+lā* > *sella*), whereas cases of *-cl-* (< *-tl-*) consist of instances in which there is no morpheme boundary between *t* and *l* (e. g. **pō+tlom* > *pōclum* > *pōculum*). There is no doubt that grammatical boundaries may have an impact on syllabification and thereby on the phonetic realization (cf., e. g., Davidsen-Nielsen 1974; Nyman 1977: 179–186). On the other hand, it seems clear that complete articulatory assimilation can take place only across a syllable boundary. Accordingly, it was hypothetized that *-d+l-* was syllabified as *-d.l-*, and this phonotactically pro-

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blematic sequence was phonetically realized (first presumably in less careful styles of speech), and subsequently relexicalized, as *-ll-*. On the other hand, given no interfering factors, the natural syllabification of the sequence *muta cum liquida* (i. e. *-CR-*) is *-.CR-* (cf. Bernardi Perini 1974: 106-7). Therefore, *-tl-* (which had no intervening constituent boundary) was syllabified as *-.tl-*. This mode of syllabification of course blocked the realization of complete articulatory assimilation. Now consider, first as a theoretical possibility, what would have happened in the case of *-t.l-*. Most likely, *-t.l-* (just as *-d.l-*) would have been changed to *-l.l-*. The syllabification *-t.l-* could be conditioned only by an intervening grammatical boundary, and in Nyman (1977) I argue that in **capit+los* (< *capillus*) there was one. Now, **trans+queit+los* is very likely to be another case to this effect.

3.2.1. EXCURSUS: On other possible reflexes of *-t+l-* > *-ll-*.

In Nyman (1977: 186–7) I suggested that *pullus* 'young animal, esp. chicken' and *quallum*, a variant of *quālum* 'wicker basket', might involve other cases for he sequence *-t+l-* becoming *-ll-*. Neither word is entirely clear etymologically. In particular, the relation between *quālum* and the verb *quatio* continues to be a moot point, despite André's (1964: 70–71) shrewd plea for a return to this old etymology. And the relation between *quālum* and *quallum* (the latter is a hapax) involves another problem. If *quallum* is a real form, it could be described as resulting from an expressive gemination (*quālum* > *quallum*; cf. *bēlua* > *bellua*; *Iūpiter* > *Iuppiter*; Szemerényi 1954: 40).

As far as *pullus* is concerned, I am convinced, despite Szemerényi (1977: 317–8; and comm. by letter), that its customary equation to Skt. *putrá+* 'boy, son' (e. g. LEW. II[3] 386; DELL.[4] 544; Sommer & Pfister 1977: 187) is correct. But there are a few phonological and semantic issues that must be clarified.

While Skt. *putrá+* is mapped onto IE. **put+lo+*, Lat. *pullus* is usually derived by positing the weird base-word **put+slos*, clearly in order to block the un-Latin output **puclus*. But given the parallels **capit+los* > *capillus* (Nyman 1977) and **trans+k^{w}eit+los* > *tranquillus*, the base-word **put+slos* (which would result in **pūlus*) can be replaced by **put+los*. This brings out the equation *putrá* = *pullus* on the morphological level.

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There appears to be a semantic gap between **put+los* 'young boy' > Lat. *pullus* 'young animal; chicken', and probably therefore Szemerényi did not accept *pullus* as deriving from **put+los*. But this gap is scarcely too wide to patch up. It is quite feasible to think that YOUNG was an inherent, indeed salient, feature in the semantic representation of IE. **put+los*. Therefore, this proto-word might be glossed as "young+ling". Now, it is a well-known fact that lexicalization of semantic features takes place on the basis of those concrete contexts of situation in which words are used. For example, Gk. *νεοσσος* 'young bird, nestling' is a derivative of *νεος* 'young'. The specialized meaning of *νεοσσος* cannot be explained without the assumption that for some reason this word was originally used when talking about younglings of birds. In the same vein, IE. **putlos* may be compared to Gk. *νεος*, or Engl. *young(ling)*. In contrast with the other Indo-European languages which have reflexes of **putlos*, the meaning of this word was specialized in Latin to denote chickens. The reason for this is probably that **putlos* was used in contexts of divination to refer to sacred chickens which were fed and watched by a *pullarius* (cf. Cic. Div. 1,77; etc.). It is probable that this special meaning (Proto-Latin **putlos* 'chicken') provided the basis for further colligations of this word with other birds (see below) and then, with other animals, until *pullus* began to be used to refer to a special kind of young boy, too, the catmite.

The etymological relation between **putlos* and *pullus* can be proven by another consideration as well. For this purpose, let me first quote from Szemerényi (1977: 317–8): "The suffix *-illus* always indicates that the base-word has an *n* or *l* in the stem. In other words, *putillus* as a diminutive, demands a base word *put-no-* or *put-lo-*, not *puto-*. There can be no doubt that of the two alternatives only **putlo-* can be admitted. This means that *putillus* is an old diminutive of the inherited **putlo-*". Accordingly, Szemerényi relates the word *putillus* (usually but at least inexactly glossed as) 'young boy, Knäbchen' to **putlos*, and I agree with him in this. But if it can be made evident that *putillus* (< **putlo+los*) must be semantically related to *pullus*, the upshot will be that *pullus* indeed derives from **put+los*.

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In Plaut. Asin. 694 (*dic igitur me⟨d⟩ . . .*) *passerculum putillum* 'so, call me . . .) a young sparrowlet', *putillus* is an adjective attribute of *passerculus*, a caressing diminutive of *passer* 'sparrow' (cf. Asin. 666 *dic me igitur tuum passerculum*). It may be significant that *putillus* is colligated with a word denoting a bird. And I even venture the assumption that this colligation actualizes a semantic selectional restriction: *putillus* is inherently restricted to select only "bird-words" as its heads. This assumption is corroborated by Varro (Men. 568 B.) who uses this word to denote a young bird. These are the only occurrences of *putillus* in the literature. This is not much but suggestive enough of the following equation: *passer(culus) putillus* = *pullus passerinus* 'youngling of a sparrow, "sparrowling"'; cf. Hom. Il. 2,311 *στρουθοῖο νεοσσοί* 'the youngling of a sparrow'. The semantic resemblance between *pullus* 'young bird' and *putillus* 'young (/—BIRD)' suggests that these words are etymological cognates. And if Szemerényi's base-word **putlo+* is segmented as *put+lo+*, the result is *pullus* in Latin. The proto-word **putlo+los* need not have been specifically diminutive in origin. Diminutive is a subcase of metaphor. Therefore, **putlo+los* may be glossed as 'bearing a resemblance to *putlos*'.

In addition to *capillus* (< **capit+los*), *pullus* (< **put+los*), [*quallum* < **quat+lom*?], and *tranquillus* (< **tran(s)+quit+los*), the "sound law" *-t+l-* > *-ll-* is evidenced by the word *camillus* 'young boy, servant' as well, if we accept Flobert's (1976) contention that the source of this word is Etruscan *catmite* (= Gk. *Γανυμήδης*). Under this assumption *camillus* would derive from the proto-word **ca(t)mit+los* consisting of the Etruscan stem *ca(t)mit+* and of the Latin "pertinentive" suffix *+lo+*.

4. Proposal for an Alternative Etymon

On the analogy of Lat. *transLUCidus*, *perSPICuus*; Gk. *διαΦΑΝής*; Ger. *durchSICHTig*; Swed. *genomSKINlig*; Fin. *läpiNÄKYvä*; Hun. *átLATSZó*; etc., it is evident that the element *+quillus* in *tranquillus* must involve a verb of SHINING or SEEING. This requirement seems to be rather well borne out by Borgeaud's solution but there are a few problems that case some doubt on its feasibility. On the strength of § 3., the root

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underlying *+quillus* must be *t*-extended, i. e. **+kʷeit+los*. However, the words derived from **kʷeit+* seem not to warrant the conclusion that this root denoted a visual phenomenon. Rather, **kʷeit+* refers to sense-perception in general.; or example, Skt. *citrá+* (if it belongs here at all; cf. Mayrhofer I 387, 389) is '(clearly) visible' as well as '(clearly) audible'. So, if we adhere to the heuristic assumption that the root underlying the element *+quillus* denotes either shining or seeing, i. e. a visual phenomenon, Borgeaud's proposal is not entirely satisfactory.

The Latin labiovelar *-qu-* has two Indo-European sources, viz. the monosegmental *-kʷ-* and the bisegmental sequence *-ḱw-* (Skt. *aśva+*, Lat. *equus* < **eḱwos* 'horse'; etc.). Now, Indo-Iranian and Balto-Slavic allow the reconstruction of the *t*-extended root **ḱweit+* 'to shine; bright, white' (e.g. Skt. *śvetá+* 'bright, white'; Lith. *švieciu, šviesti* 'to shine'; cf. Pokorny 628–9)[5], and here I think is our missing etymon. The base-word **trans+ḱweit+los* is an exact morphosyntactic parallel of *tra(ns)+luc+idus* and the other instances brought forward in the begin-

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[5] It is a moot question whether it is feasible or not to reconstruct the *d*-extended variant **ḱweid+* on the basis of Germanic languages and Lith. *šviedrùs* 'shining, bright'; Skt. *śvindate* 'it is white'. But *šviedrùs* (as well as *šveidrùs* 'id.') is likely to be a contamination of *svidùs* 'shining' (cf. *svidėti* 'to shine') and *šveitrùs* 'id.' (cf. Fraenkel II 1053), and the late verb form *śvindate* may witness the Middle Indic development *-nt-* > *-nd-* (Tedesco 1948: 349).

The highly ambiguous occasional forms *šviedrùs* and *śvindate* scarcely allow the reconstruction of **ḱweid+* but Germanic reflexes are quite systematic. Goth. *hveits*, OIcel. *hvítr*, etc. (including Frisian, etc.; Holthausen 1955: 201–2) can be derived only from Proto-Gmc. **hvīta*, the most natural source of which is **ḱweid+*. Nevertheless, some weight ought to be given to the fact that the inherited reflexes of the proposed **ḱweid+* in the Gmc. languages consist only of adjectives meaning 'white'. As the element *+n+* is quite frequently employed in the formation of color adjectives (e.g. **bhrū+n+* > *brūn* 'brown'; Gk. φρύνη 'toad' [i.e. 'the brown one']; **ḱas+n+* > Lat. *cānus* 'grey'; OHG. *hasan* 'shining grey'; **pṛḱ+n+* > Gk. περκνός 'dusky, dark'; Skt. *pṛśni+* 'spotted'; etc.), the possibility cannot be ruled out that Proto-Gmc **hvīta* derives from an earlier **hvītta*, the source of which may be **ḱweit+nó+* (see e.g. Krahe & Meid 1969: 114). But admittedly this is a controversial issue.

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ning of this section. And as we have seen, *-t+l-* (i. e. *-t.l-*) is a quite conceivable source of Latin *-ll-*.

5. The problem of trans

In etymological solutions proposed for *tranquillus* the element *tran+* is derived from the underlying form *trans+* by the application of a rule (or "sound law") formalizable as follows:

(1) $s > \varnothing / N__qu$

(see Sommer & Pfister 1977: 190; LEW. I³ 702; etc.). This rule hinges only on two putative instances, viz. *inquit* 'he says, said' < **en+squet* and *tranquillus* < **trans+qu-*[6]. It is true that there are no instances of surface *-nsqu-*, nor are there examples showing that an underlying *-nsqu-* would have developed otherwise, but nonetheless it must be recognized that this "sound law" does not fit too well the overall pattern of Latin phonology (cf. Leumann 1977: 212).

5.1. The Indo-European root **sek^w+* 'to see; to say' (cf. Baldi 1974) has the reduced variant **sk^w+* (see Pokorny 847). Now, nothing seems simpler than relating the Latin verbs *inseque* 'speak!' and *inquit* to each other in terms of this variation:

Gk. *ἔννεπε* < IE. **en+sek^w+e* > Lat. *inseque* (> *insece*)
Gk. *ἐνισπ(εῖν)* < IE. **en+sk^w+et* > Lat. *inquit*.

Although this relation seems morphologically quite irreproachable, the phonological difficulty involved in the change *-nsk^w-* > *-nk^w-* is a real one: Phonetic as well as distributional considerations speak for the notion that in a cluster such as *-nsC-* (where C = any stop consonant) the nasal segment is most prone and most likely to disappear. Doubtlessly, the expected reflex of

[6] Szemerényi (1977: 308–9) implies the formulation $s > \emptyset / N__C$ in order to solve the phonological problem experienced in the derivation of *septentrio* from **septen+strio*, but as a matter of fact his etymology of *septentrio* involves no phonological problems. Given Szemerényi's etymology, *+strio* and *+trio* were synchronic variants at a given état de langue, and it was the variant *+trio* which superseded *+strio*, not by dint of a sound law, but rather in consequence of the fact that the variant *+trio* was folk-etymologically associated with the lexeme *trio* 'threshing-ox'.

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**ensk^{w}et* in Latin would have been ***ē$^{(n)}$squit*. This being the case, I consider it advisable to pay heed to Szemerényi's (1977) phonological principle: "If a given etymon, though fundamentally evident, involves phonological difficulties, the researcher should seek for a more "economic" solution" (294). Clearly, the putative **en+sk^{w}et* is a case in point, and because the phonological difficulty here consists in the non-deletability of the underlying *-s-* in the sequence *-nsC-*, it is unavoidable to assume that there is no underlying *-s-* in the word which historically underlies *inquit*[7].

I think it best to challenge the derivation of *inquit* from **ensk^{w}et*—at least by means of the phonologically conditioned sound law (1). Unfortunately, it seems to be next to impossible to find a credible etymon, but nevertheless there are some considerations that are likely to be of heuristic value in approaching this etymological problem

First, unlike Gk. *ἔννεπε* and *ἐνισπεῖν* which belong to the same stylistic level, viz. epic diction, Lat. *inseque* and *inquit* are stylistically and functionally quite disparate. While the first belongs to solemn epic contexts (e.g. Livius Andronicus translates the Homeric *ἔννεπε* by *insece*), *inquit* is at home in lively popular speech. In Greek the functional counterpart of the latter is *ἔφη* (Kieckers 1912: 145–75; 1920); both verbs are used, sometimes even pleonastically. as "insert" sentences in quotations of direct speech. The functional difference is apt to render the genetic relationship between *inseque/ἐνισπεῖν* and *inquit* less self-evident than we are used to believe.

Second, *inquit* is a conspicuously defective verb. Practically the only forms in use are the 1st sg. *inquam* and the 3rd sg. *inquit*. Other forms such as *inque* 'say!', *inquiunt* 'they say'; etc. are more or less artificial formations occasioned by deducing the paradigmatic mates of *inquit* by way of analogy. It seems that in these deductions *inquit* and *inquam* were not considered to belong to the same conjugational scheme: for example, the

[7] It has been proposed (Lewy 1907: 422; cf. Baldi 1974: 78 n. 3) that there was a "*s* mobile" in the root **sek^{w}+*. This is unlikely, however, though positing this for the reduced variant **(s)k^{w}+* would of course resolve the problem in a flash.

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utterance *aliquis inquiat* 'some one may say' in Rhet. Her. 4,3,5 prompted Priscian to say that *inquias* would presuppose **inquiam* (Gram. 2,495,20–Keil). The synchronic separation of *inquit* and *inquam* may indicate that these two word forms derive from different sources.

Third, *inquit* and *inquam* are functionally disparate. While the former always functions as a parenthetical sentence inserted usually after the first phonological word (cf. Janson 1976) of a direct quotation (Scherer 1975: 222–3), the latter only rarely serves this purpose. As a rule, *inquam* functions as a sentence particle corresponding to *indeed, certainly,* or the like; e.g., Plaut. Amph. 378 *Cuius es?:: Amphitruonis inquam Sosia* 'Whose are you?:: Why, I'm Amphitryon's Sosia'; Capt. 571 *Negas te Tyndarum esse?:: Nego inquam* 'Do you deny that you are Tyndarus?:: I certainly do'; Pers. 491 *Aio inquam, apud te est, inquam* 'Yes indeed, at your house, indeed'. It may be the case that the particle function of *inquam,* quite evident in Plautus and Terence, is original.

Fourth, unlike the other verbs of saying, *inquit* does not accept embedded sentential object complements. For instance, though *ait* and *inquit* pass for synonyms, it is not possible to make the a.c.i. construction *se esse Sosiam* dependent on *inquit*:

$\left\{\begin{matrix} ait \\ **inquit \end{matrix}\right\}$ *se esse Sosiam* 'he says that he is Sosia'.

Nor does *inquit* accept nouns, even pronouns (except in late Latin), as direct object complements. In this respect *inquit* clearly differs from *inseque*: while *uirum mihi insece* 'ἄνδρα μοι ἔννεπε' is correct (Liv. Andr. 1), ***uirum mihi inque* would most probably have been judged as impossible by native speakers.

To sum up: The above considerations suggest that *inquam* and *inquit* are not cognates of *inseque,* and therefore the base-words **en+skʷet* and **en+skʷām* must be jettisoned. It is even likely, as envisioned by Brugmann (1910: 277), that *inquam* and *inquit* derive from different sources. There is some justification in the suggestion that *inquam* is an adverbial construction in origin, perhaps consisting of *in* and *quam.* The category

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change *inquam* (adverb) > *inquam* (verb) would have resulted from an abductive reinterpretation similar to *nequam* (adverb) > *nequam* (adjective) 'worthless' (cf. LEW. II[3] 163). It would be formally possible to relate *inquam* to Pindar's enigmatic *ἔμπαν*, as suggested by Brugmann (1910: 277), but the semantic problem must be left open in the present context. As far as *inquit* is concerned, its syntactic bevavior indicates that it is not a verb of saying in origin. Unlike the verbs of saying which involve three-place predicates, viz. *x* SAY *y* to *z*, *inquit* opens only one "slot", viz. that of grammatical subject (*inquit ille*). Therefore, the etymon is likely to be or consist of an intransitive verb. Two possibilities come into mind. According to the first one, the meaning function and syntactic behavior of *inquit* are roughly comparable to those of the Finnish verb *tokaista* which is in fact a derivative of the verb *tokata* 'to thrust'. So, while *tokaisee* can be glossed as 'inquit', the Greek counterpart of *tokata* is *ἐμπαίειν*. Under the assumption (not made before) that the initial labial stop in *παίειν* reflects IE *k*^{w}-, *inquit* and *ἐμπαίειν* could be related in formal terms (i. e. **en*+*k*^{w}-). Another possibility: *Inquit* may be a reflex of the pre-compound stage of the verb *init* (i. e. /in+it/) 'he enters; he starts'. More precisely, it is conceivable that the proto-phrase **enqu*(*e*) *eit* (> *inquit*) '(and) he enters' served much the same text-linguistic function as *ol* 'inquit' (related to Lat. *ul*+*trā*, etc.) in Old Irish (Havers 1911), viz. the deictic function of indicating speaker change. That **enqu*(*e*) *eit* is likely to have occurred in prehistorical Latin is evident on the basis of expressions such as Lucr. 3,484 *conturbari inque pediri* 'to be confused and impeded'; etc. (see Bernard 1960: 56–70) which, true, are artistically extended reflexes of this ancient état de langue; cf. also *endoque plorato* 'implorato-que' and *transque dato* 'traditoque' in Fest. 402,33–34 L. (on *endo* see Leumann 1977: 561–2).

5.2. It has been customary to justify the loss of *s* (Rule (1)) by *inquit*, when dealing with *tranquillus*; and by *tranquillus*, when dealing with *inquit*. The discussion devoted to *inquit* in § 5.1 involves an attempt to break this vicious circle. Now, the lack of *s* in *tran*+*quillus* suggests that there was no *s* in the historically underlying form of this word.

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The prehistory of the preposition/prefix *trans* is largely unknown (some discussion in Anttila 1969: 155). It is generally held that *trans* is originally the present participle of a hypothetical verb **trāre*, allegedly reflected in the verb *intrāre* 'to enter' (LEW. II[3] 700; Leumann 1977: 549, cf. 145)[8]. However, I agree with Anttila that it is more reasonable to regard *intrāre* as deriving from the adverb *intrā*. Nor is there reason to ascribe the preservation of the "word-final" *-ns* (instead of *-s* with compensatory lengthening of the preceding vowel) to the underlying sequence *-nts* (Leumann 1977: 145), because as a matter of fact *trans+* is not an entirely "free" morpheme. It is always colligated with verbs or nouns. In short, I do not believe that *trans* derives from the present participle **trān(t)s*.

I suggest the following solution to the problem of *trans*. Some Latin prepositions as well as prefixes show clear traces of the variation PREP. ~ PREP.+*s* (in which PREP. stands for "preposition" and "prefix"), viz.

ab	~	*ab+s*
ci(+tra)	~	*ci+s*
ec(+f-)[9]	~	*ec+s* (= *ex*)
ob	~	*ob+s* (> *os+*)
po+[10]	~	*po+s*[10]
prī+[11]	~	*prī+s*[11]

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[8] Leumann is inconsistent here. On the one hand, he states (1977: 540): that *trāns* is the participle of (*in+*)*trāre*; on the other hand, he maintains this: "*trans-d-* > *trązd-* > *trād-*, die Länge *ā* stammt erst aus der Ersatzdehnung . . ." (204).

[9] Plaut. Aul. 665 *ecfert*; Mil. 1153 *ecferri*; etc.; cf. Leumann 1977: 210; but cf. also Gk. ἐκ ~ ἐξ; and Osc. *ehtrad* < **ek+trad*.

[10] Cf. *po+situs* (*pōno* < *po+s(i)no*; Leumann 1977: 561); *po+lubrum* < **po+low(e)+brom* (LEW. I[3] 774); etc.—The variant *po+s* is reflected in *pos+teri* 'coming generations'; etc.

[11] The element *prī+* (< **pre+i*) is reflected in *prīuus* < **prei+wos* "celui qui est en avant" > "celui qui est isolé des autres" (DELL.[4] 536); cf. Festus 252,25 L: "*Pri* enim antiqui pro *prae* dixerunt".—The variant *prī+s* might be reconvered in *pris+cus* 'ancient'; *prīmus* (< **prīs+mos*); etc.; it is true that *pris+* is customarily considered as deriving from **pri+is* (i.e., zero-grade of the comparative suffix *+yos*; cf. LEW. II[3] 286; Leumann 1977: 317).

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sub ~ *sub+s* (> *sus+*)
ul(+tra) ~ *ul+s*[12]
ut ~ *ut+s* (> *us+*)[13]

The provenance of *-s* is a disputed matter. There has also been controversy about the relation of Gk. *ἄψ* 'away, back' to *ἀπό* 'ab', on the one hand, and to Lat. *abs*, on the other. But these issues are irrelevant to the present discussion.

Now consider *ab* ~ *abs*. We may assume, at least for the sake of argument, that this variation was handed down from IE. to Latin as **ap* ~ **aps*. In prehistorical Latin (terminus ante quem 600–500 B. C.) final stops tended to be realized as voiced by a kind of neutralization (Szemerényi 1973: 55–59). The result of this change is orthographically represented as *ab* ~ *abs*. Here orthography reflects morphophonology and indicates that *ab* was interpreted by native speakers as the (synchronically, "psychologically") basic variant[14]; whereas *abs* resulted from the application of a morpholexical rule, formalizable as

(2) $\varnothing \rightarrow$ s/PREP.+__

These considerations hold for *ob* ~ *obs* and *sub* ~ *subs* as well.[15]

That the morpholexical rule (2) was to some extent productive is evidenced by

(*ci+* →) *ci+s*
(*ul+* →) *ul+s*

which are likely to be innovations brought about in accordance with this rule (i. e., on the analogy of *ab* ~ *abs*, etc.). Despite

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[12] Cf. Thielmann (1887).

[13] Cf. *us+quam* 'anywhere' (LEW. II[3] 344).

[14] Cf. Quint. Inst. 1,5,69: "Frequenter autem praepositiones quoque copulatio ista corrumpit; inde *abstulit, aufugit, amisit,* cum praepositio sit *ab* sola."

[15] A similar tendency for the prefix *de+* to become *des+* in Spanish has been unravelled by Malkiel (1976: 293–5): ". . . the majority of the traditional Hispano-Romance prefixes . . . end in *-s*, witness *es-* . . .; *pos-* . . .; *res-* . . .; *sos-* . . .; and *tras-* . . .; so that *des-* fitted a ready-made mould far more smoothly than would have done its potential rivals *de-* and *di-*" (293).

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the fact that the bases *ci+* and *ul+* were not used as such, they must nonetheless be credited with psychological reality. Note that the longer variant is *ci+tra*, although **cis+tra* would have been phonotactically equally feasible (cf. *ex+tra*). To the group of analogical innovations we must add

(*tran+* →) *tran+s*.

Accordingly, I suggest that the PREP. *trans* developed from an earlier *tran+* by the analogical application of rule (2). (A suggestion to the same effect has been made already by Corssen 1854: 292.) The development of *trans* bears some resemblance to that of *po+* → *po+s*, in which the newer variant *pos+* became psychologically central, as is evidenced by the fact that it was used as the base for further morphological derivations (e. g. **pos+ti* > *post*). Also *trans+* became the lexicalized variant (comparable to *ex*), and the old *tran+* survived only as an opaque "palimpsest effect" in *tranquillus*.

Cognate synonyms of *trans* in IE. languages can be derived from **tr̥H+* by means of various root extensions. Indo-Iranian witnesses **tr̥H+é/ós* (Skt. *tiras*; Avest. *tarō*); O. Irish *tar* can be derived from **tr̥H+es*; and the underlying form for Germanic may be reconstructed as **tr̥H+k^{w}e*. In the above forms I follow Anttila (1969: 155), but Lat. *trans* must now be interpreted as not resulting from a secondary nasalization, but rather from a secondary assibilation; i. e. from the application of the morphological rule (2) to the lexicalized base *tran* (on the model of *ab* ~ *abs*, etc.).

Formally, *trān* (note the long vowel!) can be derived from **tr̥Hn*, the *n*-extension of which might be interpreted as a contextually determined variant of *m*. On this assumption it is possible to relate (**tr̥Hm* >) *trām* (> *tran* /__*s*; *traŋ* /__*qu*) to *trāmes*, *-itis* 'cross-way' and possibly also to *trāma* 'warp in a web'.

Now there are at least two possibilities to account for the *m*-extension in **tr̥Hm*. A painstaking effort was made by Flensburg (55–71) to establish that *trām* results from a superlative formation **trā+mo+* (< **tr̥H+mo+* in our terms; cf. **sup+mo+*

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> *summus*). As evidence for the nominal origin of *trām* he cited, in addition to *trāmes* and *trāma*, a wealth of alleged *m*-extended formations of the root **ter+* which would require semantic as well as morphological screening. One drawback in Flensburg's analysis is, in my opinion, that it cannot account for Gk. *τρᾱνως* 'clearly', in fact 'thoroughly', *τρᾱνής* 'clear', and (Hom.) *τέτρηνα* 'I pierced (through)' which, according to the analysis presented in Nyman (1979), all contain *τρᾱν* (< Jon. *τρην*), the Greek counterpart of *trām*. Now *trāmes, -itis* can be interpreted as a nominalization of *trām* on the model provided by the synonymous noun *līmes, -itis* (cf. Regula 1957: 190), as *τρᾱν+ῶς*, *τραν+ής* and *τέ+τρην+α* (from which the present form *τετραίνω* was later abduced) have been created on the basic form *τρᾱν*.

Given the above analysis, *trām* could be interpreted as an adverbial accusative (cf. Corssen 1854: 293); i. e. **tr̥H+m*. Or we might take account of Hamp's (1977: 148) reconstruction of the suffix *+H_2m*, "an old element of manner or degree", and derive *trām* from **tr̥(H)+H_2m*.

6. Summary

The relatively long and winding road that we have followed leads to the conclusion that *tranquillus* may very well be derived from **trām+ḱweit+los* 'transparent'. This semantic reading which was first proposed by Bréal and resuscitated by Borgeaud is superior to the current etymological gloss 'very quiet' in that it is morphosyntactically motivated, especially if the underlying root is identified as **ḱweit+* 'to shine'. As far as the phonological aspect is concerned, it is entirely feasible and non-contradictory to derive Lat. *-ll-* from *-tl-*, provided that this sequence was separated by a syllable boundary reflecting an underlying morphological constituent boundary. The rule *-nsqu-* > *-nqu-* commonly resorted to in Latin historical grammars to account for ***tran(s)quillus* and ***in(s)quit* was shown to be unfounded. There are independent reasons for considering *trāns* to be an *s*-extended variant of *trām*. So, there was probably no underlying *s* in *tranquillus* at all. The same is likely to hold for *inquam, inquit* as well. It is true that the cognation with *inseque* looks

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self-evident at first sight but there is more than one reason making this relation suspect[16].

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Sihler, Andrew L., *The Latin 'tool' Suffixes and the Formation of "regula, tegula", and "tragula"* , Indogermanische Forschungen, 84 (1979) p.157

The Latin 'tool' Suffixes and the Formation of *rēgula, tēgula,* and *trāgula*

In a recent article (Sihler 1977) I argue that the conventional derivation of PIE. **rēĝ-* 'king' from **rĕĝ-* 'send/go/arrange in a straight line' is invalidated by both formal and semantic objections. The nub of the formal difficulty is the long vowel: there is no justification for a lengthened-grade vowel in all attestations of such a root noun, still less in its ancient derivatives like **rēĝ-niH*$_2$- 'queen'. A similar question arises in connection with three words attested in Latin: *rēgula* 'rule' (in all the English senses, that is 'measuring rod' hence 'principle, precept'), *tēgulum* 'roof', and *tēgula* 'roofing tile' (hence *tēgulae* also 'roof'). These are universally and without discussion agreed to be derived from the roots **reĝ-* as above and **teg-* 'cover' (Pokorny 1959: 854–7 and 1013–4). That both of these roots are Indo-European is clear; that they are well-attested in Latin with appropriate meanings is also certain. But I have not discovered any demurrer, in standard handbooks or anywhere else, from the conventional description of *rēg-* and *tēg-*, in these words, as lengthened grades of **rĕĝ-* and **tĕg-*. In view of the absolute uniformity of opinion, a detailed review of the Fachliteratur would be supererogatory; a representative sample, in order of publication, would be Meillet & Vendryes 1960 § 572 (unchanged from earlier editions), Ernout & Meillet 1959 s. vv. *regō*, *tegō*, and *trahō* (unchanged from the first edition), Leumann 1961 § 170 I B, Zucchelli 1970: 173 s. v. *rēgula*, described as from the root of *regō* 'a grado allungato come *tēgula*', and, most recently, Leumann 1977 § 263 B 1. Nothing, however, in the morphology of PIE. would lead us to expect a lengthened grade in a root with an *l*-suffix. Moreover, the remarkable similarity of the two forms in question is a mere coincidence, presumably, in terms of the usual interpretation of their formation. There is,

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however, an analysis of *rēgula*, *tēgula*, etc., that accounts for the long vowels in a plausible way, and likewise the similarity of the forms, by relating both to a specific element in the formations of the two words: namely, the PIE. 'tool' suffix in **-dhlo-*.

Genuine lengthened grade in PIE. is limited to so few morphological categories that we must start with the assumption that normal grade light base roots **reĝ-* and **teg-* underlie the formations in question. That is, the long vowels must be a Latin development; and the usual means of getting long vowels from short ones in Latin is by compensatory lengthening. (Analogy, the other principal source, can be dismissed out of hand as a possibility in the present instance.) What is needed, then, in the etyma of *rēgula* etc. is a lost consonant or consonants.

The formant **-sleH$_2$-*, which accounts for so many long vowels in Latin nouns, will not produce the desired outcome here. **Reĝ-sleH$_2$-* and **teg-sleH$_2$-* would yield, first, **rekslā- *tekslā-*, and the development of **-ksl-* is clearly seen in e. g. *āla* 'armpit' < **akslā-* (cf. the diminutive *axilla*, Buck 1948 § 207, Leumann 1977 §§ 124, 208). Accordingly, **reĝ-sleH$_2$- *teg-sleH$_2$-* would certainly have given §*rēla* §*tēla* rather than attested *rēgula tēgula*; indeed, there is a *tēla* 'web' which surely reflects this very consonant cluster though differently segmented: **teks-leH$_2$-* (*texō*).[1]

The only remaining *l*-derivatives with preceding consonant are **-tlo-* and **-dhlo-*, which together with **-tro-* and **-dhro-* make up the set of functionally equivalent 'tool' suffixes, typically neuter but not rarely feminine. From the point of view of meaning, such an affix would fit perfectly here, since a *rēgula* is a 'thing you use to put (or make) something straight' and a *tēgula* (no less than a *tēgulum*) is a 'thing you cover with'. Formally, however, we have some puzzles.

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[1] The symbol * denotes, as customary, an unattested form thought (by me or someone quoted) to have a reasonable possibility of having existed somewhere in the history of the language; the symbol § before a construct denotes an unattested form thought never to have existed, or one unlikely to have, for whatever reason. Forms whose status is evenly balanced between these two categories are generally marked with a star.

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The original distribution of the four affixes may be assumed without discussion to have been phonologically determined. An archaic-looking system is preserved in Sanskrit, where *-tra-* is the 'elsewhere' alternant, and *-dhra-* occurs only when the affix is contiguous to a root-final voiced aspirate—i. e. in accord with Bartholomae's Law. (Sanskrit gives no information on the question of **l* vs. **r*, which is not at issue here anyhow.) The primacy of the forms in **-t-* is suggested by their occurrence in Indo-Iranian in neutral environments, and is alluded to in the universal—if usually rather offhand—comparison of the 'tool' suffix **-tro-* etc. with the agent suffix **-ter- *-tel-* etc. (e. g. Leumann 1977 § 285). Certain it is that all primary suffixes beginning with a voiceless stop can be presumed to have had phonotactically determined variants in voiced aspirate stops, as perhaps most clearly seen in Sanskrit: *sameddhár-* 'kindler of the sacred fire' (Rigvedic, from *idh* 'kindle'); *ŕddhi-* 'increase, prosperity' (Atharvavedic, from *ŗdh* 'thrive' plus nomen actionis *-ti-*); *iddhá-* 'kindled' (Rigvedic, from *idh* again plus participial *-tá-*); *inddhé* 'is kindled' (*idh* again; the Rigvedic occurrence may actually be *t*-less, i. e. *indh-é* like *duh-é* 'gives milk', rather than *indh-té*); and *dogdhra-* 'milk pail' (from *duh* 'milk' plus the nomen instrumenti suffix *-tra-*). — A minor puzzle here is that Greek and Latin have turned **-dhlo-* and **-dhro-* into allomorphs independent of the original conditioning environment, whereas analogous **-dho-*, **-dhi-*, **-dhu-*, **-dher-*, for some reason show no such tendency. (Of course, some authorities doubt that Bartholomae's Law was pan-Indo-European; see footnote 2, below.)

In Latin, significantly, the full array of four tool suffixes occurs only following a vowel; following consonants, only **-tro-* and **-tlo-* have been recognized so far, and there are not many unequivocal examples of them, at that. ('Vowel' and 'consonant' here refer to Latin words; thus PIE. laryngeals, and **y* and **w*, count as 'vowels'.) After vowels: *rutrum* 'spade' < **ru-tro-* (*ruō*); *pōc(u)lum* 'drinking cup' < **peH₃(y)-tlo-* (*bibō*); *stābulum* 'abode, stead' < **steH₂-dhlo-* (*stō*); and *crībrum* 'sieve' < **k̂rey-dhro-* (*cernō*). There are fairly many examples of each. But after consonants there are found only **-tro-*, e. g.

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mulctrum 'milk pail' < **melĝ-* or **molĝ-* or **mlĝ-*; and **-tlo-*, e. g. *operculum* 'lid' < **op-wer-tlo-* (*operiō*). Such forms are not in any case very numerous; but the total absence of reflexes of **-dhlo-* and **-dhro-* after stops and the like is remarkable: after all, it is generally supposed that the types in **-dh-* specifically arose from the original **-t-* in consonant clusters (via Bartholomae's Law). This makes the total avoidance of position following consonants by the actually attested suffixes in **-dh-* strange indeed.

I suggest that occurrences of these suffixes after consonants do indeed occur in Latin, if rather rarely; they have been overlooked hitherto. As the most probable examples of such a formation, I would like to derive *rēgula*, *tēgula*, and *tēgulum* from **reĝ-dhleH$_2$-*, **teg-dhleH$_2$-*, and **teg-dhlo-*. (As hedged below, it is not necessary or desirable to think of these formations as being hugely ancient; the very occurrence of **-dh-* after a plain stop suggests a rather late date.) Having put the arguments forth for these, I will briefly take up the matter of stopless nomina instrumenti in *-l-*, and finally discuss some rather less straightforward questions of nomina instrumenti in **-dhlo-*.

The clinching argument in favor of the derivation of *rēgula* etc. proposed here—one might go further and say that it is the sine qua non of such a suggestion—would be the proof that **-ĕgdhl-* would necessarily give Lat. *-ēgul-* by the operation of demonstrable sound laws. I think it can be shown that there are good reasons for thinking that this is indeed the development, but the general lack of clear evidence about the fate of such clusters dilutes the rigor of the proof. This is regrettable, but far from unique; the complexities of the phonology of consonant clusters in Latin necessitate rather more sound laws of an ad-hoc sort than is desirable, or even tolerable, under more favorable circumstances. And even under the best conditions, the technique for dealing with such matters is a little loose: there is little to do but surmise (by whatever means) what the original structures were, take note of the attested forms, and decide which of the various outcomes is original and which have been distorted by such factors as analogy. As a general principle, the most shortened or otherwise transfigured outcome is taken as original, pro-

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bably correctly, and excuses are sought for the remainder. One example will suffice. According to Leumann 1977 § 203 b β, *lustrō* 'make bright' is certainly from **lukstrō*, and *Sĕstius* (gens name) is the same as *Sextius*. In addition to the doublet *Sextius*, however, there are forms such as *sextus* 'sixth', *textus* 'woven' (and many words of the same family), altogether many more forms than those showing 'regular' *-st-* < **-kst-*, but these are convincingly explained as showing the restorative effects of analogy; likewise complexes like *extenuō* 'reduce' are plainly the result of leveling vis-à-vis the preverb *ex-*. In addition *dexter* 'right', *iuxtā* 'next to' and some others—also outnumbering the 'regular outcome'—owe the survival of their clusters to relative chronology, since the syncope of an earlier **-ksit-* created their clusters after the period of reduction of original **-kst-*. On the other hand, the most certain instance of **-kst-* > *-st-* is actually from **-kstr-* (*lustrō* etc.). A different idea quoted by Leumann 1961 § 146 c suggests that the **k* was lost after long vowels, since *illustris* probably, and *lustrō* possibly, have *-ū-* < **-ow-*. The parallel clusters **-ksk-* and **-ksp-* exhibit unhelpfully various reflexes.

For the problem at hand, the difficulties come from two sources: primary suffixes beginning with voiced aspirates are decidedly rare, and, what few putative sequences of the type in question there are, they are typically suspected of having had their phonological development disturbed by the forces of analogy. As for the affixes themselves, apart from the 'tool' suffixes under discussion there are only three or four. The athematic imperative in **-dhi* (Skt. *śrudhi* 'hear!' etc.) is quite lost in Latin. The 2nd pl. mediopassive **-dhwe/*-dhwom* (e. g. Skt. *sácadhve* 'you follow', *ásacadhvam* 'you followed') has been ingeniously discovered in Lat. *-minī* (Szemerényi 1970 § 2, 2, 1 ad fin.); but even if that is the correct explanation of the form, it is never directly joined to a root in Latin, and in any case its startling transmogrification keeps it from bearing on the question at hand. The conflation of consonant-stem and *i*-stem nouns in Latin entailed the complete elimination of original **-bhos* etc. in favor of reflexes of **-ibhos* etc., so that the inflection of nouns shows no forms comparable to e. g. Skt. *vāgbhyás* 'to/from voices'.

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The more or less adverbial **-bhi*, as in Gk. *naûphi* 'aboard ship', seems not to occur in Latin at all.

Considering that **-dhlo- *-dhro-* are presumed to have arisen from the phonotactic combination of **-tlo- *-tro-* with a preceding voiced aspirate, other suffixes with basic voiceless stops might provide evidence for the fate of clusters like **-gdh-* in Latin, since e. g. past participles in **-to-* and verbal nouns in **-ti-* and **-tu-* would of course have occurred with roots ending in voiced aspirates, as in the Sanskrit examples cited above. The outcomes of such sequences in Latin cannot be counted upon, however, to be true inheritances uncontamined by analogizing across morpheme boundaries (Leumann 1977 § 197e Zusatz).[2]

Similarly, preverbs ending in stops in Latin simply assimilate to the articulation of the word-initial consonant of the simplex verb. Hence the *-ff-* of *afferō* 'bring' cannot judiciously be traced via sound law to **-dbh-*; likewise *affingō* 'form, invent' does not really reflect **-ddh-*. Some confidence is possible in rejecting these as normal developments because of compounds like *referō* 'return' and *interficiō* 'kill'; these incorporate sequences whose normal, i. e. undisturbed, reflexes in medial position are well-

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[2] Szemerényi 1970 § 6.2 states flatly that outside of Indo-Iranian, voiced aspirates become plain voiceless stops before plain voiceless stops (the latter is practically always **t*, of course); hence Gk. *léktron* 'Lager, Bett' < **legh-tro-* (Pokorny 1959: 658–9). This refreshing suggestion puts **t* in the same class with **s* phonotactically, which is reasonable, and cuts the Gordian Knot of the otherwise vexed question of the phonological history of Latin participles like *scrīptus*, &c. (cf. Leumann 1961 § 142a). But it turns the development **-dh-t-* > (**-t-t-* >) *-st-*, as seen in *aestus* &c. (and favored by Szemerényi himself; to be discussed shortly) into a great puzzle, since **-tt-* should have just one outcome in Latin whatever its ultimate history. Perhaps worse, Szemerényi's suggestion cuts the ground from under the only hypothesis available to account for the origin of **-dhro- *-dhlo-* in the first place.

I do not know what the explanation of Gk. *léktron* &c. might be. It appears that the alternations in this root became confused very early, in Greek, such that *-x-* (which I assume, contra Szemerényi, originally alternated only with *-kh-*) is seen alternating now with *-k-*, now with *-kh-*, and even with *-g-*.

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understood (in contrast to the considerable uncertainty about sequences like **-dbh-* or **-ddh-*). Sound laws would yield §*reberō* and §*interdiciō* (< **ent(e)ro-dhak-*) or §*interbiciō* (< **enter-dhak-*), so that *referō* and *interficiō* must be the outcome of some sort of reanalysis (perhaps periodically reapplied, whenever sound changes disturbed the identity of the morphological partials). Only forms at some distance from the source paradigm escaped such leveling, e. g. *sacerdōs* 'priest' < **sakro-dhoH$_1$-;* a few forms like *condere* 'establish' and *abdere* 'put away, withdraw' are exceptions, more or less, though *facere* is far enough removed in both form and function from these presumed cognates that it is not really surprising that it failed to serve as the model for analogical reconstitution like that of *reficiō*. Of course, since the Proto-Italic reflexes of voiced aspirates are generally held to have been voiceless, there is at least a possibility that *-ff-* is directly from **-dbh-* (via **-tf-*?), Leumann 1977 § 196, 3a. There is nothing implausible about the phonetics, but as will be shortly seen, the 'regular' outcome of e. g. **ad-dhiŋĝh-* (or rather **adzdhiŋĝh-*) would necessarily have been §*astingō*, §*assingō*, or §*ādingō* rather than *affingō*. It seems wisest to regard *affero* and *affingō* as the results of parallel processes, which is to say that the sound laws have been so battered by analogical leveling that such forms provide no evidence at all for the normal treatment of sequences like the **-gdh-* that we are concerned with.

Only a few sequences that are not obviously tainted by leveling can be posited, and those few all entail **-ddh-* < **-dh-t-* (Leumann 1977 § 167 Zusatz, § 171c); this means that the interpretation of the already scanty evidence is complicated by the Indo-European rules of dental stop sequences, whereby we are dealing with **-dzdh-* or the like rather than **-ddh-* pure and simple, Ever entirely in character, Latin seems to show three different outcomes: (1) *-st-*, (2) *-ss-*, and (3) *-V̄d-*.

The first is seen in *aestus* (*u*-stem) 'heat' < **aydh-tu-*, and *aestās* 'summer' < **aydh-to-tāt-* (Leumann 1977 § 171c, Pokorny 1959: 11–2). Leumann regards these derivations as unimpeachable evidence for **-ddh-* > *-st-*, though of course *aestās* could equally well be < **ayss-tāt-* < **aysso-tāt-* < **aydh-*

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to-tāt-, with the development of type (2), and *aestus* could could (somewhat feebly) be explained as a contamination of **aessus* by *aestāt-*. In any case the examples given exhaust the evidence.

Outcome (2) is attested in participles, namely *iussus* 'ordered' (*iubeō* < **yewdh-*), *fīsus* 'trusted' (*fīdō*), and *gressus* 'stepped' (*gradior*)—and, probably connected with the latter, *grassor* 'advance'. Leumann 1977 § 171c alludes vaguely to the possibility of 'jüngere Normalisierung' at work here, but it is not completely clear how leveling etc. would operate. *Iussus* could owe its form to the aorist (perfect) formation, via the intermediary of roots reflecting **t* and **d*, on the proportion *d* : *ss* : *ss* seen in *lūdō* 'play' : *lūsī* (< **lūssai* < **lŏwd-s-*) : *lūsus* (< **lussos* < **lud-to-*), whence *iussī* : **iustus* (like *aestus*) → *iussī* : *iussus*. Such an explanation is less cogent for *gradior*, however, which being deponent has no perfect stem at all. There is even room for wondering whether a form like *aestus* really must be taken as pure sound law over *gressus* etc. A form like *comēstus* (and derivatives) 'entirely consumed' < **-ed-to-* makes it clear that there are wrinkles even in the solidly-based reconstruction **-tst-* > *-ss-*, since the efforts known to me to account for the *-st-* in *comēstus* by analogy are all drastically far-fetched.

Type (3), hardly worse than type (1), is enshrined in a single form, *crēdō* 'believe'. The nearly unanimous opinion of scholars derives it from **k̂red-dheH*$_1$-, and compares it to Skt. *śraddhā-* 'trust', Av. *zrazdā-*. The further analysis of the form into **k̂red-* 'heart' and **dheH*$_1$- 'put', which is subject to dispute, is immaterial to the problem at hand, which is: how does **-edzdh-* become Lat. *-ēd-*? The informal principle cited above—that the 'shortest and most transfigured' reflex is generally taken to be the phonologically regular one—would seem to nominate *crēdō* for that honor; and even if one takes (1) or (2)—or even conceivably both—as being the regular reflexes of **-ddh-*, *crēdō* presents many puzzles. Szemerényi 1952: 49–50 is correct to reject the oft-repeated suggestion originating with Ascoli that contamination by *condere* and *abdere* disrupted the expected outcome of §*crestō* or §*cressō*. His own suggestion (laconically described as 'komplizierter' by Leumann 1977 § 171c) pro-

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ceeds from the observation that the correct formulation of the etymon of *crēdō*, at least as far as the present stem goes, is not based on Skt. *śraddhā-* but on *śrad-dadhāmi*, i. e. **k̂red-dhedheH$_1$-mi* (Szemerényi 1952: 50–1 writes **kret dhidhēmi*). It is true that, notwithstanding the primarily aorist character of the root *dhā*, Rigvedic Sanskrit has only one instance of *śrat(h)* with an aorist stem of *dhā* (next to 5 presents, one causative, 2 participles, and an infinitive). According to Szemerényi's convictions about the sound laws, this (admittedly highly plausible) **k̂red-zdhedheH$_1$mi* would have given Lat. **krestidō*. With the syncope of the reduplication vowel, the development would have been **-estd-* > *-ēd-* (51). Szemerényi does not comment on the phonetic details of this development, which is unfortunate, since the proposed phonetics are surprising. Close parallels are lacking, but unless one appeals to interference from *condere* and *abdere*, which on semantic grounds is hardly plausible, one might anticipate, rather, §*crestō*, indistinguishable from the aorist forms. *Quīndecim* '15' < **quinquedecem*, *ē-dūcō* 'lead out' < **eks-dewk-*, etc., are all forms where the functional relationship between the complex forms and their simplex ingredients is so straightforward that the development of their clusters across morpheme boundaries has minimal evidential value. For somewhat the same reasons *necdum* 'and not yet' alternating with and derived from *nequedum*, cannot be used as evidence, and besides the *-kd-* cluster is presumably not of the same chronological layer as Szemerényi's *-ēd-* < **-estd-*.

Pure reason, that is to say dead reckoning, is the least reliable principle in historical phonology, but is not wholly useless. Let us at least look at a sequence like **-gdh-* from the point of view of the probabilities, based on more certain aspects of Latin phonology. Taking **dh* > PItal. **ƀ* as a given,[3] the three possible outcomes at the PItal. level would be **kƀ*, **xƀ* (*x* = velar spi-

[3] Szemerényi 1952 has argued at length that this sequence of developments is not quite as inevitable as traditionally held. That is certainly true; it seems to me, however, that Szemerényi considerably overstates the objections to the phonetics of the traditional interpretation. Stopification (as it were) of voiced slit spirants is an enormously commonplace diachronic occurrence.

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rant), or **gþ*. Of these the first two look more likely; but the last, especially across a morpheme boundary, in a late coinage, or in a form for any other reason morphologically transparent, is far from impossible. For instance, many speakers of English (including me myself) have /widθ/ and /bredθ/ for *width*, *breadth*, instead of the much more likely-looking /witθ/ /bretθ/ (like /streŋkθ/ and /leŋkθ/, whose voiceless stops perhaps survive unaltered because the simplex forms /strɔŋ/ /lɔŋ/ have no /g/ to compete with /k/). More to the immediate point, *crēdō*—vexed as the question is—seems to say that one possible outcome of a cluster of voiced stop plus voiced aspirate retained (or reacquired) some degree of voicing; this rules out **kþ* and makes **xþ* unlikely, since **ss* (from whatever source)—the only certain instance of long voiceless spirant—remains voiceless intervocalically. Similarly *-st-* < **-sþ-* < **-zdh-*, Szemerényi 1952: 44ff., is supported by some good etymologies and seems to be nearly as good evidence as **ss*. Something like **gþ* would inevitably have become **gð*, in accord with the readiness of short spirants to become voiced in any sort of a voiced environment. After all, to assume that *-bulum* and *-bula* are lurking in *tēgulum rēgula* etc. is on the face of it virtually to guarantee that the forms are not of PIE. date, since—like all instances of *-bula/bulum* in Latin—they lie outside the expected PIE. distribution of **-dhlo-*. That is, the forms could well have been coined in a barely pre-Latin period as **rĕg-ðlā-* and **tĕg-ðlo-*.

If **-ĕgðl-* is granted, there are two possible developments, **-eggl-* and **-egll-*, with little to choose between them. Both are plausible starting points for *-ēgul-* as attested. The former possibility is relatively clear-cut: Latin had a more or less optional trade-off between the length of a vowel and that of the following consonant (Buck §§ 208–9, Leumann 1977 §§ 184–5). We have seen an example of that in *lūsus* 'played', in which secondary *lūs-* prevailed over etymological **lŭss-*, presumably because of the attraction of the aorist *lūsī* < **lūssai* and the frequent occurrence of *ū* in other forms as well (*lūdō*, *lūdicer* 'playful', *lūdus* 'game', etc.). Since there are clear derivatives from *reg-* and *teg-* in Latin with long root vowels, it is easy to find support for the replacement of **-ĕgg-* by *-ēg-*. But it is at least as possible

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that *-$\bar{V}GG$- > -$\bar{V}G$- (G = any voiced stop, V = any vowel) was simply a sound law; certainly in Latin vocabulary, *-GG-* sequences are limited to two types, namely a fusion of a prefix with a root (e.g. *suggerō* 'bring under') and presumed nursery words (or the like, e. g. *gibber* 'hump-backed', Leumann 1977 § 184bβ). Such a sound law, if it existed, was minor indeed: I can locate no sure examples of it. In any case, the details in the development of an original **-ĕgll-* are even harder to speculate about than for **-ĕggl-*, beyond the certainty that somehow the long *ll* would be reduced; compensatory lengthening of the vowel would be an unsurprising consequence, whether directly (**-ĕgll- > *ēgll*) or by way of an intermediate step (say, **-ĕgll- > -ĕggl- > *-ēgl-*).

One remaining question: are there any other, similar formations that are parallel to the forms just discussed? Leumann 1961 § 172 IV B reflects the opinio communis in surmising that forms like *fulcrum* 'prop; bolster' (*fulciō*) and *speculum* 'mirror' (*speciō* 'look, gaze') are built from *t*- less forms of the nomen instrumenti suffix. That certainly could be, but as Leumann 1977 § 160b suggests, underlying **folktrom *spektlom* etc. are entirely plausible phonologically; furthermore, such etyma would (a) distribute the known allomorphs of the 'tool' suffix with greater uniformity, and (b) help account for the rise of the functionally-equivalent *t*-less suffixes *-rum* and *-ulum* in Latin. It is unclear whether Latin inherited the latter from PIE. Leumann 1977 § 283 1a asserts that they are 'im Kern ererbt', but the use of plain *l*-suffixes for nomina instrumenti is not often encountered outside of Latin (Brugmann 1906 § 285aβ). The starting point for the formation, whether Italic or earlier, is presumably the PIE. affix **-lo-* which evidently formed adjectives parallel to (and possibly arising from, by dissimilation) the formant **-ro-*. Both made in fact not simply adjectives, but as primary derivatives formed pseudo-participles ('participial adjectives') of active meaning. Participles and the like easily become agent nouns (and vice-versa), and of course adjectives are effortlessly nominalized in the early Indo-European languages; accordingly it is a simple matter indeed to move semantically from **dhiĝh-lo-* 'modeling' to nominalized 'modeling one' to purely agentive

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'modeler', hence Lat. *figulus* 'potter'. Now, it is true that many a 'tool'-word is little more than a neuter sort of agent noun—as in Eng. *scraper*, *eraser*, *roller*, and so on—but the meaning of forms built with **-tlo-* etc. is typically different from 'something that does so-and-so' (the normal thrust of the agent-type formation); rather, it is 'something that you use to do so-and-so with'. Hence a **dhuĝh-tro-* is not quite accurately a 'milker', but a 'thing you use to milk with' (specifically 'milk pail'); a **peH$_3$(y)-tlo-* is not 'a thing that drinks', but a 'thing you use to drink with', i. e. a 'cup'. In this vein, it is noteworthy that a *speculum* is not a 'thing that sees' but a 'thing you use to see with', which points away from an adjectival element in **-lo-*.

As Leumann notes (1961 § 172 IV B), the *t*-less formation in Latin is particularly common where roots end in dorsal stops, and there thinks in terms of *-culum* avoiding such roots for dissimilatory reasons. It makes better sense all round to hold that *-rum* and *-ulum* arose after dorsal stops as the result of sound laws operating on **-tro- *-tlo-*. Almost accidentally, this set the stage for reanalysis whereby *speculum* and *fulcrum* could be segmented as *spec-ulum fulc-rum*. This would be sufficient to account for the *t*-less suffixes with the appropriate function in Latin, which were never very productive in any case. Evidence for a PIE. **-lo-* isofunctional with **-tlo-* (as opposed to isofunctional with **-ro-*) strikes me as unpersuasive; but see Solta 1970. It might be added that compensatory lengthening to *§spēculum* *§fūlcrum* analogous to *rēgula* etc. is not looked-for here. The loss of a consonant in voiceless clusters does not call for compensatory lengthening; thus *Sestus* < **Sektos*.

There is another phonological detail that needs tidying up. Buck § 207,2, Leumann 1977 § 221 b, and many others point out that **-lkt-* > Lat. *-lt-* regularly, e.g. *fultus* 'propped' > **folktos*, pple. of *fulciō*. Accordingly, *mulctus* 'milked' and *mulctrum* 'milk pail' from *mulgeō* are accounted for by reintroduction of the *-k-*. Reintroduction is the proper explanation, but it must be remembered that **-lktr-* is not bound to behave exactly like **-lkt-V-*, as the reduction of consonant clusters in Latin is rather unpredictable on just such points. My opinion is that *fulcrum* probably

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represents the original reflex of **-lktr-*, viz. **folktrom*; accordingly, *mulctrum* was refashioned from **mulcrum* rather than from **multrum*. There remains a bit of a puzzle as to why the consonant clusters in derivatives of *mulgeō* were restored, contrary to the usual custom of Latin. Leumann 1977 § 221 b wonders whether **multus* 'milked' became *mulctus* to avoid homonymy with *multus* 'many'. This somewhat prescientific notion is best discarded in favor of taking **multus* < **molktos*, and **mulcrum* < **molktrom* to have been the regular outcomes of the clusters involved. There would surely have been a good deal of talking about milking in contexts where milk pails were also referred to, so that it is straightforward to take attested *mulctus mulctrum* for **multus *mulcrum* as ordinary examples of paradigmatic leveling. A plausible parallel (but without the criss-cross contamination) is attested by *tortus* 'twisted' vs. *torculum* 'a press' if the latter is from **tork(ʷ)-tlo-* rather than (by conventional wisdom) from **tork(ʷ)-lo-*. That is, probably **-rkt-* yields Lat. *-rt-* exactly like *-lt-* < **-lkt-*, thus *tortus* < **tork(ʷ)-to-*; correspondingly *-rkl-* < *-rktl-* would be the counterpart to *-lkr-* < **-lktr-*, hence *torculum*. The weakness in this line of argument of course is that the putative affixes **-tlō-* and **-tro-* are equally covert and hypothetical in both *torculum* and *fulcrum*.

Finally, some forms that bear more directly on the structure of *rēgula* etc. are: *repāgula* pl. tant. 'barriers (at the circus)', *cingulum* 'belt', *rādula* 'razor', *coāgulum* 'rennet' and *trāgula* 'dragnet'; these too might be reflexes of forms in **-dhlo-*. *Iŭgulum* 'collar-bone; throat' on account of its short vowel must be taken as an instance of a *t*-less, or rather **dh*-less, formation. Functionally too it can hardly be taken as a specimen of a tool formation, since it is hardly 'something you use to yoke with', but rather a 'yoker' (**-lo-* pseudo-participle) or even a 'little yoke' (diminutive).

The first two forms in the preceding paragraph are equivocal both formally and functionally. *Cingulum* might reflect **kingdhlo-* with *-ngulum* < **-ngdhlom* quite parallel to *-rculum* < **-rktlom* as in *torculum*; but there is no particular evidence for it in preference to conventional **king-lo-*. Semantically a 'belt' might appear to be 'a thing you use to gird with' rather than 'a girder';

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this would favor a 'tool' formation, but a pan-Germanic formation exemplified by Eng. *girdle* 'belt' < OE. *gyrdel* indicates that a pseudo-participle in **-lo-* is just as possible.

Repāgula, if from **pāg-* 'fit together' at all, suffers from the same ambiguities as *cingulum*, and has difficulties all its own. If from **păg-* a development **păgdhlo-* > *-pāgulo-* would replicate every detail of the histories of *rēgula* and *tēgula* proposed here. Unfortunately, the shape *pāg-* is well-attested in Latin, alternating (in effect) with *păc-* and less commonly *pāc-*. Altogether the details of the whole family are too confused regarding both form and meaning for any confident opinion about the probable structure and history of *repāgula*.

The vowel of the root underlying *rādula* appears to be long, viz. *rādō* 'scrape, rub, shave', pple. *rāsus* presumably < **rāssos* by regular sound law. On the other hand, except for these few Latin forms in *ā*, the root appears to be a fairly ordinary ablauting root with long vowel, *rēd-/rōd-* (Pokorny 1959: 854)—that is, presumably **reH$_1$d-/*roH$_1$d-*; cf. Lat. *rōdō* 'gnaw'. It is not readily believable that an inherited paradigm **rădō*, **răssai*, **răssos* < **r̥Hd-ō* **r̥Hd-s-*r̥Hd-to-* would have spontaneously lengthened the root vowel throughout, since *-V̆ss-* sequences generally remain unless influenced by a long vowel elsewhere in the paradigm, as exemplified by *lūsus*, above; moreover *missus* 'sent' shows that *-V̆ss-* > *-V̄s-* is not inevitable even when there is a long vowel handy (perf. *mīsī*) and the force sometimes runs in the contrary direction (*mittō* probably from **mītō* thanks to *missus*, Leumann 1961 § 132b, though in 1977 § 184c 'ohne Rechtfertigung'). A paradigm with no long vowels, like *gradior* 'step', is expected to leave a participle like *gressus* undisturbed, and accordingly one would expect inherited **răssus* to behave like *gressus*. Pokorny, partly on the basis of Welsh evidence, posits **rəd-d(h)-* > Lat. *rādō*. The function of the root enlargement is unclear, but that is always a trouble with enlargements and perhaps no real objection; more of an obstacle is the acceptability of PItal. **răd-* as a possible zero-grade of **reHd-*. Such phonology is in my opinion unsatisfactory. Happily, the vastly more likely **r̥Hd-* (= **r̄d-* in Brugmann's system) would presumably yield Lat. *rād-* by sound law. This would have been

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the expected root form underlying the pple. *rāsus* < **ṛHd-to-*; the original shapes of both *rādō* and especially *rāsī* are less obvious, but the point is that even if the length of the vowel of the root is somewhat uncertain, by at least two very different analyses the root (or pseudo-root) in the Latin verb would wind up as *rād-*. Accordingly, the length of the vowel in *rādula* is not the red flag that it is *rēgula* and *tēgula*. There is, however a very good reason for seeing **rād-dhlo-* (or the like) as the etymon, a reason quite different from any arguments available for the explanantion of *rēgula* etc.: Lat. *rāllum* 'scraper (esp. for cleaning plowshares)' must reflect **rād-lo-*, as the formal, functional, and phonological details all fit perfectly. *Rādula* accordingly must be derived from a different formation, and we have a plausible candidate for that in **rād-dhlo-*.[4] If so, moreover, we have here another example to add to *crēdō* as an instance of **-ddh-* > *-d-*.

In *coāgulum* 'rennet' we have a case nearly as clear as *rēgula* etc. The formation is ambiguous functionally, however: it could with equal plausibility be traced to a pseudo-participle in **-lo-* ('clotter') or a tool suffix in **-dhlo-* ('means used to make dairy curd'). The vowel length, in my opinion, emphatically favors the second alternative. If Leumann's derivation of *figulus* from **dhiĝh-lo-* is valid (1977 § 168 Zusatz), *coāgulum* cannot reflect **-ăg-lo-*. On the other hand, the theory proposed here to account for the structure of *rēgula* etc. would predict **ăg-dhlo-* > *āgulu-*. So far as I can see, there is no good reason to doubt that the word springs from the familiar **ăĝ-* 'drive' (*agō*), and no analogical source for the *ā* is to be found elsewhere in the

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[4] This analysis presupposes a particular relative chronology for some sound changes. The anaptyxis of **-l-* to *-ul-* (or rather, **-ol-*) must precede the development of *-d-* from **-dð-*. Otherwise, if the two developments were in the reverse order, **rādðlo-* would have fallen together with **rādlo-*, both resulting in *rāllum*.

There is a possibility, presumably wholly speculative, that *rādula* and *rāllum* are indeed from a single etymon, **rād-lo-* or the like, and are doublets. But the regular Latin development of **-dl-* is *-ll-*, there seem to be no clear cases of *-dul-* < **-dl-*, and note that the rustic word —that is, the word that might show a non-standard reflex —is *rāllum*, with the expected outcome of **-dl-*.

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paradigm. (An appeal to *āctus*, *āctiō*, etc., would surely be far-fetched.)

Trāgula, finally, I think must be taken to confirm the essential details of the theory here proposed no less emphatically than *coāgulum*.[5] It is certainly derived from *trăhō* 'drag, pull', viz. **tragh-* (**dhragh-*) (Pokorny 1959: 1089). It has a peculiar shape: the sequence of a voiceless stop and voiced aspirate in the same root violates a well-known constraint on PIE. root shapes (see Szemerényi 1952: 37), but that need not distract us; and it occurs in a somewhat suspect distribution, being attested apart from Italic only in Germanic, Celtic, and marginally in Slavic. More of a problem is the attested pattern of vowel alternation, *ĕ* ~ *ŏ* ~ *ă*, which is like nothing else in the world; it complicates the question of whether the long vowel in *trāgula* is inherited or, like that of *rēgula* etc., a Latin development. Of course, within the context of purely Latin structures, the vowel of the root presents no unusual appearance: the alternations in *trahō trāxī tractus* (*trāctus*? Evidence seems to be lacking) remind one of *regō rēxī rēctus*, and many others, while the consonants jibe with e. g. *scrībō scrīpsī scrīptus*. Altogether, then, the basic paradigm of *trahō* and the numerous derivatives in *tract-* can be derived from a single etymon **trăgh-*. Only *trāgula* shows a form in *-g-*; no words are attested which would prop up an explanation accounting for the replacement of expected *-h-* by analogy, in contrast to the celebrated *figūra* case (Leumann 1977 § 168 Zusatz a); if §*trahula* is the reflex of **tragh-lo-*, such a form would not have been any more subject to innovation

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[5] *Trāgula* is polysemic: (1) a kind of javelin of non-Roman type; (2) a dragnet; (3) a kind of sledge. Despite Varro's attempt to etymologize the first meaning from *trahō*, it seems unlikely that it would have any necessary connection with that (or any other) Latin word, and the explanation ventured seems decidedly far-fetched to my taste. Meaning (3) is apparently a diminutive of *traha* or *trahea* 'sledge', which by contrast is obviously from *trahō*. If from **tragh-lo-* it would tend to confirm: (a) Leumann's derivation of *figulus* 'potter' from **dhiĝh-lo-* by sound-law rather than by analogy (see below); and (b) the etymon *trāgh-*, since it is after all *fĭgulus* rather than §*fīgulus* < **dhĭĝh-*.

In the present discussion we are concerned only with meaning (2), in which the employment of a tool suffix is probable.

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than e. g. *vehiculum*. (Leumann 1977 § 168 Zusatz a speculates that *figulus* might be the lautgesetzlich outcome of **dhiĝh-lo-*, which would of course equally well explain *trāgula* if from **trāgh-lo-*. Sound laws aside, however, **-lo-* is functionally proper in *figulus* but harder to justify in *trāgula* 'dragnet'.)

The long vowel of *trāgula* is less of a crux than the *-g-*, because it is not known whether the root was **trăgh-* rather than **trāgh-* in the first place—unlike *rēgula* etc., in which an etymon in **-ē-* is out of the question. Ernout & Meillet s. v. *trahō* seem to declare in favor of an etymologically short vowel, as they refer the reader to *rēgula* and *tēgula* for a suitable parallel to *trāgula* vis-à-vis *trăho*. The root vowel of *trahāx* 'grasping, greedy' (Plautus) and *trahō* is definitely short, that of *trāxī* appears to be long, according both to epigraphic evidence and a not-wholly-trustworthy statement by Priscian (Meillet & Vendryes 1960 § 399); that of *tractus*, *tractiō*, etc., is uncertain. It is possible that *trahō* reflects **trāhō*: an originally long vowel might have been shortened by contamination with *vehō*, which is after all temptingly similar semantically as well as formally; or *trahō* < **trāhō* might be a case of vocalis ante vocalem corripitur, like *creō* < **crēō* 'produce', since the process apparently took place before *-h-* (thus *prehendō* 'catch' < **prēhendō* < **praehendō*).

It is here proposed that *trāgula* reflects **trăgh-tlā-*, that is **trăg-dhlā-*. The etymon with a short vowel would provide the most completely gratifying conformation of the suggested analysis of *rēgula* etc., but even **trāgdhlā-* is welcome. We have previously commented that, for several reasons, *rēgula tēgula* are best traced to Italic formations rather than directly to PIE. **reĝ-dhlo- *teg-dhlo-*; the most potent reason of course is that such formations do not conform to the expected distribution of the element **-dhlo-*, but there are phonological uncertainties entailed in deriving Lat. *-g-* from **-gdh-* as well. The etymon of *trāgula* could likewise date from a relatively late period in pre-Latin history, if the actual neologism was compounded of **traγ-* and **-ðlo-*. Unlike *rēgula tēgula*, however, it could be as old as PIE. The phonological difficulties remain the same, of course, but if truly inherited, *trāgula* would be a uniquely interesting formation: (1) it would add to the patheti-

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cally few attestations of Bartholomae's Law in Latin; (2) it would be the only instance of **-dhlo-/*-dhro-* in Latin in the etymologically correct environment; and (3) together with *rādula* it would tend to suggest that, whatever the hidden details, *crēdō* is after all a possible outcome of **ḱred-dheH$_1$-* via sound change.

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Zu einer strukturalistischen Semasiologie

Im Jahre 1964 hat E. Coseriu einen berühmt gewordenen Aufsatz mit dem Titel „Pour une sémantique diachronique structurale" veröffentlicht (Travaux de linguistique et de littérature, publ. p. le Centre de Philologie et de littérature romanes de l'université de Strasbourg II: 1, 139ff.). Der Aufsatz ist später ins Spanische übersetzt und zusammen mit anderen Arbeiten von ihm in einem Sammelband gedruckt worden: „Principios de semántica estructural", Madrid 1977. Meine Verweise beziehen sich auf diese vom Verfasser durchgesehene spanische Version.

C. will im erwähnten Aufsatz zeigen, daß sich Wörter ebenso wie Phoneme in strukturellen Einheiten organisieren lassen und daß sich der Platz eines Wortes in der lexikalischen Struktur durch seine 'Opposition' zu anderen Wörtern bestimmen läßt. Um diese Behauptung zu stützen, werden u. a. folgende Schemata von einer lateinischen Wortgruppe entworfen (29f.):

Latein	Spanisch
senex	viejo
uetulus	
uetus	
iuuenis	joven
nouellus	
nouus	nuevo

	alt	nicht alt
für Personen	senex	iuuenis
für Tiere und Pflanzen	uetulus	nouellus
für Dinge	uetus	nouus

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Diese Schemata sind in der Tat den von den Phonologen entworfenen ganz ähnlich, und man könnte nun für die semantische Entwicklung dieser Wörter vom Lateinischen zum Spanischen eine phonologische Terminologie verwenden: die im Lateinischen vorhandene Opposition zwischen den Wörtern *senex*, *uetulus*, *uetus* ist neutralisiert, usw[1].

Der Wortschatz einer Sprache ist ja nun unendlich viel größer und mannigfacher als das Phoneminventar; ist es überhaupt möglich, die meisten Wörter in ein Netz einzufangen, das für einpaar Dutzend Phoneme bestimmt ist? C. ist sich natürlich dessen bewußt, daß er die sprachliche Wirklichkeit vereinfacht hat, bemerkt er doch S. 29 Anm. 13: "Las estructuras léxicas que siguen han sido simplificadas, en parte, en el sentido de que, sin que hayan sido 'falseadas', pueden ser incompletas. Así, en latín existían en la misma esfera semántica: *antiquus*, *grandis*, *recens* ... Nuestro fin no es aquí un fin descriptivo, es decir, el de considerar estas mismas estructuras en detalle, sino un fin teórico y metodológico: el de mostrar el sentido en el que conviene estudiarlas." Aber auch wenn wir die erwähnten Synonyma und andere, die unleugbar das Bild komplizieren und die schöne Symmetrie zu zerstören drohen, beiseitelassen, bleibt die Frage, ob nicht die obigen Schemata der sprachlichen Wirklichkeit Gewalt antun. Um diese Frage zu beantworten, sei C.s lateinisches Schema, und zwar in erster Linie die Verwendung der Wörter *nouellus* und *uetulus* vom alten bis zum späten Latein hier etwas näher untersucht.

Zuerst ist zu bemerken, daß das von C. entworfene spanische System in den meisten idg. Sprachen sein Gegenstück hat: es gibt verschiedene Wörter für 'neu' und 'jung', aber der Gegensatz beider wird mit nur einem Wort für 'alt' bezeichnet, vgl. C. D. Buck, A Dictionary of Selected Synonyms in the Principal Indo-European Languages (Chicago 1965) 957f.[2]. Das lateinische System, wie es C. darstellt, stellt also eine Ausnahme dar, und

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[1] Die Schemata sind u. a. von R. Anttila, An Introduction to Historical and Comparative Linguistics (1972) 135 abgedruckt worden.

[2] Dort wird auch bemerkt, daß die Wörter für 'neu' und 'jung' bisweilen ihren Platz tauschen, so z.B. griech. νέος 'jung', das ja mit dt. *neu*, lat. *nouus* identisch ist.

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in erster Linie fallen *uetulus* und *nouellus* als Spezialattribute für Tiere und Pflanzen auf.

Nach C.s Schema werden *uetus* und *nouus* hauptsächlich von Sachen gebraucht. Das trifft für *nouus* zu, und der Grund ist einfach, daß es 'neu', nicht 'jung' bedeutet. Es gibt in der Tat auch alte Belege für *nouus* bei Personenbezeichnungen, und da bedeutet es eben 'neu (geworden)', z. B. Plaut. Capt. 362 *nouus erus*, Asin. 185 *nouus amator*, Ter. Ad. 938 *nouus maritus*; ebenso das ciceronische *noui homines*.

Es ist demnach zu erwarten, daß sein Diminutivum *nouellus* — außer bei Tier- und Pflanzennamen — hauptsächlich bei sächlichen Appellativen, nicht bei Menschenbezeichnungen vorkommt. In der Tat ist die letztere Verwendung nur sehr spät und selten; ich vermag lediglich etwa Ennod. epist. 1,5 (S. 10,1 Hartel) *nouellus consul* zu zitieren, wo die Bedeutung auch 'neu', nicht 'jung' ist. Aber auch der Gebrauch von *nouellus* bei sächlichen Appellativen ist im archaischen und klassischen Latein selten: das Wort wird, wie C. sagt, vorzugsweise von Tieren und Pflanzen gebraucht, und wenn es als Bestimmung eines leblosen Gegenstands auftritt, ist die semantische Verbindung mit Tieren und Pflanzen zumeist deutlich, z.B. Ovid. ars 2,698 *prata nouella*, Colum. 3,13,7 *nouelli seminis*, 3.17,3 *nouello sarmento*, 4,22,5 *nouellis frondibus*, Hippocr., De obseruatione ciborum (5./6. Jh.; ed. Mazzini, Romanobarbarica 2, 1977) S. 333 und 378f. *uinum nouellum*[3] oder Ovid. ex Ponto 3,7,16 *colla nouella* (von einem Stier). — Die Grammatiker verwenden bisweilen *nouellus* mit Bezug auf einen neuen Sprachgebrauch, z. B. Fronto S. 146,15 (v. d. Hout = 153,4 N.) *elocutione nouella*; Ter. Maur. 1973 (gramm. 6,384) *exempla nouella*, Diom. gramm. 1,334,28ff. *Antiquitatis ... Romani memores dualem numerum posteritatis usu receptum quasi nouellum usurpare noluerunt*, Aug. gramm. 5,514,42 *nouellum genus uerbi*. Auch diese Verwendung des Wortes läßt sich aus seiner Verbindung mit Pflanzennamen herleiten; der folgende wohlbekannte Satz des Horaz spiegelt die Sprachauffassung der Alten wider: ars 60ff. *ut siluae foliis pronos mutantur in annos, prima cadunt, ita uerborum uetus interit*

[3] Vgl. auch die Ableitung *nouellare* 'neue Äcker bebauen'.

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aetas et iuuenum ritu florent modo nata uigentque. — Schon im älteren Latein gibt es indessen Belege ohne Verbindung mit Pflanzen und Tieren wie Liv. 2,39,3 *nouella oppida* (Wölfflin, Ausgewählte Schriften, 1933, 118), Ovid. ex Ponto 4,12,24 *frena nouella*, und im späten Latein tritt *nouellus* immer häufiger für *nouus* ein, z.B. *Nouellae* (sc. *constitutiones*) im Corpus Iuris, Tert. adu. Hermog. 1,2 *doctrina ... nouella*, Cypr. epist. 63,1 *humana et nouella institutione*, Vopisc. Tac. 1,1 *nouello ... imperio*, Capitol. Pert. 10,9 *nouellarum rerum* (nach Stangls Emendation, die Hss. haben *nonnullarum*), Arnob. nat. 1,2 (S. 4,19 Reifferscheid) *nouellarum* (sc. *formarum*), 1,3 (S. 6,4) *nouella ... mala*, 2,66 (S. 101,21f.) *nouellam ... religionem*, Cassiod. uar. 2,28,4 *nouella usurpatione*, hist. 5,20,4 *nouellam expositionem*, Ennod. epist. 1,9 (S. 19,6 Hartel) *nouellum usum*, 5,25 (S. 145,11) *negotia nouella* (weitere Belege aus demselben Autor bringt A. Dubois, La latinité d'Ennodius, 1903, 147), Juan de Biclaro (um 600; hrsg. v. J. Campos 1960) S. 89,201 *nouello errore*, Adam v. Bremen, Gesta (ed. Schmeidler) S. 18,6 *nouellam ... conuersionem* (auch 44,19; 83,3; 111,16). Im Französischen und Italienischen lebt *nouellus* neben *nouus* weiter, und zwar ohne jegliche Beschränkung auf Pflanzen und Tiere. Im Spanischen und Portugiesischen dagegen findet sich *nouillo* bzw. *nouelo* in der Bedeutung 'junges Rind'; diese Spezialbedeutung, die auch im Sardischen ihr Gegenstück hat (Meyer-Lübke, Rom. etym. Wb. Nr. 5967,2), geht offensichtlich auf die alte Verwendung des Wortes bei Tiernamen zurück und ist von C. nicht berücksichtigt[4].

Entgegen dem Schema C.s begegnet *uetus* im Sinne von 'alt' vom ältesten Latein an mit Bezug auf Menschen, z.B. Plaut. Merc. 291 *senex uetus decrepitus*, Poen. 978 *seruos ... ueteres antiquosque*, Afranius 251 *uiuax uetus*, Ter. Andr. 7 und Heaut. 22 *uetus poeta*, Eun. 688 *uetus ueternosus senex*. Öfter steht es aller-

[4] *Nouellus, -a* 'junges Rind' begegnet mehrfach in alten Urkunden aus Spanien, z.B. im Cartulario de San Vicente de Oviedo (ed. L. Serrano 1929) Nr. 22a. 978, 35a. 1042 (vgl. A. C. Jennings, A Linguistic Study of the Cartulario de San Vincente de Oviedo, 1940, 302), Libro Becerro del Monasterio de Valbanera (ed. Lucas Alvarez, Estudios de Edad Media de la Corona de Aragón 4, 1951) Nr. 114a. 1079, 146 a. 1081.

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dings im Gegensatz zu *nouus*, nicht wie in den obigen Belegen zu *iuuenis*, vgl. etwa Plaut. Capt. 363 (*nouus erus . . .*) *ueteri domino*, Cist. 199 *socios ueteres et nouos*, Cic. Lael. 67 *num quando amici noui . . . ueteribus sint anteponendi.*

Der Gegensatz von *iuuenis* ist gewöhnlich *senex*. Bei der Verwendung von *senex* — *uetus* ist aber zu beachten, daß *uetus* praktisch nur im Plural als ein substantiviertes Maskulinum vorkommt, und zwar bedeutet ja *ueteres* 'die Alten' (im Gegensatz zu den heutigen Menschen). Singularisch im Sinne von 'Greis' ist *senex* alleinherrschend; ich kenne keinen Beleg für substantiviertes *uetus* in dieser Bedeutung (außer vielleicht dem oben zitierten Afran. 251, der allerdings fragmentarisch ist).

Auch *uetulus* begegnet sowohl im alten als auch im späten Latein bei Personenbezeichnungen, z.B. Plaut. Epid. 666 *nos uetulos decrepitos duos*, Merc. 314 *uetulus decrepitus senex*, Pers. 266 *homines uetulos auidos*[5]; Most. 274f. *istae ueteres . . . uetulae edentulae*, Lucil. 282f. (Marx) *uetulam atque uirosam uxorem*, Paul. Diac. Lang. 4,37 *mulier iam uetula*. In allen diesen Belegen (außer bei Paul. Diac.) ist die Bedeutung des Wortes mehr oder weniger pejorativ. Das fem. *uetula* hat überhaupt im vormittelalterlichen Latein in der Regel einen pejorativen Sinn, vgl. noch etwa Iuv. 6,241 *turpi uetulae*, Mart. 3,32. Das mask. *uetulus* braucht dagegen nicht pejorativ zu sein; Cicero redet z.B. fam. 7,16,1 einen Freund mit *mi uetule* an, eine Anrede, die dem frz. *mon vieux* völlig entspricht[6].

Daß das Wort mit Bezug auf Tiere und Pflanzen gebraucht wird, braucht hier nicht belegt zu werden.[7] Nur sei die folgende Stelle zitiert, aus der hervorgeht, daß *nouellus* und *uetulus* in dieser Verwendung als eine Art termini technici betrachtet wurden: Cic. fin. 5,39 *earum etiam rerum, quas terra gignit, educatio quaedam et perfectio est non dissimilis animantium; itaque*

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[5] v. Wartburg, Frz. etym. Wb. s.u. behauptet zu Unrecht, das Wort komme im alten Latein nur substantiviert vor.

[6] Vgl. Wölfflin, Ausgewählte Schriften 118 sowie Verf., Zeitschrift für romanische Philologie 83 (1967) 612 und Anm. 7 (weitere Belege für pejorative Feminina).

[7] Bei Colum. 8,8,7 z.B. werden sowohl *uetulus* als auch *nouellus* mit Bezug auf Tauben gebraucht.

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et 'uiuere' uitem et 'mori' dicimus, arboremque et 'nouellam' et 'uetulam' et 'uigere' et 'senescere'.

Wenn *uetulus* in Verbindung mit Bezeichnungen für leblose Dinge gebraucht wird, handelt es sich gewöhnlich — ähnlich wie bei *nouellus* — um Teile oder Produkte von Pflanzen, z.B. Catull. 27,1, Mart. 1,18,1 u. a. *uetulum Falernum* (weitere Belege bei M. Haupt, Opuscula 2,125), Colum. 10,428 *uetulo ... uino*, Porph. Hor. S. 114,31 (Holder) *per faecem ... uetulam uetus uinum accipe*, Paul. Fest. S. 227 (M.) *uitem uetulam*. Da sowohl *uetus* wie *uetulus* bei Personenbezeichnungen auftritt, fällt es nicht auf, daß *uetulus* bisweilen mit Beziehung auf menschliche Körper(teile) gebraucht wird, z.B. Mart. 11,29,1 *uetula ... dextra*, 10,90,1 *uetulum cunnum*, Cypr. Gall. Gen. 755 *uetulo ... corpore*. Sonst sind aber Belege für *uetulus* bei leblosen Appellativen sehr selten; ich vermag keinen zu zitieren, und im Material des ThLL. finden sich keine, wie mir Dr. W. Buchwald (München) freundlichst mitteilt.

Vetus ist ja mit dem griech. ἔτος verwandt und bedeutete ursprünglich 'einjährig'; wahrscheinlich wurde es zuerst mit Bezug auf Wein gebraucht (Ernout-Meillet, Dict. étym. s. u.). Die Verwendung von *uetus* mit Bezug auf Tiere und Pflanzen hat sich leicht hieraus entwickeln können. Wie wir sahen, werden beide Wörter schon im ältesten Latein bei Personenbezeichnungen gebraucht. Aber während *uetus* von der ältesten Zeit an auch mit Bezug auf beliebige leblose Gegenstände gebraucht wird, ist diese Verwendung von *uetulus* bis ins späte Latein selten. Die von den romanischen Sprachen vorausgesetzte Verallgemeinerung von *uetulus* (frz. *vieux*, ital. *vecchio*, span. *viejo*) scheint erst spät eingetreten zu sein[8].

Wir haben also gesehen, daß die von C. für das Latein entworfenen Schemata in vielen Punkten nicht stimmen. Sie nehmen auf den grundlegenden Unterschied zwischen 'jung' und 'neu' keine Rücksicht; Belege vom Typus *nouus maritus* fanden deshalb keinen Platz. Die immer häufigere Verwendung von *nouellus* vor sächlichen Appellativen und die zahlreichen Belege

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[8] Zu der lange Zeit andauernden Konkurrenz zwischen *uetus* und *uetulus* in Italien s. Aebischer, Cultura Neolatina 4–5 (1944–45) 113ff.

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für *uetus* und *uetulus* vor Personenbezeichnungen vom archaischen Latein an wurden nicht berücksichtigt, ebensowenig die verschiedene substantivierte Verwendung von *uetus* und *senex*. Jede Schematisierung bedeutet natürlich eine gewisse Vereinfachung, aber C.s Schemata sind so stark vereinfacht, daß sie jenen „falseadas" (oben S. 176) recht nahe kommen. Noch wichtiger ist es, daß die von C. als ebenbürtig behandelten Wortpaare nicht gleichmäßig sind und sich nicht gleichmäßig entwickelt haben: *Vetus* wird schon im alten Latein bei Personenbezeichnungen öfters gebraucht, *nouus* dagegen nur im Sinne von 'neu'; *uetulus* tritt auch schon früh bei Personenbezeichnungen auf, während dieser Gebrauch von *nouellus* äußerst selten und spät ist; *nouellus* wird im späten Latein öfters mit Bezug auf Sachen gebraucht, aber fast nie *uetulus*. Die von C. vorausgesetzte Symmetrie in Gebrauch und Entwicklung der Wörter für 'alt' und 'jung' im Lat. ist einfach nicht vorhanden.

Trotzdem ist C. in der hier besprochenen Arbeit wie immer anregend und interessant. Die Frage, ob gewisse Adjektive vorzugsweise mit Bezug auf lebende Wesen, andere mit Bezug auf leblose gebraucht werden, und inwieweit die Sprachen in dieser Hinsicht auseinandergehen, ist sicher einer näheren Untersuchung wert. Bereits A. Schopenhauer, Die Welt als Wille und Vorstellung 1,426 (ed. Deussen) machte darauf aufmerksam, daß bei „erkennenden Wesen" im Dt. *böse*, im Frz. *méchant*, bei „erkenntnislosen" dagegen im Dt. *schlecht*, im Frz. *mauvais* verwendet werden, während engl. *bad*, ital. *cattivo*, lat. *malus* für beide Kategorien gebraucht werden. Allerdings ist bei einer Untersuchung dieser Wörter ebenso wie bei *senex*, *uetulus*, *uetus* usw. Vorsicht geboten, und vor allem sind falsche Generalisierungen zu vermeiden[9].

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[9] Statt vom Latein im allgemeinen zu sprechen, sollte man sich auf etwa das Plautinische oder das Ciceronische Latein beschränken, den Sprachgebrauch des gewählten Autors aber in allen Einzelheiten darstellen.

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Pisani, Vittore, *Lat. "capillus" e "pullus"* , Indogermanische Forschungen, 84 (1979) p.182

Lat. *capillus* e *pullus*

E' apparso nel vol. 82 (1977) delle 'Indogermanische Forschungen' (pp. 163–190) un articolo di Martti Nyman, The Family Resemblances of Latin *caput* and *capillus* (and what can be gleaned from it), in cui *capillus*, sulla base di un verso di Plauto, Epid. 623, *usque ab unguiculo ad capillum summum*, ritenuto "adaptation of a proverbial theme, cp. Hom. Il. 16, 640 *ἐκ κεφαλῆς . . . ἐς πόδας ἄκρους*", viene ricondotto a *caput* attraverso un ricostruito **capit-lo-*, per il che il Nyman si diffonde sul valore del suffisso *-lo-* e soprattutto sull'abnorme passaggio di *-tl-* a *-ll-*. La poca venerazione che io professo per le "leggi fonetiche" fa sì che io trovi piuttosto superflue le lunghe elucubrazioni del N. intesse a dimostrare il come e il perché di tale abnorme passaggio; per quanto si riferisce al luogo plautino, trovo bensì giusto il vedervi un nesso semantico quale doveva esser presente alla mente del comico latino e, aggiungo io, a quella di molti parlanti latino i quali certo sentivano affini *caput* e *capillus* come ciò avviene a noi moderni per *capo* e *capello*; al qual proposito mi faccio coraggio di riportare una facezia alquanto spinta che circolava intorno al 1920, per cui, poiché i peli del *capo* si chiamano *capelli*, quelli del *pene* dovrebbero dirsi *pennelli*, quelli dell'*ano anelli*, quelli della *mona* (dialett. per 'vulva') *monelli*[1]. Ma ciò non significa che *capillus* debba considerarsi derivato da *caput*; si tratta di uno di quegli incontri fortuiti del tipo di *secūris*, slavo *sekyra*, che non è derivato da *secō*, rispettivamente russo *sekǫ* ecc., come sarebbero portati a ritenere gli etimologi per radici, ma un "Wanderwort" penetrato dal mondo anatolico, dove è presente nell'assiro *šukurru* 'ascia da combattimento', ebraico *šegōr*; cfr. la mia Etimologia (²1967, p. 149; p. 145 dell'edizione tedesca Die Etymologie, München 1975), dove ho raccolto altri esempi di siffatte confluenze che possono alla fine determinare influssi

[1] La conclusione era: e come si chiameranno i peli posti fra ano e mona? La risposta, a sorpresa, era *trabucos* (nome de certi sigari tipo *avana*), il perché lascio che indovini l'arguto lettore.

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formali e semantici dell'una sull'altra parola, rientranti nella cosiddetta etimologia popolare[2].

Leggendo l'articolo del Nyman mi sono imbattuto nell'etimologia di *pullus* che lo studioso finnico richiama a sostegno delle sue elucubrazioni sulla fonetica di *capillus* da **capit-lo-*, e cioè il confronto col sanscrito *putrás* e coll'oscoumbro *puklo-*, chiari continuatori di un ricostruito i.e. **putlo-*; e così mi sono accorto con stupore che tanta brava gente ritiene *pullus* derivato da questo **putlo-* e ha sudato camicie (così Sommer, K. E. p. 86s.; Leumann, Lat. Laut- u. Formenl., 1977, p. 208) per mettere d'accordo tale "etimologia" con le "leggi fonetiche"; ciò senza badare che tanto sudore era invano, in quanto si tratta di due parole per concetti totalmente diversi: *pullus* è il 'piccolo di animali', mentre *putrás* e *puklo-* hanno un valore ben definito giuridicamente, lo stesso di lat. *fīlius* che non è da riunire coll'umbro *feliuf*, il quale del resto significa 'lactantes' e non 'lactentes', cfr. Benveniste, BSL. 45, 1, 82, ma col sinonimo *bilia-* messapico, quindi da **bhīl̥io-*, probabilmente secondo me da **bhūl̥io-* derivato da un **bhūlā* = greco *φῦλή* come *līberī*, **leudheroi*, lo è da **leudho-* 'popolo, nazione' (ant. alto ted. *liut*, lit. *liáudis*) indicando i figli che, nati da cittadini di pieno diritto, si differenziano dai *vernae*. Cfr. per tutto ciò Paideia 14 (1964) 215.

È per ciò che sottoscrivo all'etimologia di *pullus* che dà il Dictionnaire Etymologique de la Langue Latine di Ernout e Meillet, [4]1959, p. 544: "Forme à gémination intensive, en face de got. *fula* 'poulain'. Un rapport avec *puer* n'est pas exclu. Et, d'autre part, le grec a *πῶλος* 'poulain'." Ove va espressamente notato, come è detto ibid., p. 543, che "*puerī* diffère de *līberī*, qui désigne les enfants par rapport aux parents". E così, credo di aver avuto ragione io (Rend. Linc. 6, 5, p. 13) seguendo il Solmsen nel ricondurre *pullus* a **pul-elo-*, checché ne dica il Walde-Hofmann II, p. 386 abbagliato da *pusillus* messo erroneamente in combutta con *putrás* e *puklo-*.

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[2] Per un confronto di *capillus* col sanscr. *kapardas* (da **kapeldos*) cfr. Lingua Posnaniensis 8 (1960) 13.

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reciprocus und Verwandtes

In their reaction against "atomistic philology", structuralists occasionally disparage the historical study of individual words (etymology). But structure need not, and sometimes cannot, always be approached directly. Lost structures, in particular, may be impossible to confront head-on. Their traces are often detectable only through the investigation of isolated survivals. Because of this, the detailed examination of a single, seemingly isolated lexical item can lead to structural conclusions with wide-ranging implications, which may extend even to the level of syntax.

1. The two major Latin etymological dictionaries agree in presenting an analysis of the adjective *reciprocus* which goes in its essentials back to Brugmann's discussion in Rheinisches Museum of 1888. According to this, *reciprocus* is a copulative compound of the adjectival stems **reco-* and **proco-*. These are in turn derived from the preverbs *re-* and *pro-*[1] by means of the suffix **-ko-*.

Although this etymology has become orthodox, it depends on a number of features which are either unparalleled or actually contradicted by the evidence of both Latin itself and of related languages.

First, formally: for the existence of **reco-* outside of the word in question, no evidence from any language has ever been presented. As for **proco-*, not only is the evidence for comparative reconstruction of an IE. **pro-ko-* rather weak[2], but internal

[1] Any serious explanation must start from these two preverbs: cf. e. g. *rursus prorsus reciprocat fluctus feram* (Ennius Scen. 116 Vahlen[2]). I thank W. Cowgill and H. Hoenigswald for comments.

[2] Cf. Pokorny, Idg. Etym. Wb. 815. Walde-Hofmann proposes, despite Brugmann's hesitation (op. cit. 404), that *proceres* replaced a **procí* on the model of *pauperes*. But this is based on the gen. pl. *procum*, which points rather to athematic **prok-* (which does in fact recur in

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analysis shows that the IE. preverbs formed adjectival derivatives not through suffixation in **-ko-*, but rather by composition with the thematised zero-grade of the root **Oek^{w}-* "eye, face"[3]. Latin itself preserves clear examples of this type of derivation: *antīcus* and *antīquus* (both from **anti*), *postīcus* (from **posti*), and also *opācus*[4]. Although both Walde-Hofmann and Ernout-Meillet are aware of this (cf. s. v. *antīquus*), neither one accepts the consequences for the supposed **reco-* and **proco-*. But an updated reformulation of Brugmann's hypothesis should start, according to all clear inner- and extra-Latin parallels, from **re-Ok^{w}-o-pro-Ok^{w}-o-*, and predict **rēciprōcus*[5].

The semantic problems with Brugmann's explanation are just as serious. *reciprocus* means "moving back and forth; alternat-

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Latin: *proximus* etc.). As to *procul*, no convincing argument against the comparison with Grk. τῆλε has ever been made; this is in fact what Ernout-Meillet prefers. And in OCS. *prukŭ*, the semantic development "forward" > "left-over" which this comparison necessitates is not particularly comprehensible. Herodotean πρόκατε "immediately" may be hypostasis or Suffixhäufung. Safest is to say only that Slavic and perhaps Greek (dialects in which the **-ko-* continuant remained productive) independently created such derivatives from their continuants of **pro*. But even admitting **pro-ko-* for Indo-European would not materially affect the following argument.

[3] Thematised zero-grades of roots were frequently compounded with preverbs *et sim.*, cf. *anūpá-*, *dvīpá-* etc. from the root underlying *ăp-*; *upabdá-* from **ped-*; δίφρος from **bher-*, etc. On the compounds in **-Ok^{w}-ó-*, see Hamp, BSL. '73, who however leaves unmentioned the formal and semantic parallels offered by Il. 23. 116 ἄναντα πάραντα κάταντα etc.

In Vedic, *-ka-* derivatives without lengthening (e. g. *ánuka-*) are attested only post-Rig-Vedically, except for *antiká-* (Altind. Gram. II 2, 520). These relatively recent formations demonstrate only the general productivity of *-ka-* in Indic.

[4] On this history of this word, cf. "**apo, opācus, ἤπειρος*" (forthcoming).

[5] Brugmann was himself troubled by the lack of length in the penult, but for the wrong reason "da es sich doch um ein verhältnismäßig junges Wort handelt" (op. cit. 402: this is true neither within Latin nor in any other sense). The reasoning of Osthoff (Morph. Unt. 4, 249ff.) with which Brugmann explained away the Vedic lengthening (cf. fn. 3) is likewise, after 90 years, no longer acceptable, since it takes no account of the priority of the forms with lengthened vowels, much less of laryngeals.

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ing; returning". Clearly, the meaning derives from a conjunction of the preverbial concepts. Brugmann accounted for this additive component by calling his **reco-proco-* an adjectival dvandva. But, as Walde-Hofmann at least admits, this utterly unparalleled within Latin: no other instance of such a compound occurs. Even nominal dvandvas are notoriously rare in Latin (Leumann, Lat. Gr.[2] 403).

2. Brugmann began his article by rejecting the earlier etymology of August Pott (Etymologische Forschungen I[2], 1895, 190, 550), who had suggested that *reciprocus* was a hypostasis derived from a phrase **reque proque* "both backwards and forwards". To this, Brugmann's objection was phonologic: "und warum erscheint nirgends ... **requiprocus*?" (op. cit. 402). This seeming phonologic irregularity caused Brugmann to reject Pott's explanation outright, and to look for a derivation from a medial plain velar. But before we reject it too, let us consider any advantages it might have, starting within Latin.

First, of course, the problem of expected but missing vowel-length disappears, or rather, never comes up, since, unlike before **-Okʷ-*, we expect no lengthening before simple **-kʷ-*.

Next, the semantic problem also disappears, since the double *-que*[6] accounts perfectly for the copulative component of the meaning.

Finally, although all the etymological dictionaries suppress this fact, Pott had presented a perfect inner-Latin parallel for his **reque proque*: the phrase *susque deque* (used alone or with *habeo, esse*, or *fero*), glossed "*animo aequo esse*; ἀδιαφορεῖν" by Gellius (16.9)[7]. Beyond the absolute formal parallelism, we may note that the semantic relation between the preverbs is also identical: both phrases represent the conjunction[8] of diametrically opposed spatial movements.

[6] On the priority of the single versus the double use of **-kʷe*, see "The original syntax of conjunctive **-kʷe*" (to appear in Die Sprache).

[7] Pott's own semantic interpretation "weder achten, mit Ehrfurcht daran hinauf (*sus-spicere*) sehen, noch mißachten (*dē-spicere*)" (op. cit. 550) is clever, but hard to reconcile with the known semantic force of **-kʷe*.

[8] Bernert's assertion that the single occurrence of *susque et dēque* (Lucil. 111, vs. 5 attestations in the usual form) shows that the double *-que*

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There exists in fact another *formally* comparable expression in Latin: *usque quaque* "everywhere, continuously". Here the original semantics are not clear, but that the elements are old is suggested by the isolation within Latin of *usque* (< **uts* = **ut*, cf. *absque* (and ἄψ)[9] < **aps* = **ap*), and by the archaic (within Latin) double *-que*.

Thus, even confining ourselves to Latin, the various advantages of the earlier etymology outweigh the single disadvantage. But even stronger support comes from outside Latin.

3. Although Wackernagel had suggested the relevance of Vedic phrases of the type *á ca párā ca, úc cáva ca*, etc. to the history of *reciprocus* in 1905 (Altindische Grammatik II 1, 172), Walde-Hofmann made absolutely no mention of this fact. Ernout-Meillet simply registered it, without drawing conclusions. Only M. Leumann took its supportive value for Pott's etymology seriously enough to present both theories, without deciding between them (Lat. Gr.[2] 340).

The Altindische Grammatik has not yet reached the point of discussing preverbs. Nor are the phrases of the form P_1 *ca* P_2 *ca* recoverable from the Vedic Word Concordance. It will thus be worthwhile to present in full the Rig-Vedic instances of the construction. They are, in order of frequency:

sáṃ ca ví ca	5. 78. 6; 6. 75. 11; 10. 158. 4[10]
á ca párā ca	1. 164. 31 = 10. 177. 3; 10. 17. 6
úc cáva ca	1. 164. 51[10]; 8. 19. 23[10a]
sáṃ ca práti ca	2. 1. 15
úpa ca sáṃ ca	9. 95. 3[11]

"keine Kopula sein kann" (Glotta 1940, 81) is due only to his own preconceptions. Cf. fnn. 6 and 7 of the work cited in fn. 6.

[9] For collocations of ἄψ with ἀπό in early Greek, and related constructions, see "Preverb repetition" (MSS. 38, 1979), section 12f.

[10] In these two passages, the conjoined preverbs are separated by the insertion of a single word: the verb in 1, 164, 51b *úc caíty áva cáhabhiḥ* (cf. the position of *yánti* in the passage of fn. 11); an adjective in 10. 158. 4c *sáṃ cedáṃ ví ca paśyema* (cf. Lucil. 110 *susque omnia deque fuerunt*).

[10a] On the hiatus in this passage, cf. AIG. I, Intr. générale 5.

[11] The second couplet runs:

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In addition, the phrase *vi ca pra ca* occurs in the MS. (4, 13, 2 = 200, 8) and parallels.

Not only are these Vedic phrases the exact formal equivalents of *susque deque*, *usque quaque*, and **reque proque*, but, as was noticed already by Böhtlingk and Roth (PW. s. v. v.), Vedic also presents thematic adjectives hypostatically derived from some of these[12] phrases:

sañcavica-	AVP.[13]
ācaparācá-	Kāṭh., Kap., PB.
uccāvacá-	TS.
vicapraca-	Gaṇar.

The agreement between Latin and Vedic is not only structural, but extends even to lexical details. I. e. except for the second preverb[14],

úc cáva ca = *usque quaque.*

Similarly, except for the first preverb[15],

ví ca prá ca = **reque proque*, and
vicapraca- = *reciprocus.*

Note the differing phonologic outcomes of the old labiovelars in the Vedic hypostases *uccāvacá-* etc. on the one hand, and in

namasyántīr úpa ca yánti sáṃ ca-
á ca viśanty uśatír uśántam

(to Soma) "Obeisant (fem. pl.), they approach; desirous (fem. pl.), they enter the desirous one (masc.)." The *pāda*-division speaks for *úpa ca sáṃ ca* (with *yánti*); another possibility is *sáṃ ca á ca viśanti*, with enjambement. Neither interpretation is semantically parallel to the other examples, since the preverbs are not opposed in meaning. Furthermore (cf. section 4), both combinations do occur asyndetically conjoined in the Rig-Veda (*úpa sám* in 4. 58. 1b, not in Grassmann; *á sám* in 10. 18. 7b, wrong in Grassmann). We will therefore leave this unclear passage out of account in the following.

[12] In addition, there occur *ācopacá-* (Kāṭh.), *niścapraca-* (Gaṇap.), and *uccanīca-* (epic, classical), all from unattested underlying phrases.

[13] K. Hoffmann, Aufsätze zur Indo-Iranistik I 232–33.

[14] Note the variation of the second preverb within Sanskrit, between Vedic *uccāvacá-* and classical *uccanīca-*.

[15] An innovation on both sides. Latin *re-* is etymologically isolated in Indo-European. Skr. *ví*, though inherited, cannot be old in this word, since *ví* and *prá* are not opposites. Cf. also section 4.

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the old compounds *ápāka-* etc. on the other. Brugmann's updated followers must somehow derive both from **-k(ʷ)o-*. But for us, all is in order, since we derive the first from **-kʷe-*, and the second from **-Okʷ-o-*[16]. This was not seen by Brugmann's adherents Wackernagel (AIG. II 1, 124) and Debrunner (AIG. II 2, 520, 545–6).

The semantic agreements are just as striking as are the formal ones. The crucial fact is that in all the clear Rig-Vedic expressions, the preverbs are diametrically opposed in meaning[17], just as in Latin.

The extreme antiquity of preverb conjunction by double *ca*[18] becomes even more certain when we notice that Behaghel's law is observed in all clear cases; and that the same construction recurs in Avestan, partially with the same lexical items[19]. The combination of structural, lexical, and semantic agreements makes these comparisons particularly compelling.

4. In Vedic, when two preverbs modify the same verb, the usual methods of conjoining them are asyndeton (if the preverbs

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[16] The presence of original **-kʷep-* might also help explain the lack of a variant **recuprocus*. The phonologically regular outcome, whether from **-kʷepro-* or **-kopro-*, would be **receprocus*. Cowgill asks (per litt.) "why not **recupricus*?", and suggests "that at the time of Latin vowel weakening, perhaps there was still a strong stress on the second element, etwa ['rekʷe^prokʷos], such that the **o* of the second element developed as in initial syllable, and the **e* at the end of the first developed as in medial open syllable". The influence of free *pro* must also be considered.

[17] *sáṃ ca práti ca* denotes agreement/unity vs. opposition.

[18] In 6. 51. 2b *sanutár á ca*, semantically opposed adverbs are conjoined by a single *ca* (though the text may be corrupt: cf. Oldenberg, Noten ad loc.), and Behaghel's law is broken.

J. Klein, in conversation, points out a case of seeming preverb conjunction by *u* in 10. 25. 4a *sám u prá yanti dhītáyaḥ*. This is unrelated to our phenomenon, since asyndetic *sám prá* with verbal roots is common from the RV. on.

[19] E. g. *paračā (mraočąs) aorāčā* (Y. 53. 7); *aorāča parača tarasča* (F. 3a); *āča pairiča* (Yt. 13. 2; V. 5. 25); and *āča parača*, which, though inherited from Indo-Iranian, occurs only in Young Avestan (Yt. 8. 54, 14. 4; N. 4). Its Vedic counterpart *á ca párā ca* likewise occurs only in hymns assigned to the "popular" stage by Arnold.

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are orthotone) or composition (if they are enclitic)[20]. If we search the Rig-Veda for such conjunction of the pairs of preverbs we have found conjoined by double *ca*, we do not come across a single example, in either order[21]. The scattered instances from later Vedic texts demonstrate only the subsequent breakdown of an inherited system. Conversely, the intactness of the system in the Rig-Veda is demonstrated by the absence of **ví ca prá ca*, which does not fulfill the semantic requirements (fn. 15); furthermore, the asyndetic combination *ví prá* is quite common in the Rig-Veda (e. g. with the root *i*).

The same situation is recognisable in Latin (where, to be sure, asyndetic conjunction of preverbs has been lost[22], leaving only composition as a method of preverb conjunction). It is hardly due to chance that no early texts show any clear cases of compounds verbs involving both *sub* and *de*, or *re* and *pro*[23].

These distributional agreements, then, further demonstrate that **reque proque* fits perfectly into a system of which we find traces in Latin itself, and in earliest Indic. Pott's guess seems to correspond to historical reality.

But there is a more wide-ranging conclusion to be drawn: that a rigorously definable syntactic relation existed between conjunction of preverbs by asyndeton/composition, and conjunction by double *-k^we. The former was the normal method for most combinations of preverbs, but if the preverbs were of directly opposing meanings, the use of double *-k^we was obligatory.

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[20] Cf. Delbrück, Altindische Syntax 434ff. In the case of indeclinables, only a difference in accentuation distinguishes asyndeton from composition, since the word-boundary is "external" in both eases. This subvariety of copulative compounds may thus be traced to underlying asyndeta (cf. Delbrück, Vgl. Synt. III 192).

[21] Passages such as 1. 123. 12c *párā ca yánti púnar á ca yanti* are not counterexamples, since they involve *ca . . . ca* used to conjoin sentences, not preverbs. Cf. fn. 11.

[22] I. e. "tmesis" seems never to involve doubly-compounded verbs (e. g. *dī-ui-dere*) in Latin.

[23] *repro-* early only in *repromitto*, twice in Plautus, clearly a decompound of *promitto* (cf. Curc. 661 *promittimus*, 667 *repromisit*); *prore-* never occurs; *subdē-* only late, and mostly in decompounds; *dēsub-* only late.

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We have thus unexpectedly arrived at a conclusion similar to that of J. Gonda concerning the "original value" of conjunctive **-k^we*: that it indicated a "complementary unity"[24], and that it was thus not the equivalent of, or in free variation with, asyndeton, as Delbrück had thought[25]. Although our analysis is based on a more circumscribed set of material (the preverbs)[26] than was Gonda's, it can for this reason be exhaustive. Thus we have noticed something that Gonda did not[27]: although the two constructions, $P_1\ P_2$ and P_1 *k^we* P_2 *k^we*, are not equivalent, it would be just as incorrect to say they contrast. In fact, they stand in complementary distribution. Asyndeton/composition and double **-k^we* are thus (at least as applied to preverbs) alloconstructions of the single IE. process of conjunction.

5. Although it is somewhat surprising to find no reference to these wide-ranging and systematic agreements in the handbooks, what is far more disturbing is that neither the Latin nor the Sanskrit grammars mention that exactly the same construction occurs in Greek. Nor does Schwyzer, though he does discuss the phrase περί τ' ἀμφί τε (Gr. Gr. II 421, 430), mention the Latin or Indo-Iranian equivalents.

Now, although the formal adherence of the Greek phrase to the formula P_1 *k^we* P_2 *k^we* is perfect[28], the agreement does not seem to be total[29]. First, the meanings are not opposed: ἀμφί and περί are near-synonyms, overlapping in many uses. And although, as expected, there exist no verbs compounded with περιαμφι-, the two preverbs do co-occur in early Greek in the

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[24] Mnemosyne 1954, 189, 192; Vāk 1957, 4.

[25] Vgl. Syntax III 190 (but cf. Altind. Synt. 59). For more on the interrelationship of the various IE. conjunction processes, see the article cited in fn. 6, section 15.

[26] Although the conclusions probably hold for other lexical classes as well, cf. 1. 96. 7a *nú ca purā́ ca.*

[27] Although he did compare *usque quaque, susque deque,* and *ā́ ca párā ca* (Mnemosyne 1954, 291; Vāk 1957, 12), he drew no conclusions.

[28] περί τ' ἀμφί τε is in accordance with Behaghel's law generalised to cover metrical weights of words (rather than simply number of syllables).

[29] That Greek preserves no hypostatic adjective from περί τ' ἀμφί τε is unfortunate, but irrelevant. On the other hand, τὰ ἔγκατα "innards" may well be a hypostasis from asyndetically conjoined preverbs.

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reverse order, both compounded (ἀμφι περι-) and asyndetically (ἀμφὶ περί).

6. Beginning with the latter problem first, we note that the distribution of the two phrases is very different. περίτ' ἀμφί τε occurs only four times, and only in hexameter verse[30], whereas ἀμφιπερι-/ἀμφὶ περί occurs ten times in hexameter verse[31], and also survives into later prose. Even within the hexameter, περί τ' ἀμφί τε is more localised than ἀμφιπερι-[32]. These facts suggest that περί τ' ἀμφί τε is an archaism, whereas ἀμφιπερι- is more recent. In accord with this is the fact that ἀμφιπερι- does not conform to our modified (fn. 28) Behaghel[33], while περί τ' ἀμφί τε does. Nor can ἀμφιπερι- be considered in isolation from seven passages involving looser, discontinuous asyndetic collocations of ἀμφί and περί, e. g. Il. 17.4:

ἀμφὶ δ' ἄρ' αὐτῷ βαῖν' ὥς τις περὶ πόρτακι μήτηρ

Il. 18. 564:

ἀμφὶ δὲ κυανέην κάπετον, περὶ δ' ἕρκος ἔλασσε

and Theog. 576–9[34]:

ἀμφὶ δέ οἱ στεφάνους, νεοθηλέος ἄνθεα ποίης,
ἱμερτοὺς παρέθηκε καρήατι Παλλὰς Ἀθήνη·
ἀμφὶ δέ οἱ στεφάνην χρυσέην κεφαλῆφιν ἔθηκε,
τὴν αὐτὸς ποίησε περικλυτὸς Ἀμφιγυήεις.

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[30] Il. 17. 760; h. Dem. 276; note the pleonasm in Theog. 848:
θυΐε δ' ἄρ' ἀμφ' ἀκτὰς περί τ' ἀμφί τε κύματα μακρὰ
and in Merkelbach-West's restoration of Hes. frag. 150. 28.

[31] ἀμφιπερι- (compounded): Il. 8.348; Od. 8. 175; h. Aphr. 271; ἀμφὶ περί (asyndetic, either free adverb or adnominal): Il. 2. 305, 21. 10, 23. 191; Od. 11. 609; Theog. 3 variant, 693–94; Hes. frag. 26. 19.

[32] περί τ' ἀμφί τε occurs only following the penthemimeral (thrice) or hepthemimeral (once) caesura; ἀμφιπερι can begin the first (twice), second (five times), or third (thrice) foot.

[33] The avoidance of hiatus was evidently more important at this time than adherence to Behaghel's law.

[34] This passage includes two recensions of the same scene. Note especially the pun in the noun-epithet combination at the end of 579. The other similar passages are: Il. 15. 647–8, 23. 561–2; Od. 7. 113 (with ἀμφοτέρωθεν), and Theog. 726–7. Cf. also the pleonasms mentioned in fn. 30.

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The conclusion to be drawn is that ἀμφιπερι- forms part of a wide system of word play involving asyndetic co-occurrence of these two preverbs, whereas περί τ' ἀμφί τε is isolated, as archaisms tend to be.

Because of the synchronic near-synonymity of the two preverbs, ἀμφιπερι- is functionally close to the inherited intensive-iterative preverb repetition[35], as in Hom. προπροκυλινδόμενος, Ved. *prápra* etc.[36]. We may conclude that ἀμφιπερι- was created to serve as a metrical variant of περί τ' ἀμφί τε after the semantic merger of the preverbs[37].

Thus, chronology and distribution of attestations suggest that the frequent and integrated ἀμφιπερι-/ἀμφὶ περί is relatively more recent than the rare, isolated περί τ' ἀμφί τε. It need not stand in the way of the hypothesis that originally, preverbs capable of being conjoined by double *-k^we were never conjoined asyndetically.

7. Etymologically, it has long been known that ἀμφί and περί were not always near-synonyms. ἀμφί, a locative singular, is related to ἄμφω (cf. Latin *am(b)*- and *ambō*; similarly still NHG. *bei* and *beide*)[38]: it must have meant "on both sides". The semantic change to "around" occurred identically in Greek (where the original meaning was later expressed by extensions like ἀμφοτέρωθεν etc.) and Latin.

περί, on the other hand, is the locative singular of a root-noun from the root **per* "penetrate"[39]; the original meaning was "during penetration, through". The semantic shift "through" > "around" was this time shared by Greek and Sanskrit, whereas Latin *per* preserved the original meaning.

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[35] As opposed to functional (conjunctive) and pleonastic preverb repetition, for which see the article mentioned in fn. 9.

[36] I know of no evidence for a construction **prok^{w}e prok^{w}e*, postulated by Walde-Pokorny to account for Latin *prope*.

[37] Wackernagel Vorl. über Synt. II 159–60 (with Nachtrag) presents examples of replacement of these preverbs by each other. Latin *dī-ui-dere* is another instance of conjunction of synonymous preverbs.

[38] Skr. *abhí* : *ubhá* is comparable on neither side. Cf. J. Jasanoff, Indo-European Studies III 537ff. = BSL. 71 (1976) 125ff.

[39] On this root and its derivatives, cf. K. Hoffmann, Aufsätze I 331ff.; A. Bergren, Etymology and Usage of πεῖραρ (New York 1975).

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Thus the meaning "around" was secondary for both preverbs, and the phrase περί τ' ἀμφί τε originally meant "both through and on both sides": that is, it did indeed involve preverbs of opposing meanings[40]. But later semantic change obscured this fact, and led to reinterpretation of the phrase as an intensive formation.

8. Thus, distribution and etymology show that the Greek evidence fits perfectly with that of Latin and Indo-Iranian, in supporting our reconstruction of an Indo-European syntactic suppletion: $P_1\ P_2$ and $P_1\ k^we\ P_2\ k^we$ stood in complementary distribution. Structural correspondences such as *á ca párā ca*: περί τ' ἀμφί τε : *susque deque*, with preverbs of opposing meanings conjoined by double *-*k*^{w}*e*, and the corresponding lack of early examples of these pairs of preverbs conjoined asyndetically or by composition, render this certain. This conclusion will stand regardless of what we think of *reciprocus*.

But the perfect structural and semantic conformity of Pott's **reque proque* to this type, the presence of thematically derived hypostases from these phrases in Vedic, and the details of that dialect's distribution of *-k-/-c-* all strongly support the derivation of *reciprocus* from that phrase, against Brugmann's derivation from the morphologically unparalleled **reco-proco-*.

Only one question still remains: why not **requiprocus*? This question is perhaps wrongly formulated. Whether *-ci-* is an "exceptional" treatment of *-*k*^{w}*e-* in Latin depends on how we describe the environment. Brugmann treated sounds in isolation. This labiovelar, however, did not occur in isolation, but rather in a complex sequence of labials and labiovelars, of the sort which gave rise to another well-known "irregularity" in Latin: the *quinque* rule[41] (which incidentally did not apply to our sequence: no **recicrocus*). Thus, until faced with a counterexample (i. e. a case of preservation of the first labiovelar in

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[40] Schwyzer, Griech. Gram. II 428 is wrong in calling περί τ' ἀμφί τε "nicht einfach hypercharakterisierend, sondern rektifizierend ... 'auf beiden Seiten, [vielmehr] ringsum'". So too Wackernagel, Vorl. über Synt. II 159 and 232.

[41] Cf. Hoenigswald, JIES. 1973, 325 fn. 6; Polomé in Pratidānam (Kuiper Festschrift).

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a monomorphemic[42] sequence *k^w . . . p . . . k^w), it remains simplest to assume that Latin *c . . . p . . . qu* is nothing but the normal phonological outcome of this sequence. We are dealing with a dissimilatory[43] loss of labiality, which is actually an extremely widespread phenomenon in Latin, although it is not treated as such in the handbooks[44].

Korr.-Note:

C. Watkins draws my attention to the Hittite asyndetic antonymic collocation *katta šer*. We are faced with the frequent interpretative problem: is this archaic or innovatory? Only exhaustive investigation can tell; in the meantime, note the loss of conjunctive *-k^we in Hittite.

On the supposed *$prok^we$ $prok^we$, see "Ennian *atque atque*; *prope*" (forthcoming in Glotta).

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[42] Thus excluding the constantly re-made phrases like *quid pro quō*.

[43] This does not mean "sporadic", since there are no counterexamples. Cf. Hoenigswald, Phonetica 1964, especially pp. 210ff. The assimilatory *gain* of velarity in *quinque* etc. is phonetically quite dissimilar.

[44] Well-known instances include: *$bhw > bh$ ($> f$); *$k^wo > co$; *$swo > so$; *$woy- > uī-$ (not "*uū-*"); also in isolated words, like *tenebra* (cf. Skr. *tamisrā*), *līber* (not "*lūber*"); victory of *libet* over *lubet*, *-imus* over *-umus*; late *cinque* from *quinque*. These are all to be distinguished from non-dissimilatory loss of labiality, partly Latin (*insece* < *inseque*), and partly dialectal IE. (*sex*, *caseus*, etc.).

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Albanian *vjerr* and IE. In-Law Terms

The etymology of Alb. *vjerr* 'father-in-law' is more problematic than is generally realized. A relation with PIE. */suéḱuros/ 'HuFa'[1] has almost universally been accepted[2], but rarely have the details of this relationship been examined. The form /vjeř/ that underlies the standard orthography corresponds to /vjęř/ in Borgo Erizzo and /vjer/ in Berat and signifies both 'father-' and 'mother-in-law'. More conservative dialects have disyllabic forms such as /vjehəř/ in Dushmani and /vjehər/ in Salamis. These forms point to Proto-Albanian */vjehəř/ from an earlier **/wehVrV-/. A distinctive form for 'mother-in-law', *vjerrë*, occurs in some dialects; but this is clearly formed from the masculine term and is not a reflex of PIE. */sueḱrúHs/; therefore, the nature of the derivation of *vjerr* from */suéḱuros/ will be the focus of this article.

Meyer (1891: 475) derived *vjehërr* from */sueḱros/ by comparing OCS. *svekrŭ* rather than OIn. *śváśura-*, Lith. *šẽšuras*, or Gk. ἑκυρός. This equation he regarded as evidence of an Albanian-Slavic lexical alignment without noting that Lat. *socer* and Welsh *chwegr* could also reflect */sueḱros/. Pedersen, who had demonstrated that intervocalic */ḱ/ became /θ/ in Albanian (cf. *bathë* 'bean', Gk. φακός), accepted this equation and stated that internal */ḱr/ became /hr/ (1900: 339). Both Wescott (1948: 110) and Mann (1950: 382) derived /h/ directly from intervocalic */ḱ/. No explicit reasoning for this was stated by either but it is likely that this error was made because each knew that the

[1] With the exception of Sb for 'sibb', the core abbreviations for kinship terms used in this paper are the first two letters of the English word. Fa stands for 'father', Mo 'mother', Br 'brother', Si 'sister', So 'son', Da 'daughter', Hu 'husband', and Wi 'wife'; a preceding *y* ~ *o* signifies 'younger ~ older'; a following *ms* ~ *ws* designates 'man speaking ~ woman speaking'.

[2] Tagliavini (1937: 282) gives the history of the etymology up to his time and notes the one exception to the view that *vjerr* reflects */suéḱuros/ in some form — Weigand's abortive relation to Croatian *djever*.

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sequence */k̂r/ became Alb. /kr/ (Wescott [111]; Mann [1952: 33]; this point has been made more explicitly by Hamp [1960]).

Avoiding such difficulties, Jokl had already posited an assimilated intermediary */suesuros/ (1923: 46; 1963: 127) correctly noting that intervocalic */s/ gave the desired /h/ (cf. *kohë* 'time', OCS. *časŭ*). This explanation is unlikely, though, for two reasons. Under normal conditions prevocalic */su-/ became Alb. /d/ as in *dergjem* 'I am ill' < */suorgh-/ (cf. OE. *sorg*), *dirsë* 'sweat' < */suid-r-/ (cf. Gk. ἱδρώς). Also, Jokl assumed that Alb. /θ/ had already arisen from */k̂/ and that it was this /θ/ that was assimilated to /s/. This he justified by noting the converse development in Alb. *thaj*, Salamis dialect /θañ/ 'I dry up' < PIE. */sausnioA$_1$/ and Alb. *thi* 'sow' < PIE. */suus/. These show dissimilation of the sequence */s . . s/ to */θ . . . s/ prior to the change of */s/ to /ĝ/ before a stressed vowel. It would be very strange if in */suék̂uros/ Albanian generated the very sequence removed by dissimilation elsewhere. Accordingly, Jokl's explanation also fails.

*/suék̂uros/ is among the most deformed of all Indo-European kinship terms. The proto-form is reflected without deformation only in Germanic (OE. *swēor*, *swehor-* in the Epinal glossary) and Iranian (Av. *xᵛasura-*). Even Gk. ἑκυρός has the accent of the feminine */sueḱrúHs/. A deformation */sueḱros/ reproducing the consonant cluster of the feminine lexeme is reflected in Italic (Lat. *socer*), Celtic (Welsh *chwegr*), and Slavic (OCS. *svekrŭ*). Baltic (Lith. *šẽšuras*) and Indic (OIn. *śvāśura-*) show regressive assimilation of */s/ to the sibilant reflex of */k̂/. Armenian *skesr-ayr* (literally 'in-law-man') is based upon *skesur* 'mother-in-law' which probably reflects a similar deformation judging from the reflex of */su-/ in *kʿoyr* 'sister' < */suesoor/. Perhaps *skesur*, modelled on the masculine[3], was regressively assimilated to */k̂uek̂ur-eA$_2$/ or its reflex[4].

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[3] This would match the remodelling in Gk. ἑκυρά and Alb. *vjerrë*; there are other lexical correspondences between these three.

[4] See Winter, (1966: 204–05) for the possible Armenian reflex of */k̂u-/ as *sk-*. Because Iranian and Slavic do not participate in this regressive assimilation of */s/, it is probable that Baltic, Indic and Armenian are mutually independent in regard to this change.

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I suggest that PAlb. */vjehəř/, earlier **/wehuras/, is also a deformation, the metathesis of PIE. */suéḱuros/ to */uésḱuros/. The retention of */ué-/ as Alb. /vje-/ seen in *vjedh* 'I steal' < PIE. */uéĝhoA$_1$/ and the development of */sḱ/ to /h/ as in *ngroh* 'I warm' < PIE. */en-g^{w}hr-eesḱoA$_1$/, *hie* 'shade' < */sḱieA$_2$/ account quite well for */vjehəř/ from */uésḱuros/. In 1972 Hamp (221–25) convincingly ascribed the divergent evolution of PIE. */e/ into Alb. /ie ~ je/ and /ja/ to the difference between open and closed syllables. The change of */sḱ/ to /h/ must have occurred before this difference was manifested, for */sḱ/ acts as a single consonant in this case. Metathesis, upon which this explanation rests, is a rather common feature of Albanian linguistic history and affects even Latin loans such as *shtëpí* 'house' < *hospitíum*. Metatheses in the inherited IE. stratum are more difficult to document. The difference between Proto-Tosk */vée/ and Proto-Gheg */vóe/ 'egg' point to an earlier Proto-Albanian */vé-o/ < PIE. */o̯uiom/ progressively assimilated in Tosk but metathesized in Gheg.

Having observed that in eight of the ten branches in which it occurs */suéḱuros/ has been deformed, some account of this phenomenon may be attempted. Many kinship systems contain terms that refer to the speaker's sex. Athapaskan, for example, has several such terms: */-ḱedl-əʔ/ 'yBr,ws' (H 34); */-ḱəx̂-ə/ 'yBr,ms' (H 35); */-yaž-ə/ 'So,ws' (H 50); */-yeʔ/ 'So, ms' (H 48); */-ts'eʔ/ 'Da, ws' (H 52); */-tseʔ/ 'Da, ms' (H 51)[5]. In Proto-Indo-European such terms are exclusively confined to in-laws: */suéḱuro-/ 'HuFa'; */sueḱrúH-/ 'HuMo'; */dai-Huér-/ 'HuBr'; */ĝlo̯$_1$uos-/ 'HuSi'; and */ienÁ$_1$ter-/ 'HuBrWi'[6]. If Indo-European marriages were exogamous with

[5] Athapaskan data are principally from Hoijer (1956), signified by the item number preceded by H, and Dyen and Aberle (1974), but I have modernized and occasionally corrected the reconstruction. Krauss (1964) believes that Proto-Athapaskan did not possess voiced spirants in its earliest stages; thus my */ž, y, γ/ would be earlier */š, x̂, x/. For the meanings of these and the following IE. kinship terms, see the abbreviations in note one.

[6] Terms for spouse and the man's in-laws should also be of this type; but their reconstruction is much less secure than the terms for the woman's in-laws.

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patrilocal residence as Friedrich posits (1966: 20), then there would be an increased chance that wives spoke dialects or even languages different from those of their husbands. In such a case it is a reasonable expectation that words used only by women would receive less standardizing influence and be more prone to irregular development.

Unfortunately, it is hard to test this hypothesis within Indo-European alone for the other in-law terms are poorly attested. */daiHuér-/ is regular or nearly so in almost all attested branches: OE. *tācor*, Gk. δαήρ (to be read δαιϝέρ- in Il. 24. 769), OIn. *devár-*, Arm. *taygr*, Lith. *dieverìs*, OCS. *děverĭ*; only Lat. *lēvir* is notably aberrant[7]. Reflexes of */ĝlo̯uos-/ — Gk. γαλόως, Lat. *glōs*, OCS. *zŭlŭva* — seem tolerably regular except Arm. *tal* with *t* instead of expected *c* (see Winter: 204 however). More irregular are the reflexes of */ienÁ$_1$ter-/; Arm. *ner* and Lith. *jéntė* reflect a full-grade as do Gk. εἰνάτηρ (dialectic ἐνάτηρ). Lat. *ianatrīcēs* with its curiously remodelled ending and the inadmissible initial /ye-/ deformed to /ya-/ belongs here. OCS. *jętry* shows a u-stem ending like *svekry* 'mother-in-law' and could be a full or zero-grade. OIn. *yātar-* is definitely a zero-grade. To test the validity of attributing the extensive deformation of */suéḱuro-/ to its use by women only we may have to resort to cross-cultural analogies with other patrilineal exogamous kinship systems. However, the difference in Indo-European between a woman's in-law terms and those of a man is reasonably clear from the etymological data. Despite deformation, the woman's terms point to single reconstructable etyma while the man's terms often show conflicting bases. Thus 'WiBr' is represented by OIn. *syālá-*, OCS. *šurĭ* possibly from PIE. */sioró-/ but Gk. εἰλίονες, ON. *svilar* are from possible */suil-/ which is different from Lith. *svaínis*. 'SiHu ~ DaHu' is represented by Lat. *gener* < */ĝener-/, Alb. *dhëndërr*, but OIn. *jā-mātar-* Av. *zāmātar-* point to Proto-Indo-Iranian */dzaamaatar-/,

[7] The change of /d/ to /l/ is of course a wide-spread Mediterranean feature, still the monophthongization of */ai/ to /e/ and the occasional raising of tense long */e/ to /i/ matches known Umbrian features, whatever the legend of the Rape of the Sabine Women may lead us to suppose.

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< ? PIE. */ĝomVVter-/ neither of which match OCS. *zęti* or Gk. γαμβρός. This multiplicity of bases for a man's in-laws occurs with referents outside the essential patriliny; the woman's in-laws were within the patriliny and designated by a single base; however, because women were often of other dialect or language groups, these single bases were subjected to unusual phonological distortion.

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Albanian *drudhe* 'piece, crumb'

In TPS. 1974 (1975) 128–45 I have established the Indo-European etymon **dhrus-*, which has subsisted mainly in old participial formations or nominalizations thereof, and particularly in collective plurals. M. Camaj[1], drawing upon E. Çabej, has given as a reconstruction for the feminine *drudhe* a pre-form **dru-d-* in his section (§ 88, p. 121) headed "*-t/d*-Erweiterung". Both base and termination are thus lacking in explanation. Camaj further rejects (122) the derivation of *pidh* def. *-i* 'cunnus' from **pīzdā* on the grounds that **zd* gave Alb. *zhd*. It is true that later *zd* gave *zhd* (*gozhdë* 'nail' < Slav. *g(v)ozdь*), but there can scarcely be doubt that IE. **sd(h)* [zd(h)] gave Alb. *dh* [ð]; cf. Alb. *ethe* 'fever'[2]. On *pidh* see my article IJSLP. 11 (1968) 25–6[3]. Camaj analyzes (ibid.) *herdhe* as *her-dh-e*, but that is impossible since we must have here **horǵhi-*[4].

It seems obvious, then, that *drudhe* should be reconstructed as **druzdi̯ā* < **dhrus-d-i̯ā*, precisely equivalent to Celtic **d(h)rus-*

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[1] Albanische Wortbildung, Wiesbaden 1966.

[2] I regard *ethe* as an old collective in *-e* < **-i̯ā*, and the *th* as a devoiced **ð* in earlier final position; cf. *djathë* 'cheese'. Thus **eð* < **euzdo-* = **eus-d-* : Lat. *ūrō*, Gk. εὕω. I find neither of the hypotheses reported by Camaj, op. cit. 68 fn. 318, persuasive. On the *d*-suffix see below. On **zd* see Jokl, LKU. (1923) 99, 204.

[3] As I remarked in an oral paper at the 1966 Philadelphia Indo-European and Indo-Europeans conference, it seems unrealistic to divorce *bythë* 'buttocks' formally from *pidh*. The *th* ϑ and *-ë* are to be accounted for, once again, as in *djathë*. The *y* adjacent to a labial may be derived from *i* as in *krymb* 'worm'. We thus arrive at **bið-*, and the **-ð-* would be **-zd-* < **-sd-*, zerograde of **sed-* 'sit', as in *pidh*. I no longer associate *bi-* with Slavic *obь-*, since I believe I have shown Pokorny's **(o)bhi* (*IEW*. 287) not to exist; see Ériu 28 (1977) 145 fn. 1. The most likely source for *bi-* seems to be *mbi-*, with the *m-* removed as if it were the familiar nasal prefix (cf. *barsë* ~ *mbarsë* 'pregnant', etc.). On Alb. *mbë, mbi* see Eriu 24 (1973) 164.

[4] Glotta 54 (1976) 262.

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l-i̯ā and **d(h)rus-t-i̯ā*. The adjectival **-d-* element here may well be that which we see in Lat. *forda*, *crūdus* (both active and passive in sense), *tepidus* etc., and in Celtic *bardos*[5].

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[5] E. P. Hamp, The semantics of poetry in Early Celtic, Papers from the 13th Regional Meeting of the Chicago Linguistic Society (1977) 147–51, esp. 151. — I have since dealt at some length with this ignored IE. participial **-do-* (Festschrift Hansjakob Seiler), which seems properly to have formed verbal adjectives from intransitive and perhaps presential stems. I have attributed to this formation other important Albanian traces: *burdhë* 'sack', *hurdhë* 'sour milk or soft cheese', *zgurdhë* 'intestine'. We see then that from the point of view of verbal diathesis and transitivity **dhrus-d-i̯ā* is properly 'what crumbles' rather than 'what is broken', and is closer in formation to **d(h)rus-l-i̯ā* than to **d(h)rus-t-i̯ā*. Perfectly consonant with these findings is *ethe* < **eus-do(+i̯ā)*, which must be 'what burns', and not 'what is/has been burnt'.

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Western Indo-European Notes

6. Welsh *echen* 'need'

Welsh *echen* (fem.), glossed by Geiriadur Prifysgol Cymru (part XIX, 1965, p. 1160c) 'angen, eisiau, colled; need, want, loss', is sparsely attested but clear; it is known from Gwalchmai in the 15th century. Better representation is found for *echenog* 'needy; needy person, beggar'; we have *echenawc* from ca. 1300. The variants *achenog* (*achenawc* 13th cent.), *achanog* (*achanavc* 13th cent.) and *ychenog* are also known. In earlier Welsh morphophonemic alternation, as well as orthographic variation, be)tween *e* and *y* are fairly common; thus the appearance of *y*- in this unstressed syllable is not surprising. Additionally, the influence of *ychydig* 'a little' may perhaps be seen here. The variant *achenog* (and *achanavc* from the last) can easily be understood when we recall that the homonym *echen* 'lineage, stock, source, nature' has a variant *achen* also, which has been influenced by the shorter, and separate, lexeme *ach* 'degree of consanguinity, lineage, stock, race'. Thus we have

$(echen_1 \rightarrow (ach)\ achen) \rightarrow (echen_2 \rightarrow achen\text{-})$[1].

The Welsh base which we recover is therefore *echen*. No satisfactory etymology has been proposed for this word[2]. We may reconstruct a British Celtic pre-form as **eksinā* or **eksenā*. The first could be a feminine singulative from an old neuter **eksā*

[1] This vocalism could also have been encouraged by *angen* (earlier *anghen*) 'necessity, need, want'. For the derivation of $echen_1$ from *echenu* 'originate, cause' < **eks-ǵenH$_e$-* see Studia Celtica 12–13 (1977–1978), 5. However, contrary to the derivation of $echen_1$ from a prior *achen* f. 'lineage' by crossing with *echenu*, as I envisaged matters there, I now see $echen_1$ as prior and *achen* as a later development by contamination with *ach*; I also explain (loc. cit.) the quite separate origin of *ach* < **akkā* < **ad-kā*.

[2] I hereby reject my proposal of a semantic equivalent to *anghen* < **n-k-Vn-* which I advanced Studia Celtica 12–13, 5.

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(plural or collective); the second could be an original adjective **eks-eno-*. The stem of the old base would then be **eks-*.

We now see that there may be a remnant of this old stem preserved in the verb *echfod* 1 sg. *echfyddaf* = Mid. Ir. *esbaid* 'fail, be wanting'. Although the compound has the appearance of containing the preverb *ex*, it seems probable that this is really an ancient compound, or unified phrase, **eks-* + BE (*bu-t-*, *b(u̯)iu̯-*, *es-*) 'need be, want be (literally)'.

Up to now Lat. *egeō -ēre*, *egestās* 'need, want', *indigus* < **end-ego-s* have been compared only to ON. *ekla* 'lack', OHG. *eko-rōdo* 'only'[3]. Osc. *egmo* (fem.) 'thing' may perhaps also be related; if so, it is a concretized verbal noun in *-mā*.

As Ernout-Meillet remarks, we see the *s*-stem which we expect beside *egeō* reflected in *eges-tās* and also in *egēnus* < **eges-nos*. This leads us to recognize British Celtic **eks-* as **eg-s-*, the zero-grade of *eges-*. The morphological relation here is exactly that of Skt. *vatsá-* 'calf' : Gk. *ἔτος*, Alb. *vit* pl. *vjet* 'year', *viç* 'calf', Lat. *uetus-tus*; of Bret. *le(a)c'h* 'place', Welsh *llesg* 'infirmus' Gk. *λέσχη* 'resting place, resort' < **legh-sk-* or better **legh-s-k-* : Welsh *lle* 'place', *gwe-ly* 'bed', Gk. *λέχος*.

In stem form and affixation Lat. **eges-no-* and Welsh **egs-eno-* appear to be equivalent. The relation is reminiscent of Pers. *ruxš* : Gk. *λύχνος*.

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[3] Pokorny, IEW. 290, Ernout-Meillet, DELL[3] 342, De Vries, Altnord. EW.[2] 99.

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Peeters, Christian, ***Notes on Indo-European and Germanic Etymologies*** , Indogermanische Forschungen, 84 (1979) p.205

Notes on Indo-European and Germanic Etymologies

bosom in Indo-European

In a recent work Samuel Berr (An Etymological Glossary to the Old Saxon Heliand, H. Lang, Bern & Frankfurt 1971, 63) derives OS. *bōsom* and its Germanic cognates: OE. and OFr. *bōsen*, OHG. *buosom*, NHG. *Busen*, NE. *bosom* from an Indo-European form **bhou-/*bhū-*. Technically speaking that cannot be right. IE. **bhou-* could only give PGmc. **ƀau-* which would in turn have yielded OS. *bōsom* (that would be all right) OE. **bēasen*, OFr. **bāsen*, OHG. **bōsom*, NHG. **Bosen*, NE. */ˈbi:zəm/. If we start from IE. **bhū-* we expect PGmc. **ƀū-* and a series OS. and OHG. **būsom*, OE. and OFr. **būsen*, NHG. **Bausen* and NE. */ˈbauzəm/. Conversely if the actually attested Germanic forms have an Indo-European origin, which is by no means certain, they can only be referred to a PGmc. **ƀōs-* from IE. **bhōs-* or IE. **bhās-*.

New English *crowd*, Irish *gruth*

The Modern English word *crowd* has well known Germanic cognates beside OE. *crūdan* : MDu. *krūden*, MHG. *kroten* 'press', also nouns: OE. *crod*, MHG. *krot* 'Gedränge', 'Menge'. Outside Germanic the cognate that is generally mentioned, is Ir. *gruth* 'geronnene Milch, Quark' (Pokorny, IEW. 406, Walde-Pokorny I 650, Holthausen, Altengl. et. Wörterbuch 61). The Indo-European formula proposed to sum up this equation is **greut-* in Pokorny and Walde-Pokorny (loc. cit.). This, however, does not account for the attested correspondences. If the actual equation is duly taken into account there is only one possible Indo-European formula : **grŭt-*. A formula like IE. **greut-* should be reserved for an unambiguous correspondence of the type: OE. *crēodan* = Ir. *gruath*, which is not what we have here.

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New High German *leise*

NHG. *leise* goes back to PGmc. **līs-* as is evidenced by OHG. *līso*. From PGmc. **līs-* there are two Indo-European possibilities: **leis-* or **līs-*. In Kluge-Mitzka (Etymologisches Wörterbuch der deutschen Sprache, 20th ed., Berlin 1967, 435) the German word is referred to IE. **lei-* and connected with Lith. *lýsti* 'mager werden'. Considering the latter word, IE. **līs-* is a much better technical solution. We have indeed a well known correspondence: PGmc. **ī* = Lith. *ý* = IE. **ī*.

Gothic *hunsl*

The Gothic word *hunsl* 'victim of a sacrifice' has a disputed etymology. S. Feist (Vgl. Wörterbuch der gotischen Sprache, Leiden 1939, 277) records quite a lot of different proposals. Technically they derive Goth. *hunsl* from IE. **k^w n̥-sl-*, **kn̥-sl-o* or **kun-sl-o*. It is difficult to decide which of those proposals is the best one. It is, however, possible to refine the conditions for a convincing etymology. It can be shown that IE. **k^wn̥-sl-o* should yield Goth. **hvunsl* rather than *hunsl*. Cases like Goth. *qums* < IE. *g^wm̥-* and Goth. *nehvundja* show that in a labiovelar the labial element is normally retained even before *u* (from IE. **u* or IE. **m̥*, **n̥*, **l̥*). We can therefore say that the solutions implying IE. **kn̥-sl-o* or IE. **kun-sl-o* should be preferred to the one deriving Goth. *hunsl* from IE. **k^wn̥-sl-o*.

daughter in Germanic and Indo-European

The standard Indo-European reconstruction for *daughter* is **dhughətēr* as is well known. It is in fact an excellent summary of the equation Skt. *duhitá* = Gk. *θυγάτηρ*. Some etymological dictionaries (Oxford Dictionary of English Etymology, Oxford 1966, 245; Kluge-Mitzka, Etymologisches Wörterbuch der deutschen Sprache, Berlin 1967, 780), derive the Gmc. cognates: Goth. *dauhtar*, OE. *dohtor*, OS. *dohtar*, OHG. *tohter*, Oicel. *dóttir* from another prototype: **dhuktēr*. This form is an excellent reconstruction because it can only yield what we actually have in the Old Germanic languages. We are, however, not told why IE. **dhugətēr* is unacceptable for the Germanic languages, al-

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though this point can be proved mathematically by means of a kind of ad absurdum argumentation. Starting from IE. **dhughə-tēr* we expect Goth. **dugaþar*, OGH. **tugader* etc. instead of the actually attested forms for which **dhuktēr* is a perfect prototype.

seam in Germanic

The Old Germanic words for *seam*: OE. *sēam*, OHG. *soum*, Oicel. *saumr* suppose a Proto-Germanic form **saumaz* for which there are two possible Indo-European sources: **sou-* or **sau-*. Considering Gk. *ὕμήν* and Lat. *suō*, which seem to point to IE. **su-*, a regular zero grade, the ultimate source of the above mentioned Germanic words can only have been **sou-*. This is worth mentioning in view of the fact that OE. *sēam*, OHG. *soum* and Oicel. *saumr* are found under **si̯ū-* in Pokorny (IEW. 915–16).

Old Icelandic *rǫdd* 'voice'

Oicel. *rǫdd* 'voice' = Goth. *razda* 'speech', 'language', 'tongue' = OHG. *rarta* 'language', 'speech', 'dialect' = OE. *reord* 'language' can be IE. **ras-* or **ros-* with an additional dental which is theoretically IE. **-tˡ-* (> PGmc. **đ* after Verner's law) or IE. **-dh-*. In Jóhannesson (Isländisches etymologisches Wörterbuch, Bern 1956, 697) we find *rǫdd* under IE. **rās-* which could only have yielded Goth. **rozda*, OHG. **ruorta* = Oicel. **ródd* instead of what we actually have. Considering OE. *reord* = IE. **res-*, IE. **ros-* becomes more probable than IE. **ras-* as the ultimate source of Oicel. *rǫdd*.

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Austefjord, Anders, ***Zur Vorgeschichte des germanischen starken Präteritums*** , Indogermanische Forschungen, 84 (1979) p.208

Zur Vorgeschichte des germanischen starken Präteritums

Es ist eine seit langem bestehende und gut fundierte Auffassung, daß das germ. starke Präteritum im großen und ganzen auf das idg. Perfekt zurückzuführen ist. Das Hauptanliegen dieser Arbeit ist zu zeigen, wie sich diese Entwicklung hat vollziehen können. Dabei ist es vorerst notwendig, die formalen und semantischen Züge des idg. Perfekts näher ins Auge zu fassen.

Morphologisch ist der Perfektstamm bei den *e*-Wurzeln durch *o*-Stufe im Singular und Schwundstufe im Plural charakterisiert. Inwieweit die Reduplikation, die für das aind. und gr. Perfekt so typisch ist und auch anderswo vorkommt, schon im Indogermanischen ein fester Bestandteil des Perfektstammes war, läßt sich kaum mit Sicherheit entscheiden. Jedoch deuten altertümliche Formen wie aind. *véda*, gr. οἶδα darauf, daß dies nicht der Fall war. Wahrscheinlich trifft J. Kuryłowicz das Richtige, der annimmt, daß Ablaut und Reduplikation ursprünglich komplementäre Mittel bei der Bildung des Perfekts waren; d. h. die ablautenden Formen hatten keine Reduplikation, diese kam vielmehr den nicht-ablautenden zu[1]. Demnach wäre die Lage im Indogermanischen etwa wie im Gotischen: *niman — nam* aber *haldan — haíhald.*

Die Endungen des Perfekts (aind. Sing. *-a, -tha, -a*; gr. -α, -ϑα, -ε) weichen von denjenigen des Präsens und des Aorists erheblich ab. Auf den ersten Blick stellt sich das Perfekt also als ein Einzelgänger innerhalb der Verbalmorphologie dar, und die klassische Indogermanistik hat sein wahres Wesen nie richtig erkannt. Es sind vor allem die Entdeckung und Erforschung des Hethitischen, die das Perfekt ins rechte Licht gestellt haben.

Das Hethitische hat bekanntlich ein Verbalsystem, das in mancher Hinsicht einfacher ist als das der übrigen altindo-

[1] The Inflectional Categories of Indo-European 1964, S. 70.

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germanischen Sprachen. So gibt es neben dem Indikativ nur einen Imperativ und neben dem Präsens nur ein Präteritum, wenn man von den neugebildeten Umschreibungen vom Typ *iyan ḫarmi* 'ich habe getan' absieht, die auffallend an das Perfekt des Romanischen und Germanischen erinnern. An Genera verbi besitzt die Sprache ein Aktiv und ein Mediopassiv. Endlich unterscheidet das Aktiv zwischen *mi*- und *ḫi*-Konjugation, letztere mit *-ḫi*, *-ti*, *-i* als charakteristischen Endungen im Präsens Singular[2].

Es ist ein Hauptverdienst der Forschung, gezeigt zu haben, daß das „klassische" Perfekt zur heth. *ḫi*-Konjugation und zum Mediopassiv Beziehungen hat. In diesem Zusammenhang ist vor allem auf die bahnbrechenden Arbeiten von J. Kuryłowicz[3] und Chr. S. Stang[4] zu verweisen. Später hat Kuryłowicz die Frage u. a. in Inflectional Categories Kap. II, S. 56ff. behandelt[5].

Als gemeinsame Endungsreihe der drei Kategorien läßt sich im Singular **-a*, **-tha*, **-e/o* (< **-H_2e/o*, **-tH_2e/o*, **-e/o*) rekonstruieren. Diese Endungen scheinen primär eine intransitive Bedeutung gehabt zu haben. Beim Perfektstamm drücken sie den Zustand aus, in dem sich das Subjekt befindet. Der intransitive Charakter des Perfekts ist noch im älteren Griechisch deutlich. Das transitive Perfekt wurde erst in nachhomerischer Zeit üblich. Beim Präsens- und Aoriststamm zeigen die betreffenden Endungen an, daß sich die Handlung oder das Geschehen innerhalb des Bereichs des Subjekts abspielt. Dies ist also das Mediopassiv[6]. Und was endlich die heth. *ḫi*-Verben

[2] Im Plural haben beide Konjugationen die gemeinsame Endungsreihe *-weni*, *-teni*, *-anzi*. D. h. die *ḫi*-Konjugation hat die Endungen der *mi*-Konjugation übernommen. Einen Versuch der Rekonstruktion der verlorengegangenen *ḫi*-Endungen unternimmt E. Neu in seinem Buch „Das hethitische Mediopassiv und seine indogermanischen Grundlagen" 1968, S. 129ff.

[3] BSL. 33 (1932) 1ff.

[4] NTS. 6 (1932) 29ff.

[5] Andere Werke, die sich eingehend mit dem Problemkreis Perfekt — *ḫi*-Konjugation — Mediopassiv auseinandersetzen, sind das schon erwähnte von E. Neu sowie C. Watkins, Indogermanische Grammatik 1968, besonders Kap. VIII, S. 105ff.

[6] Näheres zum Gegensatz Aktiv — Mediopassiv, vgl. z. B. E. Benveniste, Problèmes de linguistique générale 1966, S. 168ff.

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betrifft, so sind viele von ihnen intransitiv. Das gilt z.B. für *ak-* 'sterben', *mai-* 'wachsen', *wašta-* 'sündigen'. Kuryłowicz beschreibt die *ḫi*-Konjugation als deponential[7].

Es ist ohne weiteres klar, daß die ursprünglich gemeinsame Endungsreihe im Perfekt am reinsten vorliegt (vgl. Griechisch und Altindisch). Das Perfekt wäre demnach die Grundlage der beiden jüngeren Kategorien. Gewiß nicht zu Unrecht rechnet Neu mit einem ursprünglichen diathetischen Gegensatz Aktivum —Perfektum, „die eine Diathese zur Bezeichnung einer Handlung, die andere zur Bezeichnung eines Zustandes" (op. cit., S. 133). Im Präsens der *ḫi*-Konjugation ist die Endungsreihe um das *-i* erweitert worden, das auch in den Primärendungen idg. **-mi*, **-si*, **-ti*, **-e/onti* vorliegt: *-ḫi*, *-ti*, *-i* < **-ḫai*, **-tai*, **-ai*[8]. Dies könnte mit einer im Hethitischen zu beobachtenden Tendenz zusammenhängen, Präsensformen mittels Anfügung von *-i* zu bilden. Die Formen dürften jedoch eher altererbt sein, da sie ebenfalls im lat. Perfekt vorliegen[9]. Auch im historisch belegten Mediopassiv schimmern die alten „intransitiven" Endungen durch. Da es sich in diesem Aufsatz nicht um das Verhältnis zwischen Perfekt und Mediopassiv handelt, scheint es nicht angebracht, alles relevante Material heranzuziehen. Nur einige heth. und aind. Medialformen seien herausgegriffen, um den Zusammenhang zu zeigen. Wenn man von den Erweiterungen *-ri*, *-ti* absieht, sind die Endungen des heth. Mediopassivs im Singular *-ḫa*, *-ta*, *-(t)a*. In der 3. P. ist die Variante ohne *t* die ältere, wie Watkins gezeigt hat[10]. Hier erscheint also wie im Perfekt die „intransitive" Reihe ohne zusätzliches *-i*. In der 2. P. Sing. Imperfekt und Aorist des Altindischen finden wir die Endung *-thāḥ*, vgl. *áyunkthāḥ*, *áyukthāḥ* zu *yuj-* 'verbinden'[11]. Im Vedischen begegnen Formen wie 3. P. Sing. Im-

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[7] Inflectional Categories, S. 68.

[8] Vgl. B. Rosenkranz, KZ. 75 (1958) 216. R. setzt für die 2. P. **-thai* an.

[9] Näheres bei Neu, op. cit., S. 178, der auch auf Rosenkranz, JbklF. 2 (1953) 344 verweist.

[10] Op. cit., S. 84.

[11] Das Verhältnis zwischen der Perfektendung *-tha* und der Medialendung *-thāḥ* (< **-thēs*) wird u. a. von Stang, op. cit., S. 31 und Kuryłowicz, Inflectional Categories, S. 58f. erörtert.

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perfekt *áduha(t)*, *áśaya(t)* zu *duh-* 'melken' und *śī-* 'liegen', wo das *-t* wiederum eine Erweiterung darstellt. Mit dem *-i* der Primärendungen haben wir 1. P. Sing. Präsens *bháre* (< **-ai*) zu *bhṛ-* 'tragen' und 3. P. Sing. Präsens *bruvé*, *śṛṇvé* zu *brū-* 'sagen' und *śru-* 'hören'.

Die ursprüngliche Gemeinsamkeit des Perfekts und des Mediopassivs, was die Endungen betrifft, wird mit der Zeit dadurch verwischt, daß letzteres dem Einfluß des Aktivs ausgesetzt wird. Wie dieses erhält es im Indikativ Präsens Primärendungen durch Hinzufügung von *-i* (vgl. aind. *-e* < **-ai* oben)[12]. Auch beeinflußt die ursprünglich aktivische Reihe **-m*, **-s*, **-t* immer mehr das Paradigma des Mediopassivs: dem aind. *bháre* entspricht bekanntlich im Griechischen ein φέρομαι. Die älteren Formen *bruvé*, *śṛṇvé* werden nach und nach durch die jüngeren *brūté*, *śṛṇuté* ersetzt.

Das Perfekt drückt, wie oben angedeutet, einen subjektbezogenen Zustand aus, und eben dies ist die primäre Bedeutung des idg. Perfekts. Das Moment „als Ergebnis einer vorausgegangenen Handlung", das von vielen erwähnt wird, ist sekundär. Bekanntlich fehlt es häufig bei aind. und gr. Perfektformen[13]. Die Form γέγηθα z.B. ist etwa als 'ich bin voller Freude' zu übersetzen. Sie hat also reine Zustandsbedeutung, und diese wird im Verhältnis zum Präsens γηθέω hervorgehoben, das in höherem Maße eine aktivische oder dynamische Bedeutung hat: 'ich freue mich'. Von einem Zustand als Ergebnis einer vorausgegangenen Handlung kann m. E. nur dann die Rede sein, wenn diese perfektiv ist. Dies läßt sich an einer Form wie gr. πέφευγα beobachten: 'ich bin außer Gefahr, weil ich die Flucht ergriff'. Das idg. Perfekt erinnert also an das Zustands- oder *sein*-Passiv des Deutschen. Auch hier ist das Bedeutungsmoment des Zustands primär, was aus Sätzen wie *er ist von allen geliebt, damit ist ihr nicht gedient* ersichtlich ist. Wenn sich der Zustand aus einer vorhergegangenen Handlung ergibt, muß diese perfektiv sein: *der Feind ist geschlagen, die Tür ist geöffnet.*

[12] Man bemerke, daß das heth. Mediopassiv ein *-i* nur in den erweiterten Endungen *-ḫari*, *-tati* usw. aufweist.

[13] Eine gute Darstellung des gr. Perfekts findet sich bei J. Wackernagel, Vorlesungen über Syntax I (1926), S. 166ff.

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Wir wollen uns jetzt dem germ. starken Präteritum zuwenden. Für dessen Herkunft aus dem idg. Perfekt spricht deutlich die Gestalt des Wurzelmorphems mit *o*-Abtönung im Singular und — bei den drei ersten Ablautreihen — Schwundstufe im Plural. In die gleiche Richtung weisen auch die Endungen des Singulars. Die Ø-Endung der 1. und 3. Person läßt sich aus idg. **-a*, **-e* erklären mit Vokalabfall im absoluten Auslaut. Ebenfalls ist das *-t* des Gotischen und Nordischen aus idg. **-tha* herleitbar. Zwar fehlt dem germ. starken Präteritum — mit Ausnahme einiger wohlbekannter Formen im Gotischen und anderswo — die Reduplikation. Wie wir aber schon gesehen haben, steht es keineswegs fest, daß diese dem idg. Perfekt als solchem eigen war.

Semantisch und syntaktisch gibt es aber tiefgreifende Unterschiede zwischen dem germ. starken Präteritum und dem idg. Perfekt. Während letzteres einen Zustand am Subjekt ausdrückt und demzufolge intransitiv gebraucht wird, ist ersteres ein rein temporales Gegenstück zum Präsens, es wird wie dieses transitiv oder intransitiv gebraucht.

Es fragt sich nun, wie diese Umwandlung im Germanischen vor sich gegangen ist. Wie es scheint, ist das Problem in der einschlägigen Literatur dürftig behandelt worden. Eine Erklärung dafür dürfte sein, daß man in den meisten Darstellungen das idg. Perfekt nicht präzis genug umrissen hat. H. Hirt gibt zwar einige Hinweise, wenn er schreibt: „Die Bedeutung des Perfekts war ursprünglich sicher nicht die der Vergangenheit, aber schon im Idg. konnte es teils die Vergangenheit, teils die Gegenwart bezeichnen, und das hat sich wohl schon im Idg. und dann weiter im Germanischen und anderswo dahin entwickelt, daß gewisse Perfekte Präsensbedeutung erhielten, andere Vergangenheitsbedeutung. Die ersten nennen wir Präteritopräsentia, die andern sind die gewöhnlichen Präterita.“[14] Daß das Perfekt ursprünglich sicher nicht Vergangenheitsbedeutung gehabt hat, ist richtig. Was er aber weiter ausführt, ist nicht ganz klar. Bezieht sich „schon im Idg.“ auf ein späteres Stadium als „ursprünglich“? Man bekommt jedenfalls nicht zu wissen, wie sich diese zweifache Bedeutung entwickelt hat.

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[14] Handbuch des Urgermanischen II (1932), S. 141.

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Neuerdings hat W. Meid die vorgeschichtliche Entwicklung des germ. Präteritums — sowohl des starken als auch des schwachen — behandelt[15]. Die älteste Bedeutung des idg. Perfekts wird als „die des *Zustandes am Subjekt*“[16] beschrieben (S. 18). Wie aus der Darlegung S. 33–34 hervorgeht, ist bei durativen Wurzeln dieser Zustand nicht das Ergebnis einer vorausgegangenen Handlung: „Die Bedeutung solcher 'Perfekta'[17] ist als rein medial-zuständlich aufzufassen, d. h. sie bezeichnen wohl einen Zustand am Subjekt, doch ohne vorausgegangene abgeschlossene Handlung.“ Unsere Auffassungen vom idg. Perfekt stimmen also gut überein.

Die weitere Entwicklung des alten medial-zuständlichen Perfekts behandelt Meid besonders S. 40ff. Er legt dabei großes Gewicht auf dessen Unterordnung unter die aktivische Diathese, wie sie u. a. im Altindischen und Griechischen zu beobachten ist. Infolgedessen konnte bei transitiven Verben auch das Perfekt transitiv gebraucht werden. Auch im Germanischen, wo das Medium schließlich schwand, mußte nach Meid das Perfekt dem Aktiv untergeordnet werden und eventuell transitiven Charakter erhalten. Dies ist durchaus möglich. Da aber Gegensätze wie Aktiv—Medium, transitiv—intransitiv, subjektbezogen—objektbezogen nichts mit Tempus zu tun haben, ist es schwer einzusehen, daß die „Aktivisierung“ des Perfekts auch eine Tempusverschiebung bewirken sollte. Im Lateinischen gibt es bekanntlich die Verben *memini* 'ich erinnere mich' und *ōdi* 'ich hasse'. Es sind dies alte Perfekta mit präsentischer Bedeutung. Beide flektieren aktivisch, und beide können ein Objekt zu sich nehmen: *memini alicujus, ōdi aliquem.* Dies beeinflußt jedoch nicht ihre Zeitreferenz. Auch die germ. Präteritopräsentia sind im Hinblick auf Diathese als aktivisch anzusehen. Sie haben aber ihren alten Gegenwartsbezug beibehalten.

Vergegenwärtigen wir uns noch einmal die „Urbedeutung“ des idg. Perfekts: Es bezeichnete einen Zustand des Subjekts. Bei perfektiven Verben war dieser das Ergebnis einer vorausgegangenen Handlung. Der Ausgangspunkt der Umwandlung zum

[15] Das germanische Präteritum 1971.

[16] Von Meid hervorgehoben.

[17] D. h. Perfekta zu durativen Wurzeln.

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starken Präteritum des Germanischen müssen m. E. Perfektformen von perfektiven Verben sein, bei denen also ein Bedeutungsmoment der Vergangenheit vorhanden war. Eine Form wie **kwam(a)* konnte demgemäß als Vergangenheitsform zu **kwemō* 'ich komme' umgedeutet werden. Diese Erscheinung hat auch in den modernen Sprachen ihre wohlbekannten Parallelen. So ist im heutigen Französisch *je suis venu* die gewöhnliche Präteritalform zu *je viens*. Auf dieser Stufe konnte sich die Analogie geltend machen, so daß auch durative Verben Vergangenheitsformen aus ehemaligen Perfekta erhalten konnten, z.B. **skain(a)* zu **skeinō* 'ich scheine'. Falls das Perfekt nicht bereits nach dem oben erwähnten Vorgang transitiviert worden war, kann man sich als letzten Schritt die analogische Übertragung auf die transitiven Verben vorstellen: Die Tempusopposition, die jetzt bei **kwemō—*kwam(a)*, **skeinō—*skain(a)* usw. vorhanden war, hat sich auch bei **teuhō—*tauh(a)* usw. durchgesetzt.

Nicht alle Verben besaßen im Indogermanischen eigene Perfektformen. Nach Ausweis des Lateinischen und des Griechischen trifft dies z.B. für **bher-* zu (*fero—tuli*, φέρω—ενήνοχα). Demnach wäre **bar(a)* eine analogische Neuschöpfung des Germanischen. Dasselbe dürfte auch für eine Reihe anderer Formen gelten. Es ist jedoch kaum möglich, diese im einzelnen Fall mit Sicherheit zu bestimmen.

Das starke Präteritum, wie es in den überlieferten Sprachen vorliegt, läßt sich jedoch nicht in seiner Gesamtheit aus dem idg. Perfekt erklären. Die 2. P. Sing. des Westgermanischen (ahd. *liwi*, *zugi* usw.) fällt, was sowohl die Wurzelgestalt als auch die Endung betrifft, ganz aus dem Rahmen des Perfekts. Es scheint jetzt eine allgemein verbreitete Auffassung zu sein, daß es sich hier um eine ehemalige Aoristform handelt[18]. Die Mehrzahlformen sind hinsichtlich des Wurzelmorphems zweideutig. Die Schwundstufe der drei ersten Reihen läßt sich sowohl aus dem Perfekt als auch aus dem Aorist erklären. Was die Endungen betrifft, so weist das *-n* der 3. P. auf aoristische Herkunft hin.

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[18] Diese Erklärung geht auf J. von Fierlinger, KZ. 27 (1885) 430ff. zurück.

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Es ist jedoch nicht ausgeschlossen, daß wir es hier mit einer analogischen Übertragung des *-n* des Präsens zu tun haben. Man beachte, daß diese Endung auch bei den präteritopräsentischen Verben vorliegt. Die Einwirkung seitens des Aorists muß zu einer Zeit stattgefunden haben, wo das ehemalige Perfekt schon präteritale Bedeutung angenommen hatte. Es ist bezeichnend, daß die Präteritopräsentia, die die dargestellte Entwicklung nicht mitgemacht haben, auch in der 2. P. Sing. des Westgermanischen ihre altererbte Perfektform bewahrt haben.

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The Gothic Verbal Dual in *-ts* and Its Indo-European Origins

One of the most intriguing questions concerning Gothic verb morphology involves the origin of the Gothic second person dual ending in *-ts*. The problem here stems from the fact that the Gothic ending seems to correspond to the Sanskrit second person dual ending *-thas*, both forms together implying an Indo-European suffix **-the/os*. However, as most scholars agree, "Sk. *th* may be an Indic innovation" (Prokosch 1939: 212), suggesting that the original ending was actually **-te/os*. Thus, the Indo-European dental consonant should have produced Gothic *-þ*, not *-t*, according to the operation of regular phonological change. Although many hypotheses have been suggested to explain this fact, the following paper attempts to provide a novel explanation for the occurrence of the ending *-ts* in Gothic. It is suggested that the discrepancy between the Sanskrit and the Gothic forms is a function of an original regular phonological alternation in Indo-European between **-t* and **-d* (> Go. *-t*), whose source may be found in sandhi phenomena. I frankly admit that this tonclusion cannot be proven absolutely correct; however, I feel chat it is as reasonable as any that have been suggested and therefore merits consideration as a possible explanatory hypothesis.

Among the early explanations of the origin of Gothic *-ts* are two suggested by Brugmann (1916: 641). He says: "Die Annahme, daß *-ts* zunächst nur hinter *s, f, h* gestanden habe und von da aus verallgemeinert worden sei, eine Auffassung, die durch das *-t* der 2. Sing. Ind. Perf., z.B. *skalt*, nahe gelegt ist . . ., ist wenig wahrscheinlich, weil keine Form auf *-sts* oder dgl. erscheint: es heißt z.B. *ga-sēhvuts*, nicht **ga-sēhts*. Eher ist glaublich, daß *t* vor dem stimmlosen *-s* lautgesetzlich aus einem Spiranten entstanden war, vgl. aisl. *z* aus *ðs* in *góz*, Gen. zu *góðr* 'gut' u. dgl. Mark. 10,38 steht *wituþs* in CA." Most other explanations of

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the ending tend to follow one of the two theories which Brugmann propounds here. Prokosch (1939: 212), for example, says of the suffix: "The ending of the 2nd pers., *-ats*, must be identical with Sk. *-a-thas*, IE. *-o-tes/-tos*, probably for earlier *-e-tes/-tos* (Sk. *th* may be an Indic innovation). This should give us Go. *-aþs*; *t* for *þ* is probably dissimilation, somewhat similar to the fact that *st* never became *sþ*." Wright and Sayce (1954: 137–138), on the other hand, accept the first proposal made by Brugmann: "*nimats* has *-ts* from the pret. dual; . . . The pret. indic. is morphologically an old perfect, which already in prim Germanic was chiefly used to express the past tense . . . *-tha*, the original ending of the second pers., would have regularly become *-þ* in Goth. . . . , except after prim. Germanic *s*, *f*, *x* where it regularly became *-t*, as Goth. *last* 'thou didst gather' . . . This *-t* became generalized in prim. Germanic, as Goth. O. Icel. *namt*. But in the West Germanic languages the old ending was only preserved in the preterite-present verbs, . . ." Hirt (1932: 139) also prefers a modified version of Brugmann's first suggestion, "stating that regular /ð/ or /þ/ < /t/ had become /t/ again before /s/ after the loss of the preceding vowel" (Schmidt 1974: 83–84).

Some innovative proposals, however, have been made in recent years. Stang (1949: 338) maintains that the original ending included **-t*+a laryngeal consonant, which became *-th* in Sanskrit and generally *-þ/-ð* in Germanic with complete loss of the laryngeal. But the Gothic ending, he believes, continues to reflect the influence of the laryngeal, which prevented the shifting of **t* to *þ*. Schmidt (1974: 84–85) makes the interesting suggestion that the ending "must be traced back to the Proto-Indo-European numeral for 'two', **dwō-* or its weak form **dwo-/dwi-*". He says: "If we conclude that the same numeral form **dwo/dwi-* is reflected in the Gothic inflectional endings of the dual 2, we have a solution for the troublesome *-ts*, because now it can be explained according to regular phonologic developments rather than through spasmodic, unpredictable changes. The development of **-dw(o)-s/dw(i)-s* > **-t(w)-z* > **t(u)-z* > Go. *-t-s* follows exactly definable phonologic processes: first, short *-o-/-i-* regularly disappears in weakly stressed final syllables; the preceding *-w-* becomes *-u-* and is lost in third or subsequent

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syllables; /d/ becomes /t/ according to the Proto-Germanic consonant shift; and P. Gmc. /z/ becomes Go. /s/ in final position. The final *-s* of the dual 2 may have its origin in the **-si/-s* ending of the sing. 2, in the *-s* of the dual 1 (IE. **-wes/wos*), or the plural 1 (IE. **-mes/mos*)."

In this paper I shall follow Schmidt in maintaining that the *-t* of the Gothic ending derives from a PIE. **d*. However, I feel that Schmidt's argument that this **d* is part of the numeral 'two' is weak for two reasons: first, the occurrence of this numeral as a desinence is attested elsewhere only in the pronouns; and second, the correspondence between the Gothic form and the Sanskrit ending is so close that any proposal should attempt to account for their common origin.

Before directly pursuing an explanation of the source of Go. *-ts*, some background remarks about the development of the Indo-European verbal endings are necessary. Erhart (1970: 113) argues that in early Indo-European, "es bestand wohl damals noch kein Unterschied zwischen der 2. und der 3. Person, zwischen dem Plural und dem Singular usw.". He notes that "die Endungen der 2. Person Sg. enthalten zum größeren Teil ein *s*: aind. *ḥ, si*, gr. *s, si*, het. *š, ši*, arm. *s*, lat. *s*, got. *s*, sl. *šь* usw. . . ." (1970: 56); but he is quick to point out (1970: 56–57) that a second-person ending in **-t* is attested in the second person singular ("(*t*) het. *t, ti*(?). toch. AB *t*(?); (*t* + *V*) aind. *tha, thāḥ*, gr. *thēs*(?), het. *ta, tari*, toch. A *tār, te*, B *tar, tai*, air. *tha, ther*, got. *þ*"), and the second person non-singular (dual-plural) ("(*t* + *V*) aind. *ta, tha, thaḥ, tam* (usw.), gr. *te, ton*, het. *ten*(*i*), toch. A *c, cār*, lat. *tis, te*, got. *þ*, lit. *te, ta*, sl. *te, ta* usw."). Moreover, "in einigen Personalendungen der 2. Person (Sg. u. Pl.) stehen die Elemente *s* und *t*(*h*) nebeneinander: gr. *stha*, het. *šta, šten*(*i*), toch. A *st*, B *sta*, lag. *istī, istis*" (Erhart 1970: 57). After pointing out the general occurrence of **-t* as a desinence of the third person (sg.) ("aind. *t, ti*, gr. *ti*, het. *t. zi*, toch. A *t*, lat. *t*, got. *þ*, sl. *tь, tъ*"), he thus concludes: "Die Endungen der 2. Person (aller drei Numeri) enthalten zum Teil denselben Kern . . . wie die meisten Endungen der 3. Person Sing. Der Unterschied *t* (3. u. 2. Pers.): *th* (nur 2. Ps.) ist vielleicht in der Weise zu deuten, daß die schon seit der pie. Periode bestehende

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phonetische Variation *t* ~ *th* später zur sekundären Differenzierung grammatischer Formen ausgenützt worden ist" (1970; 57–58). It is interesting to note that **-s* also occurs in certain third person singular endings: "Toch. A *pälkäs* (present tense), and Hitt. *pāiš* 'gave' and *dāiš* 'put' (preterit)" (Schmalstieg 1977: 24). What all of this seems to imply is that the 2–3 person category whose original exponent was probably **-Ø*, cf. Watkins 1962: 90–106[1], utilized both **-s* and **-t* as its desinences, with **-s* gradually becoming specialized primarily in the second person, and **-t* in the third, although remnants of the original vacillation are still historically attested. Thus, I believe that at some stage of development Indo-European possessed a verbal paradigm like the folllowing:

1. pers. *-m*
2. pers. *-s/-t*
3. pers. *-s/-t*

The number of endings in which these formants came to appear gradually increased[2], many resulting from the simple process of contamination, e. g. **-ti* < **-t* + *i* (= a deictic particle indicating "hic et nunc", cf. Watkins 1962: 100), **-ont* < **-oN* (= "a marker for the non-singular (i. e. dual/plural)") + *t* (="the element found in the 2nd plural **-t(e)*") (Schmalstieg 1976: 25).

Now from an early date it seems that Indo-European had phonological alternations which were the result of external sandhi. Of special importance here is the fact that "voiceless stops and spirants became voiced before voiced consonants,

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[1] The occurence of **-Ø* in the second person function is still attested in the singular imperative (**age* 'lead' : Skt. *ája*, Gk. *áge*, Lat. *age*). Moreover, as Erhart (1970: 57–58) says: "In einem kleinen Teil der Fälle sind die Endungen der 3. Person Sg. akonsonantisch: aind. *a*, *e*, gr. *ei*, *e*, het. *i*, *a*, *ari*, toch. AB *Ø*, got. *Ø*, lit. *a* usw. . . .; als ihre Bausteine sind der thematische Vokal und der Präsensdeterminativ *i* (bzw. *r*) zu erkennen."

[2] As Biese (1950: 3) remarks: "It is mostly assumed that the early history of Common Indo-European, a highly inflectional language, goes back into a non-inflectional or pre-inflectional stage, inflection in the form in which we find it in Common Ie. being of comparatively late development."

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although it is by no means certain that they did so before vowels as well" and that "voiced stops became voiceless before voiceless stops and spirants, and possibly in pause also" (Ward 1946: 102). Thus, Brugmann (1930: 883–884) argues: "Stimmlose Geräuschlaute im Wortauslaut wurden vor stimmhaften Geräuschlauten selbst stimmhaft. Etwa **edōd bhrātrai* 'er gab dem Bruder' = ai. *ádād bhrā́trē*; **tāz dhughəteres* (*dhugdheres*) 'diese Töchter' = ai. *tā́ duhitáras* . . ., ai. *sū́rē duhitā́* 'Tochter der Sonne' aus **sūraz d-* . . .; gr. *Athénadze* = *Athénaz de*, gortyn. *uiéed dé* 'filii autem' aus *uiéez dé*; aksl. *poz-dъ poz-dě* 'spät' zu av. *pas-ča* lat. *pos-t* . . . ." Likewise, "stimmhafte Verschlußlaute im Wortauslaut wurden vor stimmlosen Geräuschlauten selbst stimmlos. Etwa **tot peĥu* 'dieses Vieh' = ai. *tát pā́śu*; lat. **tot per* (*topper*), **it circō* (*iccircō*), **at serō* (*asserō*, vgl. osk. *aserum*). Auch vermuten Einige, vielleicht richtig, daß diese Consonanten auch im Satzauslaut stimmlos gesprochen worden sind." Therefore, the verbal ending which is generally reconstructed as **-t* would have had two sandhi variants, /t/ and /d/.

It not infrequently happens in the development of languages that one sandhi variant is generalized at the expence of the other, especially if the factors which originally conditioned the alternation are in some way altered. Shevelov (1965: 366–367), for example, notes that after final consonants were lost in all words except prepositions in early Slavic, "with the rules of the outdated IE. 'voicing type' of *sandhī* gradually forgotten, the 'voiced form' of a great many prepositions, i. e. the statistically prevailing form with a final voiced consonant (as it occurred before vowels, sonants, and voiced consonants) was perceived as the basic, normal form. . . . this form of preposition in many Sl languages was generalized. This explains why Sl has *iz* 'from' as a counterpart of Li *ìš* OPr *is-*, Gr *es*, La *ex*, Ir *ess*, all with a voiceless final consonant." Moreover, the two variants may each be assigned a specific and autonomous function. Thus, Schmalstieg (1974: 187), cf. Szemerényi (1970: 308), suggests that "the Indo-European 1st sg. secondary ending **-om* and the primary ending **-ō* (derived from **-om* in pre-consonantal sandhi) were originally merely sandhi variants. In general the phonologically newer form in **-ō* takes over the primary func-

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tion of the present tense, whereas the older form, the ending **-om* is found in the non-present formations. Thus, for example, we find the 1st sg. pres. Gk. *phér-ō*, Skt. *bhár-ā-mi* vs. the 1st sg. imperfect Gk. *épher-on*, Skt. *ábhar-am.*"[3]

I would like to suggest that the Gothic second person dual ending *-ts* and the Sanskrit second person dual ending *-thas* both derive from an original IE. **-Te/os* (where *T* = *t* or *d*), which itself is a contamination of the old second-third person verbal desinence **-T* with the so-called connecting vowel **-e/o-* and a marker of the non-singular, **-s*[4]. However, Gothic attests the voiced sandhi variant of the dental, while Sanskrit attests the voiceless one. Apparently when the contaminated ending was being created, speakers could choose either variant as the basis of the suffix. Thus, there was a period of vacillation in use when two alternate forms of the ending **-Te/os* existed side by side, with the Indo-Iranian dialects eventually generalizing the one containing the voiceless dental consonant and the Germanic dialects eventually generalizing the variant containing the voiced dental consonant[5]. To be sure, recent studies of linguistic variation and change show that such unsettled developments are to

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[3] A laryngealist interpretation of this alternation as a function of sandhi appears in Heller 1957.

[4] In regard to the use of **-s* as a marker of the non-singular, see Schmalstieg 1977: 133–139. It is clearly seen in this function in the contrast between the Sanskrit verbal endings 1 sg. *-m(i)* and 1 pl. *-mas(i)*.

[5] It is clear that the emergence of a special inflectional non-singular category in the verb was a rather late development in Indo-European, probably dating from the time when the various dialects had begun to emerge as autonomous entities. Lehmann (1974: 210–202) says: "The system of verb endings clearly points to an earlier period in which there was no verbal inflection for number . . . For the dual and plural endings are obviously defective. We cannot reconstruct endings in these two numbers which are as well supported as are those of the singular, except for the third plural. Only the third plural, as indicated, can be posited for an early period of PIE. The development of precisely third-person forms to express number supports the assumption that the number category was used for congruence." Thus, the occurrence of such dialectal variation in use is to be expected here because dialectal differentiation was underway when this ending appeared.

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be expected. As Bailey (1973: 157) observes: "... we should take notice of discoveries concerning the spread of language changes in the geographical dimension that have been made by advocates of the new lectology ... [A change] begins variably rather than categorically; that is, it begins as a rule that sometimes operates and sometimes does not." The ending **-de/os* would then yield Go. *-ts* through the operation of Grimm's Law and the loss of weakly accented short vowels in final syllables.

I believe that the processes which gave rise to this form in Gothic are neither limited to this ending nor to the Germanic group itself. Despite the sobering exhortation of Collinge (1959: 232) that until "a sound statement of the criteria which will allow us to admit the influence of the whole utterance upon the part in working out the histories of stable, repeated and separately quotable IE. word-sounds" appears, "external sandhi is an attractive but expensive and unreliable toy for historical phonologists"[6], the general analysis presented here explains some other interesting phonological data.

For example, Szemerényi (1973: 59–60) notes the existence of some sort of voiced/voiceless alternation in Indo-European which continued into Germanic when he writes: "Corresponding to the Italic group of *sub super- supero-*, which has a secondary *s-* (cf. Skt. *upa/upari/upara-*), Germanic offers Goth. *uf* 'under', Old Norse *of* 'over', Old High Germ. *ūf*; Goth. *ufar* 'over', ON. *yfer*, Old Engl. *ofer*, OHG. *ubar*, *ubir* 'über'; Goth. *ufaro* 'over', OE. *yferra*, OHG. *obaro* 'upper'. But beside these forms which show the regular correspondences Gmc. *f/ƀ*: IE. *p*, there are forms with a surprising *p*: OE. *up*, *upp* 'upward', *uppe* 'above', *uppan* 'from above'; ON. *upp* 'upward', *uppe* 'above'; Old Saxon *up*, *upp* 'upward', *uppa* 'above', *uppan*. Whatever

[6] It should be pointed out that Collinge (1959: especially 232) admits his adherence to a strict Neo-grammarian approach to sound change, a view which contemporary variation theory has largely discredited. It is this view which colors his feelings toward sandhi as an explanatory statement. Thus, he calls any explanation which includes sandhi phenomena "a mechanism for the *ad libitum* disregarding of the provisions of sound-laws" (232); and he concludes his article by saying, "After all, Leskien gained a notable if not always remembered victory in 1876; let us not too lightly surrender the ground then won."

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the explanation of the Germanic geminates, it is clear that they are an innovation, in the present case an innovation confined to North Germanic and part of West Germanic. The point of departure was Gmc. **up* as is shown by Gothic *iupa* 'above', *iupana* 'from above, from the beginning', *iupaþrō* 'from above'; this could be made 'expressive' either by consonant-gemination or by vowel-lengthening: *ūp* is seen in OHG. *ūf* 'upward', *ūfana* 'from above'. The source of Gmc. **up* can only be a pre-Gmc. **ub* which coincides with Lat. *sub*, Umbrian *sub*: all show voicing of the final stop in IE. **up*." He continues: "A further instance—which already provides the transition to Indo-European—is presented by the demonstrative pronoun **to-*. Its neuter is generally posited as **tod*, a reconstruction prompted by the Germanic evidence since Goth. *þata* points to a pre-Gmc. **tod*. This form can also be represented by Lat. *is-tud* but the Latin form might just as well reflect an IE. **tot*. This formation is originally in all probability a repetition of the stem, i. e. **to-to* just as the masc. *so* can appear as **sos* (Skt. *sas*, Gk. *hós*) from **so-so*. We must then assume that IE. **tot* either changed to IE. **tod* or else that the final stop was voiced in pre-Germanic, Italic, and, as shown by the Old Irish pronouns *(h)ed* 'id', *ced* 'quid', even in Celtic."[7]

Moreover, Buck (1933: 245) observes: "In Italic the endings *-ti* and *-t* became respectively *-t* and *-d*, the difference between primary and secondary endings being thus maintained. Cf. Osc. 3. sg. pres. indic. *faamat*, *kasit* but 3 sg. perf. indic. *kúmbened*, 3 sg. pres. subj. *fakiiad*. In early Latin too, inscriptions have *fhefhaked*, *feced*, *sied* but usually *-t* in all tenses as later." The loss of the final **-i* of the primary ending, which Latin later

[7] Szemerényi in this article (1973) argues that "in a fair number of IE. languages single stops, perhaps also spirants, in word-final position became voiced" (62) and that sandhi alternations developed only "at a subsequent stage" when "the voiced sounds were unvoiced by the voiceless initial of a following word" (62). Although his position is an interesting one, I have chosen to follow the more traditional analysis because of his admittedly scanty evidence (62). However, it is obvious that Szemerényi's hypothesis presents no problems for what I have suggested since it may simply be that the dental of both the Sanskrit and the Gothic endings derives from **-d*, with Sanskrit showing a later devoicing and Gothic preserving the original form.

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generalized, is traditionally ascribed to syncope (Buck 1933: 98–99) and is parallel by a loss of **-i* in, for example, *et* from **eti* (cf. Gk. *éti*) and *tot quot* from **toti *kʷoti* (cf. Skt. *tati kati*). I believe, cf. Meillet and Vendryes 1953: 146ff., that the Italic verbal suffix *-d* simply represents a generalization of the voiced sandhi variant of the secondary ending at the expence of the voiceless one in /t/. Thus, it seems that this Italic verbal desinence results from the same basic process which gave rise to the Gothic form in *-ts*.

Although the analysis just presented is speculative in the sense that it cannot be proven absolutely correct, I again submit that it is as reasonable as any which have been purported up to this time. If it does nothing else, the proposal should re-emphasize that rarely are there unique solutions to the complex questions posed by the reconstruction of Indo-European.

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A Case of Homonymic Clash in Germanic

The first element **agwi-* (IEW. 44)[1] of the Germanic compound **agwi-þahsiō* (Modern German *Eidechse*) 'reptile' is attested in a variety of forms in the oldest Germanic dialects. In OHG. *egi-*, the normal form, is sometimes replaced by *ewi-* or other variants[2], and OE. *ā-ðexe* attests a form for which explanations have proved elusive[3]. Conclusions concerning the OHG. forms, too, have been somewhat tentative. Meillet[4] speaks of 'quelque influence particulière'. Seebold[5] considers that *ewi-* may be phonologically regular, but that it may also be a deviant development. Zupitza[6] was more definite. He considered accentuation to be responsible. His suggestion has found no support, however, probably because evidence that the development of *-gw-* could be affected by accent has been offered neither by him nor by anyone else. The fact is that *-gw-* is a difficult consonant[7], so that uncertainties of this type are inevitable. This present

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[1] IEW. is used as the abbreviation for Julius Pokorny, Indogermanisches etymologisches Wörterbuch, 2 volumes, Berne and Munich 1959–69, Volume I.

[2] The forms are given in Otto Gröger, Die althochdeutsche und altsächsische Kompositionsfuge mit Verzeichnis der althochdeutschen und altsächsischen Composita (Gesellschaft für deutsche Sprache in Zürich, Abhandlungen 11, Zürich 1911) 307.

[3] 'Verderbt', Alois Walde, Vergleichendes Wörterbuch der indogermanischen Sprachen, ed. Julius Pokorny, third volume (Register) ed. Konstantin Reichardt, 3 volumes, Berlin and Leipzig 1927–1932, I 65; 'starke Umgestaltungen', Elmar Seebold, ZfvSf. 81 (1967) 125.

[4] A. Meillet, Mémoires de la Société de Linguistique de Paris 15 (1908–9) 102.

[5] ZfvSf. 81, 125/6.

[6] Ernst Zupitza, Die germanischen Gutturale (Schriften zur germanischen Philologie 8, Berlin 1896) 99.

[7] Studied most recently by Seebold ZfvSf. 81, 104–133 (his results are tabulated p. 132), but even accepting his findings, his summary contains many explicit uncertainties.

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study attempts to cast some incidental light on the probable development of *-gw-*, but is more explicitly concerned with identifying circumstances which may have interfered with its regular development in the case of **agwi-*.

Despite the difficulties surrounding the development of *-gw-*, it is clearly important, in considering, essentially, why **agwi-* gave rise in OHG. to forms in both *egi* and *ewi*, to know the development of *-gw-* before *i*. Fortunately, this seems to be one of the easier combinations. The uniform result is *-w-*. The examples are the following:

Indo-European **aigʷh-* 'be ashamed' (IEW. 14): Gothic *aiwiski*, OE. *ǣwisc* 'outrage', Gothic *aiwiskōn*, 'treat shamefully', Gothic *unaiwisks* 'shameless', MLG. *eisch* 'mean', MLG. *eicheln*, *ēchelen*, *ēgelen* 'feel disgust';

Germanic **mag-wī* from Indo-European **maghos* 'youngster' (IEW. 696): Gothic *mawi*, *mawilō*, OE. *mēowle*, ON. *mǣr*, *meyla* 'girl';

Indo-European **(s)teigʷh-* (IEW. 1018), Gothic *stiwiti* 'patience';

Indo-European **tek-u̯i* 'female servant' (IEW. 1059): Gothic *þiwi*, OE. *ðēowu*, *ðēowe(n)*, OHG. *diu(wa)*, ON. *þý*, *þír*, OS. *thiwi*, *thiu* 'maid';

Indo-European **maiqʷo-* or **moiqʷo-* 'grey blue': Germanic **maigwí*, OE. *mǣw* 'mew' (Walde-Pokorny 2, 302, Zupitza 66).

Not all of these examples are certain, it is true. *Aiwiski*, with its cognates [8], is one of the stronger ones. The Middle Low German forms *eichelen*, *ēchelen* and *ēgelen* 'feel disgust' are, despite the guttural, perfectly normal descendants of **aiwilōn* [9], and although the exact device by which the Greek cognate *αἶσχος* is derived from the root is not clear [10], these minor difficulties need not be relevant to Germanic, where other, independent, deriva-

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[8] The OE. *ǣwisc* has a long *ǣ*, not the short vowel given by Seebold, ZfvSf. 81, 123.

[9] See Heinrich Schroeder, PBB. 29 (1904) 557.

[10] See A. Meillet, Les dialectes indo-européens, nouveau tirage avec une introduction nouvelle et des additions (Collection Linguistique 1, Paris 1922) 26.

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tions can be made from the root. This remains true even though Greek is the only other branch of Indo-European to attest the root. The derivation of *mawi* seems safe enough despite difficulties of detail with the exact Indo-European form[11], and the phonological development of Indo-European *-gh-* followed by *-ᵘ-* can safely be treated as identical to that of *-gᵘh-*[12]. *Stiwiti* is rather less certain, because the Germanic *-w-* may be derived from Indo-European *-ᵘ-*[13]. At the same time, the correspondences given in IEW. suggesting *-gᵘh-* are perfectly satisfactory. *Þiwi* is doubted only by Seebold[14]. Whilst, purely aesthetically, one would like to see Germanic derivatives with *-g-* attested from this root before accepting it as a certain reflex of *-gw-*, nevertheless the derivation given is quite impeccable both phonologically and semantically[15]. Finally, the umlaut in OE. *mǣw* suggests a following *i*, though another derivation is given in IEW. 751. This root is ignored by Seebold.

Hence, such evidence as there is suggests that *-gw-* becomes *-w-* before *i*. There are no counter-examples, but, at the same time, there is one difficulty which must be discussed before we can claim to have taken into account all the circumstances affecting the development of the consonant. The difficulty is that the above analysis assumes that, in these roots, the *-gw-* was invariably followed by the *i* of the normal nominative ending, or the *i* of the derivative syllable quoted. Yet the fact is that the sound following *-gw-* could vary in inflected forms or when other suffixes were used, and that the other sounds involved could cause *-gw-* to develop differently and open up the possibility of analogical interference. Thus *mawi* has *-j-* in its oblique cases in Gothic[16]. Before *j* the normal development of *-gw-*

[11] Seebold, ZfvSf. 81, 129.

[12] See Hans Reichelt, IF. 40 (1922) 41–51.

[13] See Francis A. Wood, Modern Language Notes 20 (1905) 44.

[14] ZfvSf. 81, 125, 130.

[15] See P. Persson, ZfvSf. 33 (NS. 13) (1895) 291, Felix Solmsen, ZfvSf. 34 (NS. 14) (1897) 2, Karl Brugmann, IF. 19 (1906) 381.

[16] See Wilhelm Braune, Gotische Grammatik mit Lesestücken und Wortverzeichnis (twelfth edition by Karl Helm; Sammlung kurzer Grammatiken germanischer Dialekte A 1, Halle 1947), paragraph 96, page 58.

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would be *-g-*[17]. Since the Gothic attests *-w-* throughout the declension, it is clear that analogy of some sort has taken place. In this instance the *-g-* of the oblique cases has given way to the *-w-* of the nominative. The opposite may have taken place with ON. *ylgr* 'female wolf' (IEW. 1178). The possibility of analogical reformation of words containing this instable consonant is hence a lurking difficulty which has to be faced before any form is accepted as phonologically regular.

Moreover analogy seems to be not only a theoretical possibility but actually to have affected the development of **agwi-* 'reptile'. Just like *mawi* it would be followed by a variety of sounds in its declension. This is true whichever class of nouns it belonged to, but a difficulty of detail with this word is that we are not sure of the declension to which the noun belonged. It is attested only in the compound and never on its own. A tortuous clue to its class may be offered by the Greek cognate ὄφις which is an *i*-stem[18]—a class which is obsolescent in Greek and had probably made few recent gains[19], so that this may have been the original class in Indo-European and hence in Germanic too. Sanskrit *áhi-* and Avestan *aži* also point to an *i*-stem. There is also an *-i-* in the Germanic compound, which however is not conclusive proof that we are dealing with an *i*-stem, for OHG. *herzisuht*, for example, has an *-i-* which is not present in the declension of *herz*[20]. Moreover, even if we knew the stem, it might not be relevant to the phonology of the compound: OHG. *awizoraht* etc.[21] preserve a *-w-* which is lost from the simple form Gothic *augō* etc.[22]. A further difficulty is that the exact

[17] Zupitza, Die germanischen Gutturale 72.

[18] See Eduard Schwyzer, Griechische Grammatik auf der Grundlage von Karl Brugmanns griechischer Grammatik (volume 2 ed. Albert Debrunner, vol. 3 [Register] by Demetrius J. Georgacas, 3 volumes in 4, Handbuch der Altertumswissenschaft 2,1, Munich 1934–53) I 462 n. 4.

[19] See P. Chantraine, La formation des noms en grec ancien, Collection Linguistique 38 (Paris 1933) 111.

[20] On this *-i-* in the neuter nominative singular of consonant stems see Hermann Hirt, Indogermanische Grammatik (7 volumes; Indogermanische Bibliothek 1, 1, 13, Heidelberg 1921–37) III, 102.

[21] IEW. 776/7.

[22] Forms given IEW. 776/7.

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endings of *i*-stems are not clear in all the various grammatical cases, so that the probable phonological developments are all the harder to reconstruct. This is only a difficulty of detail, however, as the point is that *-g-* and *-w-* alternated in the declension, and this seems fairly safe to assume even if errors of detail as to the actual sound in a given case are inevitable.

The likely alternation between *-g-* and *-w-* is illustrated as aptly by assuming that **agwi-* is an *i*-stem as it is by making any other assumption about its declension, and on this assumption the word would develop as follows. **Aw-* would develop before *i* on the evidence given above, and *i* occurred after *-gw-* in the nominative and accusative singular, and in the accusative, genitive and dative plural[23]. The effective ending in other grammatical cases is open to doubt. The genitive singular may have had *-j-* or *-i-* or the *-a-* of the *a*-stems[24] giving *-g-* or *-w-*, and for the most part the ending of the dative singular is analogically remodelled on the pattern of the *a*-stems[25]. At what point this analogy took place is not clear, but according to Seebold[26] a following *-e-* or *-a-* would cause *-gw-* to develop to *-g-*. The instrumental singular has an obscure history. The possibilities include a form with Indo-European *-ī̆ā*[27], giving *-g-*, but there may have been others giving *-w-*.

The nominative plural has *-ei-*[28] with unclear implications for the development of the *-gw-* (Seebold says *-g-*), though an ending *-ijiz*[29] would cause *-w-* to develop.

The development of *-gw-* in the different grammatical cases is not, therefore, a simple matter, but the important point seems

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[23] See Hermann Hirt, Handbuch des Urgermanischen (3 volumes; Indogermanische Bibliothek 1, 1, 21, Heidelberg 1931–4) II, paragraph 44, pages 43/4.

[24] Hirt, Handbuch, should be compared with Hans Krahe, Germanische Sprachwissenschaft, vol. 3 (Wortbildungslehre) by Wolfgang Meid (3 volumes, fourth edition of vol. 1, sixth edition of vol. 2, Sammlung Göschen 238, 780, 1218, 1218a, 1218b, Berlin 1960–7) II, paragraph 13, page 26.

[25] Hirt, Handbuch, II, 43/4 and II, 35; Krahe, paragraph 4, II, 9/10, and paragraph 13, II, 26.

[26] ZfvSf. 81, 132. [27] Hirt, Handbuch II, 43. [28] Hirt, Handbuch II, 43/4.

[29] Krahe II, 27.

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clear all the same, and that is that both *-g-* and *-w-* could develop in the declension, as reflexes of earlier *-gw-*. The alternation may have shown the following pattern:

Nominative singular:	**awi-*
Accusative singular:	**awi-*
Genitive singular:	**awi-* or **agj-*
Dative singular:	**agō* **agǣ* or **agai*
Instrumental singular:	**awi-* or **agj-*
Nominative plural:	**awī-*
Accusative plural:	**awi-*
Genitive plural:	**awi-*
Dative plural:	**awi-*

If the word was in another declension, it would have shown a different split between *g* and *w*, but it seems fair to say that in the declension given above one might expect the forms with *-g-* to be lost.

However, with this word the matter is not as simple as that. What seems to have been overlooked in the numerous previous discussions of related problems is that the above declension rather resembles that of two other words. They are:

> Indo-European **əkᵘā* 'water', Germanic derivative **ahwjǒ* 'watery land' (IEW. 23): OE. *īeg*, OHG. *-awa*, *-ouwa*, ON. *ey* 'watery land';
>
> Indo-European **okᵘ-* 'eye' (IEW. 775–7): Gothic *augō*, OE. *ēage*, Old Frisian *āge*, OHG. *ouga*, ON. *auga*, OS. *ōga* 'eye'.

These words too, as well as **agwi-* 'reptile', probably alternated **ag-* and **aw-* in their declensions. *Augō* (taking the Gothic form) is an *n*-stem, so that accepting Seebold's findings[30]— other people's rules would also give alternations of some description—we would expect the following development. In the nominative and accusative singular and in the nominative and accusative plural, we would expect **ag-* before *ŏ*, and in the genitive and dative plural we would expect **ag-* before *a*[31].

[30] ZfvSf. 81, 132.

[31] Krahe paragraphs 27–8, II, 45/6.

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The genitive and dative singular would, however, have *-w-* before *i* from older *e*[32], as was established above: endings in *-a-* attested in the OE. and ON. are from the *a*-stem, and hence probably late[33]. Thus the word for 'reptile' has **aw-* predominantly, with occasional **ag-*; 'eye' has **ag-* predominantly, with occasional **aw-*. The confusion between these two words might not have been very important, but it seems likely to have been very considerably increased by the development of **agwjō* 'marshy ground'. With this word we can expect **aw-* before *i* in the nominative singular, and **ag-* before *-j-* in the inflected cases[34]. Here we have a more even balance of **ag-* and **aw-*; the nominative singular is opposed to all the other cases. Hence there is clearly ample room for confusion between these words. There were three roots, each of them alternating **ag-* and **aw-* in their declension. There was obviously scope for interference not just between one grammatical case and another but also between one word and another.

The development of *-ouwe* and cognates is fairly well understood despite difficulties with the exact form of the OE.[35], but a rather important advantage of such an analysis of these three words is that it takes much of the urgency out of objections to deriving *augō* and its cognates from the root given[36]. It is true that the phonology is irregular, but this is hardly surprising with a consonant of known instability in circumstances of especially acute conflict. The influence of **auzō* 'ear' has also been suggested[37] as a reason for the development of *augō* and the two

[32] Krahe paragraphs 27–8, II, 44/5.

[33] Krahe paragraphs 27–8, II, 44, 46.

[34] For the distribution of *i* and *j* in the declension see K. R. Brooks, English and Germanic Studies 5 (1952) 34. His phonological conclusions, however, should now be revised.

[35] See A. H. Smith, The Place-names of the North Riding of Yorkshire (English Place-Name Society 5, Cambridge 1928) 61; Karl Luick, Historische Grammatik der englischen Sprache mit dem nach den hinterlassenen Aufzeichnungen ausgearbeiteten zweiten Kapitel ed. Friedrich Wild and Herbert Koziol I (Oxford and Stuttgart 1964), paragraph 243 n. 1. page 220; H. M. Chadwick, Studies in Old English. Transactions of the Cambridge Philological Society 4 (1894–9) 138–9.

[36] Objections raised by Seebold, ZfvSf. 81, 126/7.

[37] Tentatively, by Zupitza, Die germanischen Gutturale 74.

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words are certainly similar in many respects[38]. There is no reason why the two forces should not have acted in conjunction with each other. The root was not merely extant but of considerable fruitfulness in Germanic, as is shown by the existence of derivatives such as OE. *ēawis*, OHG. *awizoraht* 'clear', OE. *ēawan*, Old Frisian *āwia*, *auwia*, Middle Dutch *t-ōnen*, MHG. *zounen* 'show'[39]. It seems perverse to regard *augō* and cognates as unrelated to this fruitful root[40] when there are excellent reasons for its irregular phonological development.

This subsequent development of *augō* from its original alternation of **ag-* and **aw-* can be compared rather interestingly with that of the two competing roots. The word for 'reptile' tended to standardise the *-g-*, that for 'marsh' the *-w-*, and that for 'eye' a combination of the two sounds *-ug-*, hence *aug-*. One might add that OE. *ǣwan* 'despise' (IEW. 14) is from a phonetically similar root. There are several ways of explaining the medial consonant *-w-* for *-ga-*. It could have spread from forms with one verbal suffix to others with a different verbal suffix. The reason for the *-w-* could also be partly the influence of related non-verbal forms, such as **aiwisk-*. But also, by being associated with such forms, *ǣwan* perhaps became less easily confused with the roots in *ag-* and *aw-*.

Before concluding these observations, it should be stated that they depend to some extent on the rule that *-gwj-* develops to *-g-*, and that this rule has not always been fully appreciated. Zupitza[41] gives the rule only cautiously, and Seebold[42] gives

[38] See Altenglische Grammatik nach der angelsächsischen Grammatik von Eduard Sievers, ed. Karl Brunner (Sammlung kurzer Grammatiken germanischer Dialekte A 3, Halle 1942) paragraph 279, page 234; Wilhelm Braune, Althochdeutsche Grammatik (thirteenth edition by Hans Eggers, Sammlung kurzer Grammatiken germanischer Dialekte A 5, Tübingen 1975) paragraph 224, page 207; Adolf Noreen, Altnordische Grammatik, I. Altisländische und altnorwegische Grammatik (Laut- und Flexionslehre) unter Berücksichtigung des Urnordischen (fourth edition; Sammlung kurzer Grammatiken germanischer Dialekte, 4, Halle 1923), paragraph 405, page 279.

[39] IEW. 776.

[40] So Zupitza, Die germanischen Gutturale 73/4.

[41] Die germanischen Gutturale 72.

[42] ZfvSf. 81, 130.

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-w- and *-g-*. The following examples, however, seem to amount to a perfectly adequate proof:

Indo-European **ēkᵘi̯ós* 'water' (IEW. 23): OE. *ǣgweard* 'watch by the sea', *ēagor* 'sea', ON. *Ægir* (sea-god);

Indo-European **knei-gᵘh-*, Germanic **hneigw-*, **hnigw-jō*, (IEW. 608) 'incline': OE. *hnǣgan* 'humble', OHG. *hneicken* 'bow', *nicken* 'nod', ON. *hneigja* 'bow';

Indo-European **sokᵘi̯ós* 'follower' (IEW. 897): OE. *secg* 'man', OHG. *beinsegga* 'attendant', ON. *seggr*, OS. *segg* 'man';

Indo-European **sokᵘ-*, Germanic **sagwjǒ* (IEW. 898), 'remark': OE. *secgan*, ON. *segja*, OS. *seggian*, Middle Dutch *segghen* 'say';

Indo-European *u̯ogᵘhi̯o-* (IEW. 1179/80), OE. *wecg*, OHG. *weggi*, *wecki*, ON. *veggr* 'wedge'.

Naturally, however, not all of these examples are regarded as certain by all scholars. *Ægir* and cognates have been regarded by Brooks[43] despite Pogatscher[44] as a neuter *es/os* stem, and hence without a *-j-*. This is made less likely by the need for Verner's Law to affect both the medial *-hw-* and the final *-s*. Purely phonologically this is impossible, though of course analogy on the model of other nouns could have affected the final *-s*. The additional need to accept an 'anthropomorphic' change of gender[45] helps to diminish the appeal of this solution[46]. *Hnǣgan* and cognates seem to be a more straightforward case, though one might add in passing that OHG. *hnegên*[47] seems to be neglected

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[43] K. R. Brooks, English and Germanic Studies 5, 42/3, 56.

[44] A. Pogatscher, Englische Studien 27 (1900) 223/4.

[45] Brooks, English and Germanic Studies 5, 56.

[46] I am grateful to Professor Wolfgang P. Schmid for pointing out that the ancient name of the River Eider in Slesvig-Holstein *Egidora* also supports an assumed **ēkᵘi̯os*.

[47] See W. Schulze, ZfvSf. 46 (1914) 188/9; Wilhelm Wissmann, Nomina Postverbalia in den altgermanischen Sprachen nebst einer Voruntersuchung über deverbative *ō*-Verba, I, deverbative *ō*-Verba (Ergänzungshefte zur Zeitschrift für vergleichende Sprachforschung 11, Göttingen 1932) 64/5.

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by Seebold in his otherwise extremely valuable discussions[48] of this family of words. Gothic *hnaiwjan* (IEW. 608) seems to be an error, as no such word is attested. *Hneiwan* (IEW. 608) has a different grade of ablaut and no sign of a *-j-* and is hence not relevant. *Secg* and cognates too are almost beyond controversy[49], though Seebold[50] expresses mild if justified doubts on the grounds that the derivation rests on the assumption that the word is related to IE. **sekᵘ-* 'follow'. There are difficulties with the words for 'to say', but they are not important in this context. They concern OHG. *sagên*[51], the phonology of which is difficult. Quite apart from anything else, there should be umlaut if the German ending was *-jan-*. There may be contamination with the root attested in OHG. *dagên* 'keep quiet'[52], or derivation from **sagh-* 'show'[53], or a different ending from that attested in other dialects[54]: but in any case the regular development of *-ǥwj-* is not illustrated by this OHG. form. It is OE. *secgan* (with gemination) that illustrates the regular development of the sound combination. Finally, though they are of some value, one would not wish to place too much weight on *wecg* and cognates[55]: the final consonant may be a simple velar and the meanings of the supposed cognates are not identical to that attested in Germanic[56].

Hence the evidence that *-ǥw-* develops to *-ǥ-* before *j* is strong even though some parts of it are difficult. There is only one possible counter-example, and that is Gothic *mawī*, *maujō-*, which, however, has already been dealt with as the regular development of *-ǥw-* before *i*.

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[48] Seebold, ZfvSf. 81, 122; Elmar Seebold, Vergleichendes und etymologisches Wörterbuch der germanischen starken Verben (Janua Linguarum, Series Practica 85, The Hague and Paris 1970) 266.

[49] See Fr. Specht, ZfvSf. 66 (1939) 48.

[50] ZfvSf. 81, 128.

[51] The form with short *-e- sagen* given IEW. 898 is wrong; see Braune, Althochdeutsche Grammatik, paragraph 368.

[52] Fritz Mezger, ANF. 55 (1940) 210.

[53] See Hermann Collitz, Das schwache Präteritum und seine Vorgeschichte (Hesperia 1, Göttingen 1912) 78f.

[54] Seebold, ZfvSf. 81, 126.

[55] Accepted by Fr. Specht, ZfvSf. 66 (1939) 43.

[56] Seebold, ZfvSf. 81, 128/9.

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For the sake of completeness it should be said that after *n* *-gw-* remains whether *i*, *j* or any other sound follows[57]. Nevertheless the rule that *-gwj-* develops to *-g-* seems strong enough to support the general conclusions arrived at here. These are that the words concerned should be ignored in phonological investigations. Their circumstances of homonymic clash are unusual, and the forms in which they are eventually attested are correspondingly difficult. We cannot be sure to what extent the regular phonological development was affected by interference from other grammatical cases and from the other roots which had taken on the nature of homonyms

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[57] Seebold, ZfvSf. 81, 116.

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Der „dativus sympatheticus“ in den Werken Thomas Murners. Ein Beitrag zu der Forschung nach dem Wesen des „sympathetischen“ Dativs in den indogermanischen Sprachen

Einleitung

1) In Thomas Murners Schriften begegnen oft Fügungen wie „Er reib die fies dem man so andt“ bzw. „Das ich in mög die lenden schmieren“[1], in welchen ein Genitiv bzw. ein Possessivpronomen grammatisch auf gleicher Stufe wie der verwendete Dativ stünde[2]. Sie enthalten mithin eine Fülle an Material, anhand dessen versucht werden kann, die Art dieses Dativs zu bestimmen, zumal sie auch viele Parallelfügungen mit Genitiv bzw. Possessivpronomen und Kombinationen eines solchen Dativs mit Possessivpronomen bieten, welche zum Vergleich herangezogen werden können. Der vorliegende Aufsatz bezweckt denn auch, das Wesen des „dativus sympatheticus“ auf der Grundlage einer Vielzahl von Stellen aus obengenannten Werken[3] zu untersuchen.

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[1] Badenfahrt XVIII: 18 bzw. Narrenbeschwörung IX: 2 (über der Abbildung).

[2] Vgl. W. Havers, Untersuchungen zum Kasussystem der indogermanischen Sprachen (Straßburg 1911) 1; siehe auch „Schlußfolgerungen“ 2 + 3b.

[3] Und zwar in der von der Gesellschaft für elsässische Literatur begonnenen und von dem Wissenschaftlichen Institut der Elsaß-Lothringer im Reich übernommenen und weitergeführten kritischen Gesamtausgabe. Diese umfaßt: I 1: Von den fier ketzeren, hrsg. von E. Fuchs (Berlin/Leipzig 1929), I 2: Badenfahrt, hrsg. von V. Michels (ibid. 1927), II: Narrenbeschwörung, hrsg. von M. Spanier (ibid. 1926), III: Die Schelmenzunft, hrsg. von M. Spanier (ibid. 1925), IV: Die Mühle von Schwindelsheim und Gredt Müllerin Jahrzeit, hrsg. von G. Bebermeyer (ibid. 1923), V: Die Geuchmat, hrsg. von E. Fuchs (ibid. 1931),

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2) Der Name „dativus sympatheticus" ist von Havers geprägt worden. Weil der sogenannte „dativus commodi" in einem Satz wie „Der Vater baute dem Sohne ein Haus" nicht durch einen Genitiv — wir ergänzen: bzw. ein Possessivpronomen — ersetzbar ist, handelt es sich seiner Meinung nach bei dem mit der possessiven Ausdrucksweise (Genitiv/Possessivpronomen) im Austausch stehenden Dativ nämlich um eine besondere Art dieses Kasus. Im Anschluß an die Bemerkung Gildersleeves zu Pindar Pyth. III 46 ἀνθρώποισιν ἰᾶσθαι νόσους, nach der „ἀνθρώποισιν more sympathetic than ἀνθρώπων" sei, schlägt Havers das Epitheton „sympatheticus" vor, weil dieser Dativ die innere Anteilnahme der von dem Verbalbegriff betroffenen Person ausdrücke und subjektiver, wärmer und innerlicher sei als der Genitiv (bzw. das Possessivpronomen), der (bzw. das) einfach objektiv ein Besitzverhältnis konstatiere[4].

Behaghel weist darauf hin, daß bei Verwendung dieses Dativs zwischen der dativischen Größe und anderen mit der Verbalhandlung verbundenen Faktoren schon vor Vollzug der Handlung eine Beziehung besteht, weil die zweite Größe einen Bestandteil, einen Zustand, ein Besitztum der im Dativ ausgedrückten Größe bezeichnet (die Benennung hält er aber für nicht besonders glücklich, weil die Hauptsache, die Verknüpfung der beiden Größen, nicht zum Ausdruck komme)[5]. Untenstehende Ausführungen werden zeigen, inwiefern auch wir in dieser Verknüpfung die Hauptsache sehen.

Zu Havers' Methodik sei hier bemerkt, daß diese uns nicht genügend durchdacht zu sein scheint: aufgrund der prinzipiellen Austauschbarkeit des in Rede stehenden Dativs gegen einen Genitiv bzw. ein Possessivpronomen und der durchaus zutreffenden Bemerkung Gildersleeves darf unserer Ansicht nach nicht ohne weiteres angenommen werden, daß hier ein besonderer Dativ

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VI–VIII: Kleine Schriften (Prosaschriften gegen die Reformation), hrsg. von W. Pfeiffer-Belli (ibid. 1927 bzw. 1928 bzw. 1928), IX: Von dem großen Lutherischen Narren, hrsg. von P. Merker (Straßburg 1918). Diese Werke werden hier folgendergestalt abgekürzt: FK, BF, NB, SZ, MS, GM, KS, I, KS II, KS III, LN.

[4] Vgl. Havers, a.a.O. 2.

[5] O. Behaghel, Deutsche Syntax I (Heidelberg 1923) 633.

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vorläge, welche dann als „sympathetisch" bezeichnet werden müßte.

Überdies muß zu Havers Bemerkungen hinsichtlich der Verwendung des possessiven Dativs durch Homer Stellung genommen werden, weil diese u. E. zu der Interferenz des possessiven Dativs in den Bereich des „sympathetischen" Dativs beigetragen haben. Er verfährt nämlich nicht ganz konsequent: einerseits ist Günthers Benennung dieses Dativs bei Homer als „dativus possessivus" seiner Meinung nach recht unglücklich[6], andererseits sieht er in Stellen wie Homer A 150 πῶς τίς τοι πρόφρων ἔπεσιν πείθηται Ἀχαιῶν eine Quelle für die Entstehung der possessiven Genitive (sic!) μοι, τοι, οἱ: die eigentliche Übersetzung wäre: Wie wird einer von den Achäern willig dir, den (deinen) Worten gehorchen; da das später hätte empfunden werden können als „Wie wird deinen Worten einer gehorchen", hätte diese Art des dativischen σχήμα καθ' ὅλον καὶ μέρος mit zu diesen von Havers als possessive Genitive bezeichneten Formen der Personalpronomina der 1., 2. und 3. Person geführt[7]. Die von Havers gegebenen Beispiele[8] werden unserer Ansicht nach zu summarisch wiedergegeben, weshalb wir sie hier im Satzzusammenhang folgen lassen.

N 616f. τὼ δέ οἱ ὄσσε πὰρ ποσὶν αἱματόεντα χαμαὶ πέσον ἐν κονίῃσιν (seine beiden Augen fielen blutbedeckt neben seine Füße zu Boden in den Staub, oder: ihm fielen (die) beide(n) Augen blutbedeckt neben die Füße zu Boden in den Staub);

Ξ 438 τὼ δέ οἱ ὄσσε νὺξ ἐκάλυψε μέλαινα, βέλος δ' ἔτι θυμὸν ἐδάμνα (die dunkle Nacht bedeckte seine beiden Augen/ihm beide Augen, und das Schwert lähmte überdies sein Bewußtsein);

O 607f. τὼ δέ οἱ ὄσσε λαμπέσθην βλοσυρῇσιν ὑπ' ὀφρύσιν (seine beiden Augen funkelten unter den buschigen Augenbrauen, oder: ihm funkelten (die) beide(n) Augen usw.);

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[6] A.a.O. 1; trotz unserer Bedenken gegen den Terminus „sympathetischer Dativ" und gegen die Kasusbezeichnungen lateinischen Ursprung verwenden wir diese, da sie sich eingebürgert haben, in unserem Aufsatz, bis wir im abschließenden Teil unserer Ausführungen die diesbezüglichen Termini einer u. E. erforderlichen Revision unterziehen.

[7] A.a.O. 70f. [8] A.a.O. 75.

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P 695f. = δ 704f. = τ 471f. τὼ δέ οἱ ὄσσε δακρυόφι πλῆσθεν, θαλερὴ δέ οἱ ἔσχετο φωνή (seine beiden Augen füllten sich mit Tränen, und seine kräftige Stimme versagte, oder: ihm füllten sich (die) beide(n) Augen mit Tränen, und die kräftige Stimme versagte ihm);

T 365f. τὼ δέ οἱ ὄσσε λαμπέσθην ὡς εἴ τε πυρος σέλας (seine beiden Augen funkelten wie die Glut des/eines Feuers, oder: ihm funkelten beide Augen usw.);

B 217f. φολκὸς ἔην, χωλὸς δ' ἕτερον πόδα· τὼ δέ οἱ ὤμω κυρτώ, ἐπὶ στῆθος συνοχωκότε (er war krummbeinig, lahm auf einem Fuße; seine beiden Schultern krumm, gegen die Brust gekrümmt);

P 231f. ἥμισυ δ' αὐτος ἔξω ἐγώ· τὸ δέ οἱ κλέος ἔσσεται, ὅσσον ἐμοί περ (die Hälfte werde ich selber behalten; sein Ruhm wird sein so groß wie der meine, oder: ihm wird der Ruhm so groß sein wie mir);

Ψ 392f. αἱ δέ οἱ ἵπποι ἀμφὶς ὁδοῦ δραμέτην, ῥυμὸς δ' ἐπὶ γαῖαν ἐλύσθη (seine Stuten rannten auf beiden Seiten des Weges, die Deichsel fiel losgelöst zur Erde, oder: ihm rannten (die) beide(n) Stuten usw.). Obige Stellen enthalten also fast alle einen Dativ, den man als „sympathetisch" betrachten könnte (A 200 δεινὼ δέ οἱ ὄσσε φάανθεν faßt Havers selbst als einen Satz mit „sympathetischem Dativ auf[9]), es sei denn, daß man die Interposition, die Position zwischen Artikel und Substantiv, als eindeutiges Indiz für die attributive Verwendung dieses οἱ betrachtet. Sogar wenn man interpositionellem οἱ den „sympathetischen" Wert abspricht, könnte man für dieses οἱ und das B 217 ohne Verb begegnende οἱ die Möglichkeit erwägen, daß hier Ellipse eines besitzanzeigenden Verbes vorliegt: man muß dann im Anschluß an Kühner-Gerth den possessiven Dativ beschränken auf Fälle, wo ἐστί(ν)/εἰσι(ν) bei der im Dativ begegnenden Person/persönlich gedachten Sache steht, die etwas hat, und auf Fälle, wo Verben wie γίγνεσθαι (zuteil werden), ὑπάρχειν (vorhanden sein), φῦναι/πεφυκέναι, μένειν mit dem Dativ des Besitzers verbunden werden[10] — es würde sich dann um Ellipse von einem dieser Verben handeln, folglich nicht um einen reinen possessiven Dativ.

[9] A.a.O. 78.

[10] Vgl. R. Kühner-B. Gerth, Ausführliche Grammatik der griechischen Sprache II 1 (Hannover/Leipzig 1898) 416.

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Im Anschluß an Thumb vermutet Havers, daß Chariton in Nachahmung der Stelle bei Xenophon Cyr. VI 4,3 ἐλείβετο δὲ αὐτῃ τά δάκρυα κατὰ τῶν παρειῶν „ἐλείβετο δὲ αὐτῆς τά δάκρυα κατὰ τῶν παρειῶν" (II 5,7) schreibe, weil die vor dem Regens stehenden und einen älteren „sympathetischen" Dativ vertretenden pronominalen Genitive in späterer Zeit dativisch empfunden worden wären[11] — mit solchen grenzen-verwischenden Hypothesen begibt man sich aber auf das Gebiet der Spekulation!

Im Latein der Umgangssprache kam die Idee der inneren Beteiligung, wie Havers bemerkt[12], in einer Weise zur Geltung, wie kaum in irgendeiner anderen indogermanischen Sprache, und zwar durch den Gebrauch des „sympathetischen" Dativs; Cicero verwendet den „dativus possessivus" (vom Typus „mihi domus est") und den „dativus sympatheticus" als echt volkstümliche Konstruktionen nur aus stilistischen Rücksichten[13], und die Dichter der augusteischen Zeit bevorzugen im allgemeinen den ausdrucksvollen „sympathetischen" Dativ vor der possessiven Ausdrucksweise[14].

In den auf die lateinische Volkssprache zurückgehenden romanischen Sprachen ist der „sympathetische" Dativ auch heute noch ganz geläufig[15].

3) Erben spricht von dem „possessiven" Dativ, der meistens als Bezeichnung einer Person neben einem Substantiv steht, das einen Körperteil oder ein Zubehör des im Dativ Genannten ausdrückt. Der besondere Status solcher Glieder wird seiner Meinung nach mit dem Ausdruck „bedingte Glieder" am angemessensten beschrieben; da sie nicht gänzlich beliebig, sondern nur mit bestimmten Verben kombiniert werden können, scheint es s. E. sicher, daß sie beim Ermessen der syntaktischen Wertigkeit in Rechnung gestellt werden müssen[16].

4) Nach von Polenz gehört der Dativ in einem Satz wie „Er klopfte seinem Freund auf die Schulter" hingegen nicht zur Va-

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[11] A.a.O. 169. [12] A.a.O. 170ff. [13] Vgl. Havers, a.a.O. 212f.

[14] Vgl. Havers, a.a.O. 219.

[15] Vgl. Havers, a.a.O. 232; siehe für das Französische und das Spanische untenstehende Erörterungen.

[16] Vgl. J. Erben, Deutsche Grammatik. Ein Abriß [11](München 1972) 253f.

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lenz des Prädikatverbs: es besteht seiner Meinung nach zwischen den zwei Nominalphrasen eine semantische Relation „x = Körperteil von y" (von Isačenko „Pertinenzrelation" genannt), welche Relation von Polenz nicht auf Körperteile beschränkt: die Relation ist in seiner Sicht ein „Zugehören", sei es als „Teil", sei es als „Besitz"[17].

Untersuchungsergebnisse mit Bezug auf den Gebrauch des „sympathetischen" Dativs durch Thomas Murner

A. Ein Pronomen weist den „sympathetischen" Dativ auf

I. In der Fügung wird kein Possessivpronomen verwendet, sondern (für gewöhnlich) ein Artikel

1) Die Fügung enthält ein transitives Verb, z. B.: BF XVIII: 2: *So er die fies im bad mir ribdt,* BF XXVIII: 58: *Das es dir reiniget lib vnd mût;* NB XLII: 19f.: *Wer ein pfrûndt hat zû verlyhen, Dem mûß man vor den seckel wyhen,* NB LXX: 56f.: *Der tûchman kan syn huß verblenden, Das im das liecht kein tücher schenden.*

2) Die Fügung enthält ein intransitives Verb, z.B.: FK 2032: *Das im die lenden müssen krachen*; BF XVI: 41: *Das im ein herli felt nit hin,* BF XVIII: 42f.: *Vnd dedt mit kelten im so we Das im der kitzel gar verging*; NB LXV: 47: *Biß ir das hembd am arß verbrandt*; SZ XX: 17–19: *Die ersten fragen, die man dût, Die ist: wie fill sy hab des güt, Vnd ob ir sey der seckel schwere.*

3) Erst durch Verwendung eines zusätzlichen Adverbiale ist der Gebrauch des Verbs in dieser Bedeutung mit dem „sympathetischen" Dativ möglich, z.B.: FK 3198: *Stiesß jm das sacrament in hals;* BF II: 58f.: *Das im durch beide backen ran Das wasser vnd die heißen trehen;* NB XXI: 1: *Sitzt dir der tüfel vff der zungen,* NB XXVI: 45–47:

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[17] Vgl. P. von Polenz, Der Pertinenzdativ und seine Satzbaupläne (Festschrift für Hugo Moser, Düsseldorf 1969), 149ff.

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Vnzucht ist worden also groß, Daß sy sich zieren alle bloß, Man sicht in mitten uff den rücken, NB LXXXIII: 51f.: *Sy sagen, das vff gablen rytten; Ach, steckent sy in in der sytten!;* MS 684f.: *so sollendt ir den stil vß rauffen Vnd ir den vber die lenden messen,* MS 1470f.: *Jch hab wol manchen also troffen, das er mir vß dem landt ist gloffen;* GM 369f.: *Das sy ein messer zuckt mit schmertz Vnd stach ir selber durch das hertz,* GM 2185: *Noch lag mir Thays stets im synn,* GM 5021f.: *Do Sysara dem wyb geloubt, Schlûg sy ein nagel jm yns houpt;* KS II: 41: 26–28: *vnd darum nem er vil tusent guldin von euch/vnd suge euch vnd der gantzen tütschen nation das marck vß den beinen;* LN 809: *Ich wolt mir ee in die zungen beissen,* LN 1185: *Der dunder schlag inen in den sack.*

II. In der Fügung wird neben dem „sympathetischen" Dativ ein Possessivpronomen verwendet

1) Die Fügung enthält ein transitives Verb, z.B.: FK 2035f.: *Der böβwicht schrey so ketzerlich, Do ich sein rechte hand jm zuckt,* FK 3190f.: *Vnd wolten das dem brûder geben, Abbrechen jm do mit sein leben,* FK 3230f.: *Bald trachten sye ein andren fundt, Wie sye jm schweigten seinen mundt,* FK 3449: *Do streckt man jm all seine glider;* BF III: 33: *Wen im sin hertz got ane zünt,* BF XVIII: 16: *Der im so iuckte sine fies;* NB VII: 82: *Wann wir syn geiß im werffen nider,* NB XII: 42: *Wen du im schendst syn gantz geschlecht,* NB LXIV: 5–8: *Man mûß sich zu der herrschaft neigen Ernstlich, dapffer, gentzlich eigen, Vor dem tisch ston, gnippen gnappen, Vnd stryflen im syn narren kappen;* GM 2333f.: *Wie wol er ir hat vmb gebracht Jren brûder vnd ir fründt;* LN 875f.: *Das er weihwasser schlucket yn, Mächt im sein euglin lauter vnd fein.*

2) Die Fügung enthält ein intransitives Verb, z. B.: NB X: 92: *Vnd blybendt im syn oren groß;* MS 1091: *Dar von in schwindlet denn ir haupt;* LN 66: *Ja brech euch euwer hertz daruon.*

3) Erst durch Verwendung eines zusätzlichen Adverbiale ist der Gebrauch des Verbs in dieser Bedeutung mit dem „sympathetischen" Dativ möglich, z. B.: FK 773f.: *Weywasser/kertzly/begynen tandt, Das hieng er jm schon zû seiner wandt,* FK 2491: *Das blût jm vß seim schenckel floß,*

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FK 3169–3171: *Der brůder sprach/ „nun ist es wor, Das mich die bößwicht tödten wellen Vnd mir auch vff mein leben stellen,* FK 3232f.: *Vmb seinen leib gantz nackent bloß Bunden sye jm ein ketten groß,* FK 3520: *Jch stach jm in sein schenckel dar* (vgl. FK über 2393: *Wie der Prior zům andren mol dem brůder erscheine . . . vnd der brůder jn mit einem brotmesser in sein bein stach*); BF II: 15f.: *Fieng er das wasser schöpffen an, Das es im durch sin augen ran*; NB XLV über dem Bild: *Der selb find ein verborgen gwin, Der im nie kam in synen sin;* SZ IL: die entschuldigung des zunfftmeysters: 24–26: *so ir nun das vil besser kindt Vnd gryfft mir in myn ampt hin yn, so wil ich nym zunfftmeister syn;* LN 687: *Sie sitzen mir in meinem haupt.*

B. Ein Substantiv weist den „sympathetischen" Dativ auf

I. In der Fügung wird kein Possessivpronomen verwendet, sondern (für gewöhnlich) ein Artikel

1) Die Fügung enthält ein transitives Verb, z.B.: BF IX: 6f.: *Das got die füs geweschen hat Einem menschen hie vff erden,* BF XVIII: 18: *Er reib die fies dem man so andt;* NB V: 118–120: *Ewer gröste wyßheit ist, Wie ir gredt müllerin gryfft die brüst Vnd den hünern die schwentz uffbinden,* NB XIII: 62f.: *Die offenlich im frowen huß Mym sack ryssendt den boden uß* (vgl. NB XXV: 54: *Dem sack, dem ist der boden uß*), NB XVIII: 91f.: *Füll der flesch den punten zů Gang hin schlaffen, hab dyn rů,* NB XCV: 100f.: *Jch lůg, by got, wie ich im dieg, Das ich dem pfaffen die blat zerschlieg;* GM 3195: *Wie man besicht die pfyn dem schwyn,* GM 3226f.: *Der bseher můß sich dar noch flissen, Das mul dem gouch bald vff zůrissen.*

2) Erst durch Verwendung eines zusätzlichen Adverbiale ist der Gebrauch des Verbs in dieser Bedeutung mit dem „sympathetischen" Dativ möglich, z.B.: NB XVIII über dem Bild 1f.: *Kumpt, lieben guattren, sindt gebetten, Der fleschen vff den riemen zů tretten,* NB XIX: 40–42: *Wa findt man yetz ein solchen man, Der mit ernst dörfft widerstan? Dem bischoff in den barte gryffen?;* LN über 572: *was narren dem grosen narren in dem haupt sitzen/vnd im fast we thůn.*

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II. In der Fügung wird neben dem „sympathetischen" Dativ ein Possessivpronomen verwendet

1) Die Fügung enthält ein transitives Verb, z.B.: NB XVII: 13: *Der ganß berupfft man iren kragen,* NB XXXIV: 36–38: *Jst es nit ein cläglich ding, Das man so grossen kosten hat, Wie man den lüsen wört ir statt?,* NB XLI: 41f.: *Die ire töchtern lerent sünden, Den hünern ire schwentz vff binden;* MS 183: *dem mann entzündts do mit syn blüt;* GM 525f.: *Vnd wolt der künigin ir rich Also vßspehen heymelich,* GM 586–589: *Myn schöne dochter Circe hatt Von mir entpfangen den gewalt, Das sy den mannen ir gestalt Verendren kan,* GM 5028f.: *So můst Anchises über mer, Das er frouw Venus schwecht ir eer.*

2) Erst durch Verwendung eines zusätzlichen Adverbiale ist der Gebrauch des Verbs in dieser Bedeutung mit dem „sympathetischen" Dativ möglich, z.B.: GM 746f.: *Sy sindt so frumb vnd ouch so byder, Eym gouch gond sy durch all syn glyder;* KS III: 35: 18–20: *es wer mir auch in mein gedanck nie kumen das die stifel ein verdrus solten daran gehabt haben.*

Im Anschluß an obige Belegstellen sei noch folgendes bemerkt.

1) Es begegnen viele mit den zahlreichen „sympathetischen" Dativen konkurrierende Konstruktionen; hier werden nur einige Beispiele erwähnt:

LN 201: *Da ward ersterckt mein hertz vnd gemüt;* BF IV: 62f.: *Darumb got in beweis genad Vnd wůsch ir houpt in disem bad:* das Subjekt/Akkusativobjekt enthält ein Possessivpronomen;

KS III: 13: 17f.: *sol er darumb seiner eeren nit beraubet sein:* im Genitivobjekt steht ein Possessivpronomen;

FK 1107f.: *Graulich warff er vff ir schlaffhuß Ein lucern/die er auch brach gar:* das Adverbiale enthält ein Possessivpronomen;

FK 1124f.: *Marie eer wend sye anliegen* (= *zusetzen*) *Mit irem zancken vnd auch kriegen:* das Possessivverhältnis wird mittels eines Genitivs zum Ausdruck gebracht (man könnte „Marie" eventuell als Dativ auffassen).

2) Manche Dative könnte man auf keinen Fall als „sympathetisch" auffassen, z.B.: BF VII: 54–56: *Vnd niemans ist der solchs bedracht, Das im zů notturfft ward gemacht Die kley-*

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dung/vnd zů schalckheyt nit; NB I: 39f.: *Doctor brandt hat manchem man Die narren schellen knipffet an.* Bei anderen ist Zweifel möglich: NB LXV: 39f.: *So fragt er, wa syn hembd sy kummen, Wer im die hosen hab genummen*; FK 275–278: *Wie das sye keiser Friderich Nit haben also lästerlich Jn dem sacrament vergeben Vnd abbrochen hand sein leben:* „vergeben" regiert bei Murner den Dativ, es könnte somit „sympathetischer" Dativ des Substantivs mit Possessivpronomen enthaltendem Akkusativobjekt vorliegen.

3) Auch in nächster Nähe alternieren konkurrierende Fügungen, z.B.: SZ IX: 15–20: *Jch scheiß meyns herren findt ins můß Vnd dratt im willig vff den fůß, Warff im zů nacht seyn fenster eyn Vnd ließ im louffen auß den weyn, Jch streich im an seyn hossen dreck Vnd leit im heimlich steyn an wegk;* LN 702–707: *IN meiner deschen sitzen narren, Die vff gůt vnd gelt da harren. Das sein besunderliche knaben, Die gern ein sackman wolten haben, Ir hend in frembden gütern weschen, Die sitzen mir hie in der deschen.*

Schlußfolgerungen

1) Wie oben gezeigt[18], spricht Erben von dem „possessiven" Dativ, der eine Person bezeichne, neben der ein Körperteil oder ein Zubehör zum Ausdruck gebracht werde. Der „possessive" Funktionswert dieses Dativs ist seiner Meinung nach ohne Zweifel aufgedeckt![19]

Ähnlich verfährt C. F. A. van Dam mit Bezug auf die spanische Sprache. Er weist darauf hin, daß man im Spanischen für gewöhnlich den bestimmten Artikel, nicht das Possessivpronomen verwendet, wenn es sich um Körperteile oder Kleidungsstücke handelt; der Begriff des Besitzes wird dann seiner Ansicht nach durch den Dativ des Personalpronomens/Reflexivpronomens ausgedrückt, z.B. in „me duele la cabeza" (mir tut der Kopf weh), „se mete las manos en los bolsillos del pantalón" (er steckt die Hände in die Taschen der Hosen), „todo el mundo le alabó mucho el libro" (jedermann pries ihm das Buch sehr/sein Buch

[18] Vgl. „Einleitung" unter 3.
[19] Vgl. Erben, a.a.O. 253.

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sehr)[20]. In diesem Zusammenhang ist zu beachten, daß schon Erdmann-Mensing bemerkt haben, daß auch wenn ein der Person zugehöriger Gegenstand (insbesondere ein Körperteil) von einem possessiven Pronomen begleitet ist, der freie Dativ der beteiligten Person — wie oben dargelegt, von Havers als „dativus sympatheticus" bezeichnet — oft vorkommt[21].

Wenn Starke den Gebrauch von Dativ und Possessivpronomen in einem Satz wie „Dem Mann zittern seine Hände" als eine pleonastische Ausdrucksweise bezeichnet[22], so müssen wir diese Meinung denn auch als nicht genügend begründet ablehnen, zumal Starke selbst erwähnt, daß in seiner Materialsammlung Belege für eine solche „pleonastische" Wendung fehlen[23].

Obenstehende Belege für den Gebrauch von „sympathetischem" Dativ in einer Fügung mit Possessivpronomen, welche noch um viele Stellen vermehrt werden könnten, machen es unwahrscheinlich, daß man es da mit Pleonasmen zu tun hätte. Es liegt viel mehr auf der Hand, anzunehmen, daß das Possessivpronomen quatale das Besitzverhältnis zum Ausdruck bringt; wird kein Possessivpronomen verwendet, so wird die Bezeichnung dieses Verhältnisses augenscheinlich als überflüssig empfunden, weil es sich zur Genüge aus der Situation bzw. dem Kontext ergibt.

Ein Vergleich der Verwendung des Possessivpronomens im Deutschen mit dem Gebrauch dieses Pronomens im Niederländischen kann in diesem Zusammenhang verdeutlichend wirken. Wie J. van Dam mit Recht bemerkt, wird das Possessivpronomen in dieser Sprache bei selbstverständlicher Zugehörigkeit eines Substantivs zu einem persönlichen Subjekt im allgemeinen häufiger gebraucht als in jener Sprache[24] (vgl. z.B.

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[20] Vgl. C. F. A. van Dam, Spaanse Spraakkunst [2](Zutphen 1958) 109.

[21] Vgl. O. Erdmann-O. Mensing, Grundzüge der deutschen Syntax nach ihrer geschichtlichen Entwicklung dargestellt II (Stuttgart 1898) 256.

[22] Vgl. G. Starke, Konkurrierende syntaktische Konstruktionen in der deutschen Sprache der Gegenwart (in: Zeitschrift für Phonetik, Sprachwissenschaft und Kommunikationsforschung 23 (1970)) 60.

[23] A.a.O. 60; von ihm allerdings als Doppelausdruck der Pertinenzrelation aufgefaßt.

[24] Vgl. J. van Dam, Handbuch der deutschen Sprache II [4](Groningen 1963) 234f.

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„ik had uit *mijn* vel kunnen springen" und „ich hätte aus *der* Haut fahren können").

Dies läßt sich unserer Meinung nach auch bei der Verwendung des sogenannten „sympathetischen" Dativs feststellen: wenn bei solchen Dativen im Niederländischen relativ öfter ein Possessivpronomen begegnet als im Deutschen (vgl. z.B. „iemand op *zijn* ziel geven" und „einem das Fell gerben"), so kann man solche durchaus „hochsprachlichen" Fügungen des Niederländischen keineswegs als Pleonasmen betrachten; dasselbe trifft für die entsprechenden Fügungen bei Thomas Murner zu. Mit anderen Worten: in den hier zur Sprache kommenden Konstruktionen bezeichnet das Possessivpronomen — falls gebraucht — das Besitzverhältnis; wird es nicht verwendet, so wird dieses Verhältnis als durch Situation/Kontext zur Genüge zum Ausdruck gebracht, als implizite empfunden.

Es sei mit Nachdruck bemerkt, daß sich so bloß der Ursprung der Verwendung des Possessivpronomens in diesen Fügungen erklären läßt, es folglich die von Erben bzw. C. F. A. van Dam mit Bezug auf das Deutsche bzw. das Spanische behauptete „possessive" Funktion des „sympathetischen" Dativs[25] gar nicht gibt — der Mangel an festen Regeln bei dem Gebrauch des Possessivpronomens in derartigen Konstruktionen in Murners Schriften[26] und in der niederländischen Gegenwartssprache zeigt, daß sich der Schreiber/Sprecher da keiner strengen Verwendungskriterien bewußt ist.

Daß auch in dem heutigen Französisch dem „sympathetischen" Dativ die Funktion der Besitzbezeichnung abgesprochen werden muß, ergibt sich z.B. aus folgender Stelle in „Les Mémoires de Maigret" von G. Simenon: „Quand je pus me retourner, l'inspecteur Dufour, assis sur le plancher, avait un des pieds de l'homme dans ses mains, et on aurait juré qu'il le *lui* avait tourné au moins deux fois. Menottes! commanda-t-il": der Dativ bringt nur zum Ausdruck, daß die Handlung dem Ver-

[25] Was die obenerwähnten spanischen Beispiele betrifft: wem sonst als mir sollte der Kopf weh tun?, in wessen Hosentaschen sollte er sonst die Hände stecken als in die eigenen?, wem sonst sollte man das verfaßte Buch loben als ihm (dem Verfasser)?

[26] Vgl. obige Beispiele.

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brecher gilt, ihn zur Übergabe zwingen muß — der Besitz ist ja schon durch „un des pieds de l'homme" hinlänglich zu erkennen gegeben[27].

Aufgrund obiger Ausführungen ist es klar, daß wir auch die Bezeichnung einer Pertinenzrelation durch den „sympathetischen" Dativ[28] ablehnen.

Wenn der von Erben[29] erwähnte Satz folgendermaßen geändert wird: „Ich habe sie auf den Mund geküßt", so wären daneben die Sätze „Ich habe ihr den Mund geküßt" und „Ich habe ihren Mund geküßt" möglich, wobei man im Anschluß an von Weiß[30] bedenken muß, daß es keine vollkommen synonymen syntaktischen Konstruktionen gibt. Würde man den Dativ in dem zweiten Satz als possessiv bzw. pertinenzbezeichnend auffassen, so könnte der Akkusativ im ersten Satz konsequenterweise denselben Status beanspruchen; wenn man, umgekehrt, den Akkusativ im ersten Satz bloß als solchen betrachtet, ohne Berücksichtigung eventueller possessiver/pertinenzbezeichnender Qualitäten, so leuchtet nicht ein, aus welchem Grunde man den Dativ im zweiten Satz anders beurteilen sollte.

Der komplexe Charakter des „sympathetischen" Dativs ist unserer Ansicht nach folgenden Umständen zuzuschreiben:

a) Es gibt Fälle, wo er sich dem „dativus commodi"/„dativus incommodi" anschließt[31];

b) Man kann ihn aber nicht, wie dies Sommer macht, prinzipiell als freien Dativ betrachten[32], zumal die Grenze zwischen dem notwendigen und dem nicht-notwendigen Dativ oft fließend ist[33];

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[27] Zitiert nach B. M. C. F. Dijkzeul, Vous comprenez? [10](Apeldoorn 1977) 24.

[28] Vgl. „Einleitung" unter 4.

[29] A.a.O. 253.

[30] Vgl. A. v. Weiß, Zur Frage der Parallelkonstruktionen (in: Beiträge z. Gesch. d. dt. Sprache u. Lit. 75 (1953)) 451.

[31] Vgl. Behaghel, a.a.O. 633.

[32] Vgl. F. Sommer, Vergleichende Syntax der Schulsprachen (Deutsch, Englisch, Französisch, Griechisch, Lateinisch) mit besonderer Berücksichtigung des Deutschen [4](Darmstadt 1959) 30.

[33] Vgl. W. Admoni, Der deutsche Sprachbau [3](München 1970) 119.

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c) Es begegnen viele Verben, die durch Verwendung eines zusätzlichen Adverbiale mit dem „dativus sympatheticus" gebraucht werden können[34].

3a) Wenn versucht werden soll, die „sympathetischen" Dative (bei denen also eine bestimmte Zugehörigkeitsbeziehung zwischen einem Akkusativobjekt/Subjekt (mit oder ohne Adverbiale) und einer dativischen Größe situationell oder kontextuell gegeben ist) gegen die „anderen" Dative abzugrenzen, so ist eine generelle Charakteristik des Dativs unumgänglich.

Nach Admoni bedeutet der Akkusativ, situationsfern und kontextfrei betrachtet, den unmittelbaren Gegenstand der Handlung, der Dativ, auf dieselbe Weise betrachtet, den Gegenstand, dem die Handlung zustrebt, ohne ihn unmittelbar zu erfassen; der präpositionslose Dativ bezeichnet, wie er bemerkt, sehr oft die Person, wogegen für den Akkusativ die Bedeutung der Sache charakteristisch ist[35]. Deutschbein weist darauf hin, daß der Dativ innere Bezeihungen zwischen den in Betracht kommenden Personen zur Voraussetzung hat[36], Brinkmann nennt die dativische Größe die „sinngebende Person" und kennzeichnet die Beziehung zwischen verbalem Prozeß und Dativ als die der „Finalität"[37], Erben qualifiziert die dativische Größe als „Zuwendgröße"[38].

Im Zusammenhang mit der Abgrenzung des Dativs gegen den Akkusativ sei überdies auf folgendes hingewiesen:

α) In manchen Sprachen gibt es keinen erkenntlichen Unterschied zwischen Dativ und Akkusativ, vgl. im Englischen „I gave the man/him a book" und „I saw the man/him", im Niederländischen „Ik gaf de man/hem een boek" und „Ik zag de man/hem";

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[34] Vgl. für Fälle, die zu a, b, c gerechnet werden müssen, obenstehende Belegstellen aus den Werken Thomas Murners.

[35] A.a.O. 116.

[36] Vgl. M. Deutschbein, System der neuenglischen Syntax (Cöthen 1917) 276.

[37] Vgl. H. Brinkmann, Die deutsche Sprache. Gestalt und Leistung [2](Düsseldorf 1971) 435.

[38] A.a.O. 146.

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β) Im Spanischen steht vor dem Akkusativ, wenn dieser eine bestimmte Person bezeichnet, die Präposition a, z.B. „mi hermano admira a este héroe" (mein Bruder bewundert diesen Helden) — wie Keniston bemerkt, dient dieses a als spezielles Indiz dafür, daß Personen- und Eigennamen, also die Kategorien, die häufig als Subjekt begegnen, in casu als Objekte verwendet werden[39]. Die sehr freie Wortfolge im Spanischen[40] — man hat z.B. die volle Freiheit, das Akkusativobjekt an den Anfang des Satzes zu stellen — macht ein solches Indiz tatsächlich unentbehrlich; die Verwendung dieser Präposition führt allerdings dazu, daß die Form des „complemento directo" und die des „complemento indirecto", wenn es sich um eine Person handelt, völlig identisch sind. Mit anderen Worten: Damit sich das Akkusativobjekt deutlich von dem Subjekt abhebt, wird dann die Grenze zwischen Akkusativobjekt und Dativobjekt verwischt.

Aufgrund obenerwähnter Ausführungen kann man die Opposition zwischen Dativ und Akkusativ in der deutschen Hochsprache (wo es keine Tendenz zur Einebnung des Unterschiedes zwischen Dativ und Akkusativ gibt und der Dativ seine starke Stellung behauptet[41]) als dahingehend betrachten, daß dieser sich in großen Zügen als der Kasus des von der Handlung betroffenen Objektes, jener als der Kasus der in die Handlung mit hineingezogenen Person umreißen läßt — von Weiß' Bezeichnung des Dativobjektes als „Gegensubjekt"[42] mißt der dativischen Größe unseres Erachtens eine zu aktive Rolle bei (vgl. die niederländische Benennung „meewerkend voorwerp" = „mitwirkendes Objekt").

3b) Was den „sympathetischen" Dativ betrifft sei folgendes bemerkt. Sommer vergleicht die Sätze „Er zerschmetterte der Schlange den Kopf" und „Er zerschmetterte den Kopf der

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[39] Vgl. H. Keniston, Spanish Syntax List. A statistical study of grammatical usage in contemporary spanish prose on the basis of range and frequency (New York 1964) 29.

[40] Vgl. C. F. A. van Dam, a.a.O. 293ff.

[41] Vgl. I. Dal, Entwicklungstendenzen im germanischen Kasussystem (in: Studia Germanica Gandensia II, 1960) 132.

[42] A.a.O. 471.

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Schlange" und sieht den Unterschied darin, daß in jenem Satz das Substantiv als subjektiv beteiligt in die Sphäre des Verbalvorgangs mit hineingezogen wird, während der Genitiv in diesem Satz nur ein Besitzverhältnis konstatiert[43]. Brinkmann verficht eine ähnliche Auffassung, wenn er darauf hinweist, daß in dem Satz „Er schüttelte dem Freund die Hand" die Handlung dem Freund gilt — die Gebärde ergreift zwar nur einen Teil des anderen (die Hand), meint jedoch nicht die Hand, sondern den ganzen Menschen[44]. Nach von Weiß bilden der Verbalbegriff und der Objektbegriff (inklusive des Präpositionalobjektes) bei der Verwendung eines „sympathetischen" Dativs gleichsam eine neue Bedeutungseinheit, in der beide Teile in reziproker Beziehung stehen[45]. Mit Bezug auf die Alternative zwischen Dativ und Akkusativ zur Bezeichnung des betroffenen Lebewesens, wenn ein adpräpositionaler Akkusativ den getroffenen Körperteil andeutet, spricht Starke von der alternativkombinatorischen Fügungspotenz der betreffenden Verben; angesichts der Tatsache, daß der gleiche Autor beide Kasus bei der Darstellung des gleichen Sachverhalts verwenden kann, dürfte seiner Meinung nach diese Alternative schwerlich als „fundamentaler Unterschied" in der „Leistung" zu interpretieren sein (Man vergleiche hierzu Thomas Murner FK über 2393: *vnd der brůder jn mit einem brotmesser in sein bein stach* und GM 369f.: *Das sy ein messer zuckt mit schmertz Vnd stach ir selber durch das hertz;* GM 2198–2201: *Jch kundt nie wyssen, brest mir was, Biß ietz der Cantzler sagt mir das, Das ich vormalß nie mochte wyssen: Der gouch hat mich in die nasen byssen* und LN 809–811: *Ich wolt mir ee in die zungen beissen, Das ich mich wider sie wolt spreissen Vnd wolt mich gegen inen sperren*). Wenn ein Satz physische oder psychische Prozesse im Innern des Menschen darstellt, wird, wie Starke ausführt[46], in jedem Falle der persönliche Dativ gebraucht: der Mensch wird dann nicht durch die Handlung eines anderen von außen physisch affiziert, die Verben treten also gar nicht (mehr) als Verben der körperlichen Berührung im eigent-

[43] Vgl. Sommer, a.a.O. 30.
[44] A.a.O. 436.
[45] Vgl. v. Weiß, a.a.O. 454.
[46] Vgl. Starke, a.a.O. 22 (1969) 34f., 23 (1970) 76ff.

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lichen Sinne auf, sondern das dargestellte Geschehen vollzieht sich als psychische Reaktion — die im Dativ genannte Person fühlt sich innerlich stark an dem Vorgang/Zustand beteiligt.

Aus einem Vergleich des „sympathetischen" Dativs mit dem Dativ als generellem Kasus, wie dieser oben charakterisiert wurde, ergibt sich kein wesentlicher Unterschied: beide bezeichnen in Opposition zu akkusativischen Fügungen grundsätzlich die in die Handlung mit hineingezogene Person!

Der Blick für das Wesen des „sympathetischen" Dativs wird unserer Ansicht nach durch den Umstand getrübt, daß — wie gezeigt — viele Verben durch die Verwendung eines zusätzlichen Adverbiale mit dem „sympathetischen" Dativ gebraucht werden können, mithin eine dativische Fügungspotenz im Sinne Starkes erlangen. Dieser extensivierte Dativgebrauch begegnet aber auch in Fügungen, wo auf keinen Fall ein „sympathetischer" Dativ vorliegt, z.B. in „Er eilte dem Ertrinkenden zu Hilfe", „Was steht zu Ihren Diensten?".

Es gibt somit keinen schwerwiegenden Grund, weiterhin von dem „sympathetischen" Dativ als Abart des Dativs zu sprechen: in Oppostion zu akkusativischen Fügungen sind alle dativischen mehr oder weniger „sympathetisch" gefärbt!

Was die Verwendung des sogenannten „sympathetischen" Dativs durch Thomas Murner anbelangt, sei bemerkt, daß Behaghels Meinung, nach der bei der Auswahl zwischen diesem Dativ und der possessiven Ausdrucksweise vor allem sachliche Gründe, daneben auch die rhythmische Gliederung und die Betonung eine Rolle spielen[47], sich im großen ganzen bestätigt. Außerdem begegnen aber Stellen, wo sich die (Nicht-)Verwendung dieses Dativs nicht zweifelsfrei erklären läßt (man siehe obenerwähnte alternierende konkurrierende Fügungen).

Aufgrund der manifesten Unzulänglichkeit der Kasusbezeichnungen lateinischen Ursprungs, namentlich für die „casus obliqui"[48], verdienen die deutschen Kasusnamen „Werfall", „Wesfall", „Wemfall", „Wenfall" unserer Meinung nach den Vorzug, da sie der Deixis der Substantiv-(c. q. Pronominal-)Formen im

[47] Vgl. Behaghel, a.a.O. 637.

[48] Vgl. „Duden. Grammatik der deutschen Gegenwartssprache" ²(Mannheim/Zürich 1966) 178ff.

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Kommunikationsfeld ‚Satz‘[49] mehr entsprechen — daneben kämen die Bezeichnungen „Primärkasus“, „Sekundärkasus“, „Tertiärkasus“, „Quartärkasus“ in Betracht (vgl. die im Niederländischen sehr üblichen Namen „eerste naamval“, „tweede naamval“, „derde naamval“, „vierde naamval“).

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[49] Vgl. W. Eichler, Sprachdidaktik Deutsch. Ein kommunikationswissenschaftliches und linguistisches Konzept (München 1974) 225f.

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Hamp, Eric P., *A Reshaped Irregularity* , Indogermanische Forschungen, 84 (1979) p.255

A Reshaped Irregularity

My Chicago colleague Bill J. Darden has recently shown[1] a subtle correlation between the accentuation of Lithuanian verbs and the distribution of *ē*- and *ā*-preterits. The only non-obvious exception to his rule is the verb *púola pùlti* 'fall'. The object of this brief communication is to show that this anomaly is not isolated; that it is matched by anomalies elsewhere in Indo-European; that these anomalies have good, if unusual, IE. origins; that the Baltic languages, while having levelled and regularized to a degree the anomalous paradigm, have retained vestiges of the anomaly, but transformed into new patterns of accent, tense-suffix, and ablaut; and that sufficient parallels and minor rules survive in Baltic verbs to indicate what the earlier development must have been.

It has been customary to point in the literature and handbooks to possible cognates in Germanic and Armenian: OHG. *fallan* OE. *feallan* ON. *falla*, Arm. *p'ul* 'Einsturz', *p'lanim* 'falle in'; for example, Walde-Pokorny under the entry **phōl-*. The trouble with this comparison as stated in those terms is that such a root has an odd configuration and the genesis of the observed abnormal forms is not in any case explained thereby. Without at least a sketch of the intervening history, no reconstruction can be believed.

Let us start with the Germanic forms, which have long been remarked by Germanicists as isolated among Germanic verbs. The verb is a class VII strong verb, and the handbooks sometimes apologize for classifying this item along with the remains of the reduplicating preterits; e. g. Sievers-Brunner does simply that. It will be sufficient for our present purposes to review briefly the membership of the heterogeneous class VII in Old English: (Each example is followed by a schematic indication of the original Germanic ablaut alternation.)

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[1] See his article appearing in Folia Slavica (Columbus, Ohio).

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hātan hēt 'command'	**ai/ē(i)*
bēatan bēot 'beat'	**au/ēu* (so also *hēawan* 'hew', *hlēapan* 'leap')
blandan blēnd 'mix'	**an/ēn* (so also *fōn fēng* 'take')
lætan lēt 'let'	* /ē
cnāwan cnēow, *blōtan blēot*, *grōwan grēow*	**ō/ē* (+ **-u-*) (*wēpan*, Anglian *-œ̄-*, *wēop* has a weak present **wōpijan*.)
fealdan fēold 'fold'	**al/ēl* + C (also *wealcan* 'roll', *weallan* 'boil')

The characteristic of this class is that it shows a preterit in **ē*, a feature of much debated origin; it would appear to be either a Dehnstufe, or a normal grade to a zero-grade (Schwundstufe) present. In modern terms, such a normal grade would have contained an IE. laryngeal (**H*); the Germanic *a*-vocalisms would then represent resolutions of syllabic **H*, or **ə* (schwa).

West Saxon *feallan fēoll* falls in as a unique item with *fealdan* etc. The vocalism of the Rushworth Mercian *fēllun* (preterit) is however significant. The West Saxon vowel breaking, as well as the paradigmatic generalization of the geminate *ll* (also to the noun), is clearly illustrated by comparison with the Anglian forms:

WSax. *feallan, healdan, fiell* 'Fall' (*i*-stem masc.)
Angl. *fallað, hāldan, fæll* (= /e/)

Now it is generally agreed that Germanic **fallan* was in origin an old nasal present, a formation that has virtually vanished from Germanic. Therefore beside **fal-n-an* we should expect a preterit more on the order of **fēl*. Yet the Baltic and Armenian vocalisms lead us to prefer **fōl*; such a result would have been highly anomalous within Germanic and would have easily prompted levellings—yet we see from the Old English that the resulting paradigm failed to fit completely into the class in which the speakers put it.

We may summarize the Germanic development:

**pHl-n-* : *peH$_o$l-* (i. e. **pəln-* : *pōl-*)
>**faln-* : pret. *fōl-*

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$>$*ʃall- : *ʃōll* (because *galan, alan, gōl ōl*, with single *l* remained)

$>$*ʃall- : *ʃēll* (i. e. integrated in class VII because of -CC structure)

I have dealt elsewhere (KZ 89, 1975, 100–9) with the fate of the IE. nasal presents in Armenian, and in that context I have included an analysis of the verb *pʿlanim*. In that discussion I have also remarked the interesting correlation to be seen in Armenian between old middle-voice verbs of IE. and certain nasal-present classes of Armenian. For the vocalism of the noun *pʿul* we may very well see an original **peH$_o$l-* = *pōl-*; it would also be the simplest and most direct phonological account to see in *pʿlanim* an earlier **pʿulanim* by normal Armenian pretonic high-vowel syncope. Yet there is nothing to prevent us from deriving *pʿlanim* direct from **pHl-n-*. In fact I see the aspirate *pʿ* as resulting from the cluster **pH*, where the *H* failed to vocalize; such a cluster also explains immediately the vocalization by Sievers Law of **n* as *an*. Between the noun and the verb in Armenian we then have an exact match in form for the Germanic verb as sketched above.

It is now time to turn to the Baltic forms. For convenience I refer below by page and paragraph number to Endzelīns' Comparative Phonology and Morphology of the Baltic Languages (trans. Schmalstieg and Jēgers). Our verb is (215 § 350) Lith. *púolu púola, púolė* ~ *púolo, pùlti*. It is noteworthy that Latv. *puol pula pult* reflects the anomaly of our concern indirectly by diverging in the preterit with a renewed vocalism; here, we shall see, is an interesting case where the original distribution of shapes has been totally effaced.

Now the preterit *púolė* must be the archaic one here. It is seen that the distribution of *ē* and *ā*-preterits among the *o*-presents is by no means as complicated as Endzelīns (§ 389) makes it out to be; contrary to the translators' comment, I also do not see the *ē*-type as being in an important way analogical. For the *o*-presents in particular the *ā*-preterite is the productive and dominant one, with the *ē*-preterit specialized for roots in *e* + obstruent

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and in *in* → *īn*, apart from some old archaic residues[2]. Among these residues I see our form *púolė*, as well as *ėdė* and *ãvė*.[3] Thus *mìrė* 'died' is surely not an innovation on the basis of *gìmė* 'was born' (§ 389), and *gìmsta* (which would otherwise call for an *ā*-preterit) must be an innovation on the basis of the old middle meaning; all of this fits in as an indirect survival of the middle-voice morphology that these two semantic verbs carried in IE. Such a background for *púola* would also fit in with the Armenian facts.

In sum, we see that *púolė* < **pōl-* indirectly matches Gmc. **fēll*. We may suppose, then, that *púola* covers an old anomalous inherited nasal present. I suggest that the exact parallel for *púolė* is to be seen in Latv. *àva* (*ē*); the parallel for the rest of the lost paradigm must then be in Latv. *àun àut* Lith. *aũna aũti* 'to shoe'. Here the nasal present survived because the root terminated in a vowel. The unique root-variant **pal-* vel sim. (probably not *pùl-*) perished.

Finally, Latv. *pula* results from attraction by *dęl dila dìlt* = Lith. *dēla dìlo dìlti* 'dwindle', which is also semantically related.

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[2] For a very different account see Chr. S. Stang, Vergleichende Grammatik der baltischen Sprachen (Oslo 1966) 386–91. His reporting is systematic but complicated. I differ too on some interpretations beyond what is discussed here. Stang's main emphasis on transitivity (388) is not totally certain.

[3] Since writing this I now see that we can remove *ėdė* from the set of special cases; at the same time the preterite class of *ėdė* is seen to be a valuable confirmation of the phonological accounting of this verbal root. In light of Werner Winter's excellent argument in J. Fisiak ed., Recent Developments in Historical Phonology, The Hague: Mouton, 1978, 431–46, esp. 433, 438, whereby vowels were regularly lengthened in Balto-Slavic before original mediae, *ėdė* now appears as the expected development of a root in original **e* + obstruent. We may further remark here that *bėgo* to *bėga bėgti* (Latv. *bę̂ga* : *bę̂g bē̂gt*) 'run, flee' (Endzelīns 215 § 350) does not violate either Winter's or this formulation by showing the productive *ā*-preterite. It seems to be an insufficiently appreciated fact that Greek φέβομαι was a defective verb restricted to the present system. Therefore we must regard *bégo* as hysterogenic and regular.

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Kortlandt, Frederik, ***On the History of the Slavic Nasal Vowels*** , Indogermanische Forschungen, 84 (1979) p.259

On the History of the Slavic Nasal Vowels

I.

How many Common Slavic nasal vowels do we have to reconstruct? The establishment of **eN* and **oN* is no longer questioned by modern investigators, e. g. OCS. *peNtь*, *poNtь*, Skt. *páñca*, *pánthāḥ*[1]. Since the publication of N. Trubetzkoy's Old Church Slavic grammar[2] it must be recognized that there was no phoneme /j/ in the language of Cyrillus and Methodius and that, consequently, we have to assume a third nasal vowel **öN*, which developed into **joN* in 11th century Bulgarian.

The South Slavic reflex of **eN* is a mid vowel, whereas the reflex of **oN* is high (Serbo-Croat) or mid (other languages). The North Slavic reflex of **oN* is also mid (Polish) or high (other languages), but the reflex of **eN* is twofold here: it is a high mid vowel, identical with the reflex of **ě*, in the acc. pl. ending of the *jo*-stems and the gen. sg. and nom. acc. pl. endings of the *jā*-stems, and a low vowel elsewhere. Since the difference cannot be explained as a secondary development, a strict application of the comparative method leads to the postulation of two unrounded nasal front vowels which merged in the South Slavic dialectal area[3]. I shall write **äN* (rather than **eN*) for

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[1] In this article I shall write the Slavic nasal vowels as the corresponding oral vowels followed by *N* in order not to overburden the text with diacritical marks. It should be clear that *N* designates a feature of the preceding vowel and does not represent a separate segment.

[2] Altkirchenslavische Grammatik (Wien 1954), especially p. 62.

[3] This conclusion was first drawn by N. van Wijk, Archiv für slavische Philologie 36 (1916) 461. I shall not discuss the numerous attempts to explain the correlating endings *-y* and *-eN*/*-ě* along separate lines because all of them fail in the same respect: an explanation involving analogic change requires not only the indication of a model, but also the presence of a plausible motivation. As Meillet put it in Rocznik Slawistyczny 7 (1914) 8: "Il est étrange que, pour éviter d'admettre

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the low front vowel, and **ęN* (rather than **ěN*) for the high mid vowel which yielded South Slavic *-eN* and North Slavic *-ě*.

The back counterpart of **ăN* is found in the nom. sg. ending of the present participle, e. g. Czech and Old Russian *nesa*, Old Polish *rzeka* (written *reca* in the Kazania Świętokrzyskie), Old Slovene *imy* (written *imugi* in the Freising Fragments), OCS. *nesy*. The North Slavic forms cannot have taken the ending from the soft flexion because the stem-final consonant is not palatalized. The nasal character of the vowel is preserved in the Old Bulgarian manuscripts, where the ending is written 48x as a nasal vowel. It is written with a special sign in the Zographensis, with the sign for *eN* in the Clozianus and the Savvina Kniga, with the sign for *oN* in the Psalterium Sinaiticum, while all of these denotations are used in the Marianus. The absence of a separate letter for this sound in the original Glagolitic alphabet must be viewed in connection with the absence of a letter *y*, for which a digraph was used. I assume that the phonetically complex unrounded back vowels **y* and **yN* had developed into a diphthong **ъi* in the South Macedonian dialect for which Cyrillus created the alphabet. When the alphabet came to be used for Bulgarian dialects which had retained the complex nasal vowel, the latter could be denoted either by a newly created sign, or by one of the existing signs for nasal vowels. Thus, both comparative and philological considerations lead to the postulation of an unrounded nasal back vowel, which I shall write **aN* and which yielded South Slavic *-y* and North Slavic *-a*[4].

Structural and chronological considerations lead to the postulation of a sixth Common Slavic nasal vowel for those case endings of the *o-* and *ā*-stems where the *jo-* and *jā*-stems have

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des traitements phonétiques qui ne contredisent aucun traitement connu des mêmes phonèmes placés dans les mêmes conditions, on ait recouru à des hypothèses analogiques qui sont ou arbitraires ou invraisemblables, comme si les difficultés morphologiques étaient, par nature, chose moins grave que les difficultés phonétiques."

[4] This conclusion was first drawn by N. van Wijk, ZslPh. 1 (1925) 283. The literature on the subject is shortly reviewed by J. Ferrell in Studia Palaeoslovenica (Praha 1971), 86–89.

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South Slavic *-eN* and North Slavic *-ě*. The preservation of nasality in the South Slavic soft inflexion shows that the nasal feature must have been present in the hard inflexion at the time of the Umlaut. I therefore posit a high unrounded back vowel **yN* which yielded *-y* in the entire Slavic area when it lost its nasality in prehistoric times. Thus, we arrive at the following reconstruction:

	front		back	
	unrounded	rounded	unrounded	rounded
high	*ęN*		*yN*	
mid		*öN*		*oN*
low	*äN*		*aN*	

The occurrence of the high vowels was limited to the gen. sg. and nom. acc. pl. case endings. They lost their nasality at an early stage in the North Slavic dialectal area. In South Slavic, the vowel height opposition was lost. The unrounded back vowels were particularly liable to denasalization because of their complex articulation. The denasalization of **aN* was apparently early in clause-final position and late before a clitic. In the Kazania Świętokrzyskie, the form **rekaN* 'saying' is written *reca* when it immediately precedes the quotation, but *rekø* before the pronominal object *ta*. In the Old Bulgarian texts, the preservation of the nasal feature in this ending is found almost exclusively before the enclitic article, as Meillet has observed[5]. The chronological status of the above system of nasal vowels can only be specified in relation to other historical developments. I therefore turn to the periodization of the history of Slavic.

II.

In this section I shall give a short chronological outline of Slavic historical phonology in order to indicate the general framework of the reconstructions. It goes without saying that a discussion of the separate developments would go far beyond the scope of this article. In the next section I shall give a more

[5] Rocznik Slawistyczny 6 (1913) 136.

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detailed chronology of those periods which are of specific interest for the rise and development of the nasal vowels[6].

I. Proto-Indo-European. To this period belong the neutralization of the opposition between the velar series after **u*[7] and after initial **s*[8] and the neutralization of the opposition between the laryngeals before and after **o*[9].

II. Dialectal Indo-European. To this period belong the loss of the aspirated stops[10], the rise of **ṣ* from **s* after **i*, **u*, **r*, **k*[11], and the depalatalization of the palatovelars before resonants under certain conditions[12].

III. Early Balto-Slavic. This is the stage of common innovations up to the loss of final **t/d*. It corresponds with SA 1–4. To this period belong the narrowing of final **-om* to **-um*[13] and the replacement of the nom. acc. sg. ending of oxytone neuter *o*-stems with the corresponding pronominal ending[14].

IV. Late Balto-Slavic. This is the stage of common innovations after the loss of final **t/d*. It corresponds with SA 5–6. To this period belong the merger of the barytone neuter *o*-stems with the masculines, the loss of the syllabic resonants,

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[6] The most important earlier publications on the chronology of the Slavic developments are the following: N. Troubetzkoy, Essai sur la chronologie de certains faits phonétiques du slave commun, Revue des études slaves 2 (1922) 217–234; N. van Wijk, K istorii fonologičeskoj sistemy v obščeslavjanskom jazyke pozdnego perioda, Slavia 19 (1950) 293–313; C. L. Ebeling, Questions of relative chronology in Common Slavic and Russian phonology, Dutch Contributions to the 5th International Congress of Slavicists (The Hague 1963), 27–42. The stages of the latter article will be referred to as E I–XVI, while SA 1–22 will refer to my Slavic Accentuation (Lisse 1975), p. xii.

[7] Cf. F. de Saussure, MSL. 6 (1889) 161–162.

[8] Cf. L. Steensland, Die Distribution der urindogermanischen sogenannten Gutturale (Uppsala 1973), 30–35.

[9] Cf. my article in Ling. Posn. 23 (1980) 127–130.

[10] Cf. IF. 83 (1978) 107–118.

[11] Cf. H. Pedersen, IF. 5 (1895) 33–87.

[12] Cf. Recent Developments in Historical Phonology (The Hague 1978), 237–243.

[13] Cf. Lingua 45 (1978) 287.

[14] Cf. Slavic Accentuation 45.

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the elimination of zero grade before oblique case endings in the formative suffix of consonant stems, and Winter's law[15].

V. Early Slavic. During this period, Slavic developed along the same lines as the West and East Baltic sister languages. It corresponds with E IV–VI, SA 7–8, and the stages 1–5 of the chronology given below. To this period belong the rise of nasal vowels, the merger of **a*, **ā* with **o*, **ō*, and the rise of **x*.

VI. Early Middle Slavic. The developments of this period form part of the trend toward rising sonority and synharmonism within the syllable. It corresponds with E VII–IX, SA 9–10, and the stages 6–10 of the chronology given below. To this period belong the palatalizations, the rise of distinctive tone, and the loss of final **s*.

VII. Late Middle Slavic. During this period, in which the trend toward simplification of the syllable structure reached its culmination, the earliest dialectal divergences developed[16]. It corresponds with E X–XI, SA 11–14, and the stages 11–15 of the chronology given below. To this period belong the metathesis of liquids, the rise of the new timbre distinctions, and the loss of /j/ as a phoneme.

VIII. Young Proto-Slavic. At this stage, the redundancies which the trend toward rising sonority had created evoked a reaction, which eventually led to the disintegration of the prosodic system and to the rise of new closed syllables. To this period, which corresponds with SA 15–18, belong the early contractions, the retraction of the stress from final jers, and Dybo's law[17].

IX. Late Proto-Slavic. This is the last stage of common innovations. To this period, which corresponds with SA 19–22, belong the loss of the acute intonation, the shortening of long falling vowels, and Stang's law[18].

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[15] Cf. Baltistica 13 (1977) 321–322.

[16] The only dialectal difference which is older originates from Illič-Svityč's law, which belongs to the Early Middle Slavic period, cf. Slavic Accentuation 28.

[17] Cf. Slavic Accentuation 14–16.

[18] Cf. Slavic Accentuation 33.

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X. Disintegrating Slavic. This is the stage of parallel but not identical developments in the separate languages. To this period, which corresponds with E XIII–XV, belong the rise of the palatalization correlation, the loss or merger of the nasal vowels, and the loss of the jers.

III.

As is clear from the preceding section, I date the narrowing of final **-om* to **-um* and the elimination of the syllabic resonants to the Balto-Slavic period. The loss of final **t/d* reintroduced the vowel **o* before a word-final nasal, e. g. in the 3rd pl. thematic aorist ending **-on(t)*, OCS *-oN*. The syllabic resonants received an epenthetic vowel, which was **u* after a preceding labiovelar consonant and **i* elsewhere. The original distribution has been obscured by a number of secondary developments, in particular by the rise of new apophonic alternations[19]. Consequently, the acc. sg. ending was **-ām* in the *ā*-stems, **-um* in the masc. *o*- and *u*-stems, **-o* in the neuter *o*-stems, and **-im* in the masc. and fem. *i*-stems and consonant stems at the beginning of the Early Slavic period. The subsequent changes were the following.

1. Raising of **ē* and **ō* before a final resonant, e. g. OCS. *mati*, *kamy*, cf. Lith. *mótė*, *akmuõ*, Gr. *μήτηϱ*, *ἄκμων*. The final resonant was lost after the raising. I assume that the acc. sg. ending **-ām* was shortened to **-am* at approximately the same stage[20]. As a result of these developments, word-final sequences of long vowel plus resonant were eliminated.

2. Labialization of **a*, **ā* and merger with **o*, **ō*. This development was posterior to stage 1 because the acc. sg. ending **-ām*, OCS. *-oN*, did not merge with **-ōn* in **akmōn*, OCS. *kamy*.

3. Rise of nasal vowels: **iN*, **eN*, **oN*, **uN*. This development cannot strictly be ordered because the nasal vowels are not opposed phonemically to sequences of vowel plus nasal con-

[19] Thus, I agree with A. Vaillant, Grammaire comparée des langues slaves 1 (Lyon 1950), 171–172. On the apophonic relations see R. Trautmann, Slavia 2 (1923) 1–4.

[20] The hypothesis that **-ām* yielded a Balto-Slavic nasal vowel, which I suggested in Slavic Accentuation, p. 6, cannot be maintained. On the 1st sg. present ending see now Lingua 49 (1979) 57.

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sonant up to the loss of the jers. The chronological placement advocated here is based upon the consideration that final **n* and **r* behaved alike at stage 1 and that the presence or absence of a following nasal played no role at stage 2. I assume that the nasal feature was lost in the accusative endings **-im*, **-um*, **-ins*, **-uns*, which yielded **-i*, **-u*, **-īs*, **-ūs*.

4. Raising before final **s*. The raising affected **-ois*, **-ōis*, and **-oNs*, cf. OCS. 2nd sg. imp. (opt.) *nesi*, inst. pl. *raby*, acc. pl. *raby*, *ženy*, for which I assume an intermediate stage **-uis*, **-ū(i)s*, **-uNs*. It did not affect **-os*, which yielded *-o* in the neuter *s*-stems and has been preserved in the suffix *-oš*, e. g. Czech *hnědoš* 'bay horse', *dlouhoš* 'tall man'[21]. There is no reason to assume that it affected **-ōs*. The raising was posterior to stage 2 because it affected the acc. pl. ending of the *ā*-stems. It was posterior to stage 3 because the acc. pl. ending of the *jo*-stems did not merge with the ending of the *i*-stems after the Umlaut, cf. OCS. *konjeN* vs. *poNti*.

5. Delabialization of **o*, **ō*. The delabialization and simultaneous lowering affected **o* to a lesser extent than **ō* because the opposition between them was rephonemicized as **o* vs. **a* at stage 14 below. It did not affect **oN* at all. I shall therefore write **a* and **ā*, but **oN*. The dat. sg. ending of the *o*-stems **-ōi* had become **-ōu* or **-ou* at this stage[22].

6. Umlaut. The back vowels **a*, **ā*, **oN*, **u*, **ū*, **uN* had fronted variants **ä*, **ā̈*, **öN*, **ü*, **ṻ*, **üN* after a preceding **j*. At this stage, **e* and **ē* merged with **ä* and **ā̈*, respectively. The archiphonemes can be written /ä, ā̈/. The merger was posterior to stage 5 because it presupposes the delabialization of **o* and **ō*. The nasal vowels **eN* and **öN* remained distinct, cf. OCS. *znajoN* 'I know', with preservation of the rounding. The other rounded front vowels also remained phonetically conditioned variants of the corresponding back vowels, e. g. **jüga* /juga/ 'yoke'.

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[21] Cf. T. Torbiörnsson, ZslPh. 1 (1925) 278.

[22] Cf. H. Pedersen, KZ. 38 (1905) 324, and C. L. Ebeling, Dutch Contributions 1963, p. 31.

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7. First palatalization. The velar obstruents had fronted variants before front vowels. When /e, ē/ merged with /a, ā/ after /j/ into the archiphonemes /ă, ā̆/, the sequences /ke, kē, ge, gē, xe, xē/ were rephonemicized as /čă, čā̆, ǯă, ǯā̆, šă, šā̆/. The rephonemicization was posterior to stage 6 because it presupposes the existence of the archiphonemes.

8. Monophthongization of diphthongs: **ai*, **ei*, **ui*, **au*, **eu* changed into **ē*, **ę̄*, **ū̆*, **ō*, **jŏ* /jō/. The occurrence of the diphthong **ui* was limited to the position before final **s*, where it had arisen at stage 4. After /j, č, ǯ, š/, the diphthongs **äi*, **üi*, **äu* changed into **ę̄*, **ū̆*, **ō*, where **ū̆* is the archiphoneme of /ū/ and the marginal phoneme /ŭ/, and **ŏ* is the phonetically conditioned variant of /ō/ after a palatal consonant. The Slavic vowel system now looks as follows:

ī		*ū̆*		*ū*		*iN*			*uN*			*i*		*u*
	ę̄			*ō*			*eN*	*oN*					*e*	*a*
		ē	*ā̆*											

After /j, č, ǯ, š/:

		ī		*ū̆*			*iN*			*üN*			*i*		*ü*
			ę̄			*ŏ*			*eN*	*ŏN*				*ä*	
				ā̆											

The monophthongization of diphthongs was posterior to stage 6 because **jai* yielded **ję̄*, not **jā̆*, e. g. in the locative endings of the *jo*-stems. It was posterior to stage 7 because **ē* from **ai* did not cause palatalization in spite of the fact that it tended to be more fronted than **ē* from earlier **ē*, as will be clear from the next development.

9. Second palatalization. The velar obstruents became fronted before the new front vowels **ē*, **ū̆* which had arisen from **ai*, **ui*, and after the high front vowels **i*, **ī*, **iN* unless the following syllable contained one of the vowels **u*, **ū*, **uN*[23]. The sequences /ika, iga, ixa/ etc. were rephonemicized as /ića, iǵă, iśă/ etc., and the long vowel /ā̆/ lost the status of an archiphoneme and came to be the fronted variant of /ā/ after

[23] Here again, I agree with Vaillant, Grammaire comparée 1, 53–55.

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a palatal consonant, e. g. **stajātę* /stajātę̄/ 'to stand'. We thus arrive at the following vowel system after /j, č, ǯ, š, ć, ź, ś/:

ī		*ū*		*iN*		*üN*	*i*	*ü*
	ę̄		*ō*		*eN* *öN*			*ä*
		ē *ā*						

It goes without saying that the second palatalization was posterior to the monophthongization of diphthongs.

10. Loss of final **s*. This development cannot be dated with precision. A comparison with the development of **s* in Indo-Iranian, Armenian, Greek, and Celtic suggests that final **s* may have become **h* in Early Slavic. The raising at stage 4 above must perhaps be dated after the rise of this **h*[24]. I date the ultimate loss of final **s* or **h* to the end of the Early Middle Slavic period because I think that it was only slightly anterior to the rise of prothetic **j* and **v* at stage 11.

The nom. sg. ending of the present participle, PIE. **-onts*, requires some discussion. The historical reflexes of this ending show that it did not merge with **-ons*. The simplest explanation is that the rise of nasal vowels at stage 3 was inhibited by a following tautosyllabic stop and that the rise of nasal vowels in the latter environment was posterior to the delabialization at stage 5. If this hypothesis is correct, the participial ending was **-ants* at the beginning of the Middle Slavic period. When **t* was lost in this ending, the new nasal vowel **aN* did not merge with **oN*. I date the rise of the new nasal vowel to stage 8 and add **aN*, after palatals **äN*, to the above schemes. When final **s* was eventually lost, the participial ending became **-aN* in the thematic flexion, **-eN* or **-aN* in the athematic flexion, and **-iN* in the *i*-flexion, while the 3rd pl. ending of the thematic aorist was **-oN* and the acc. pl. ending of the *o*- and *ā*-stems became **-uN*. I find no evidence for **aN* in non-final syllables.

11. Prothesis. The hiatus between a word-final and a word-initial vowel was filled with a non-phonemic glide. This glide merged with **j* if either the preceding or the following vowel was a front vowel (or both), and with **v* if the preceding vowel was back and the following vowel was rounded. Consequently,

[24] Cf. the Iranian development of **-ah*, **-āh* to *-o*, *-å*.

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initial /j/ lost the status of a phoneme before unrounded vowels. Initial **jä-* and **jā̈-* were rephonemicized as /e-/ and /ē-/, e. g. **jā̈xātę* /ēxātę/, earlier /jāxātę/, cf. Lith. *jóti* 'to ride', now with the same Anlaut as /ēstę/, Lith. *ėsti* 'to eat'. I assume that the rare sequence **jē-* was rephonemicized as /ę-/ in **jēskātę* 'to seek', Lith. *ieškóti*, where Arm. *ayc̣* and OHG. *eiscōn* point to **ai-*. The twofold glide before a rounded vowel gave rise to doublets, e. g. OCS. *utro* and *jutro* 'morning', cf. also *ajce* and *jajce* 'egg'[25]. The rise of **j* between front vowels was anterior to the monophtongization of diphthongs at stage 8 because it affected original **ē-*, which became **jā̈-*, but not **ai-*, which became **ē-*. The rise of **j* after front vowels before **ē-* from **ai-* must be dated to stage 9, when the opposition between /ē/ and /ā/ after /j/ became phonemically possible. The rise of **j* after back vowels before **e-* was posterior to the merger of **jē-* from **ai-* with **ję̄-* from **ei-* after front vowels. Thus, I assume that OCS. *iskati* represents the development of **ai-* after a front vowel, while the reflex after a back vowel is found in *jasnъ* 'clear', Lith. *áiškus*. The rise of prothetic **j* and **v* after back vowels was probably posterior to the loss of final **s*, which occasioned the frequent occurrence of hiatus at stage 10.

12. Delabialization of **u, *ū, *uN, *ü, *ṻ, *ü̈N*. This development yielded **y, *ȳ, *yN, *i, *ī, *iN*, e. g. **vȳdrā* 'otter', **lyNka* 'bast', **jiga* /iga/ 'yoke', 2nd sg. imp. **nesī*, acc. pl. **arbyN, *kanjiN*. As a result of the delabialization, the prothetic **v* before **y, *ȳ* received the status of a phoneme. The new **iN* from **ü̈N* did not merge with original **iN*, which had apparently merged with **eN* at this stage, e. g. **xvāleN* 'praising'. We thus arrive at the following vowel system:

ī			*ȳ*			*yN*		*i*	*y*
	ę̄			*ō*	*eN*		*oN*	*e*	*a*
		ē	*ā*			*aN*			

After palatals:

ī				*iN*		*i*
	ę̄		*ō̄*	*eN*	*ōN*	*ā*
		ē	*ā̄*		*āN*	

[25] Cf. A. Meillet, Slavia 1 (1922) 198.

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The delabialization was posterior to the rise of prothetic **v* at stage 11 because the latter could hardly develop before unrounded **y*, **ȳ*.

13. Raising of **ę̄* and **ō*. The empty hole which the delabialization of the high rounded vowel **ū* had left was filled by raising the remaining rounded vowel **ō*. The corresponding front vowel **ę̄* was raised to merge with **ī*. The phonetically complex unrounded nasal back vowel **yN* lost its nasal feature and merged with **ȳ* under the acute intonation and with **y* elsewhere, e. g. **lȳka* 'bast', **syta* 'hundred'[26]. The corresponding nasal front vowel **iN* was lowered to **ęN* while **eN* merged with **äN*. These developments yield the following vowel system:

ī	*ȳ*	*ū*	*eN*		*oN*	*i*	*y*
ē	*ā*			*aN*		*e*	*a*

After palatals:

ī		*ū̄*	*ęN*		*öN*	*i*
ē	*ā̄*			*äN*		*ä*

The raising of **ō* was posterior to the delabialization of **ū* at stage 12 because the two did not merge. The loss of **yN* was posterior to its delabialization, which gave rise to its complex articulation. The merger of **eN* with **äN* was apparently posterior to the merger of original **iN* with **eN*.

14. Rise of new timbre distinctions. The redundant timbre distinctions between the short vowels and the acute long vowels became phonemically relevant when the latter lost their phonemic length[27]. For example, earlier /a/ became distinctively rounded /o/ because it was not opposed to a rounded short vowel, while acute /ā/, which was distinctively unrounded in relation to /ū/, took the place of /a/. The rephonemicization gave rise to the following vowel system:

i		*y*		*u*		
e	*ъ*		*ъ*	*o*	*eN*	*oN*
	ě		*a*		*aN*	

[26] On the phonetic character of the acute intonation at this stage see Slavic Accentuation, p. 29. The denasalization in **ungnis* 'fire', OCS. *ognjь*, must be dated to the Early Slavic period.

[27] On the details of this process see Slavic Accentuation 29–34.

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After palatals:

i		*ü*		
e	*ъ*		*ęN*	*öN*
	ě	*ä*	*äN*	

The rise of the new timbre distinctions was posterior to the raising of **ę̄* and **ō* at stage 13 because these vowels are reflected as *i* and *u* in the historical languages. It was also posterior to the loss of **yN* because the latter yielded two reflexes, *ъ* and *y*, the timbre difference between which cannot be explained if we assume that **yN* was preserved up to a later stage.

15. Loss of /j/. When /j/ was eliminated from the phonemic system, the rounded front vowels **ü* and **öN* received the status of phonemes, while **ä* merged with /ě/, e. g. **stojäti* /stoěti/ 'to stand', cf. **jäsnъ* /ěsnъ/ 'clear'. After the surviving palatal consonants, **ü* and **öN* became the archiphonemes of /u, oN/ and the newly arisen /ü, öN/, and **ä* remained the fronted variant of /a/, e. g. gen. sg. **końǟ* /końā/. The nasal vowel **eN* merged with **äN*, so that we arrive at the following vowel system:

i	*ü*	*y*	*u*				
e	*ъ*	*ъ*	*o*	*ęN*	*öN*		*oN*
ě		*a*		*äN*		*aN*	

When **j* was lost in consonant clusters, the following vowel was lengthened, e. g. SCr. *pîšē* 'writes' from **pīsje*, earlier **pę̄sje*[28]. Since the new /ē/ did not merge with /ě/, the loss of /j/ was posterior to the rise of the new timbre distinctions.

16. Later developments. After the end of the Middle Slavic period, new instances of /ē/ and /ō/ arose from the early contractions and from the retraction of the stress from final jers[29]. Different vowel quantities in the same ending were levelled out to a certain extent[30]. In the dialect of the Kiev Leaflets and

[28] Ibidem, p. 30

[29] Ibidem, p. 39 and p. 15.

[30] Ibidem, p. 32. I still subscribe to the explanation put forward ibidem, p. 47, for the replacement of the gen. sg. ending of the *ā*-stems with the acc. pl. ending. Cf. also Vaillant, Grammaire comparée 1, p. 211.

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a part of the Macedonian dialects (Psalterium Sinaiticum), **ě* and **a* merged after palatal consonants. In the other Slavic dialects, postconsonantal **ě* was subject to raising under various conditions. Since a detailed account of these developments goes beyond the scope of this article, I shall limit myself to a few remarks.

In Russian, postconsonantal **ě* was raised to **ę* and became the counterpart of **ǫ* from **ō*. The loss of the nasal vowels was anterior to the rise of the palatalization correlation in this language. Postvocalic **ě* merged with the reflex of **äN*. These developments yielded the following vowel system:

i	*ü*	*y*	*u*
ę			*ǫ*
e	*ъ*	*ь*	*o*
	ä	*a*	

The rise of the palatalization correlation and the rise of /j/ reduced this system to the following:

i		*u*
ę		*ǫ*
e	*ə*	*o*
	a	

The unrounded back vowel **y* and the rounded front vowel **ü* became variants of /i/ and /u/, respectively, at this stage.

In Polish, the rise of the palatalization correlation and of new /j/ was earlier than in the other Slavic dialects. Postconsonantal **ě* merged with **a* before hard dentals and with **e* and denasalized **ęN* elsewhere. These developments yielded the following vowel system:

i		*u*		
e	*ə*	*o*	*eN*	*oN*
	a			

At the next stage, **e* merged with **o* before hard dentals, and elsewhere with the central vowel, which became fronted. The nasal vowel **eN* merged with **oN*, which lost its distinctive

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rounding. The merger of the nasal vowels was earlier before hard dentals than in other positions[31].

In Serbo-Croat, the loss or merger of the jers was so early that it prevented the rise of the palatalization correlation. As a result of the denasalization, **ăN* and **ęN* merged with **e*, while **oN* and **öN* became **ǫ* and **jǫ*. Postvocalic **ě* was rephonemicized as /ja/ and postconsonantal **ě* was raised to **ę*. The loss of the distinction between front and back vowels and the elimination of the nasal vowels yielded the following system:

i		*u*
ę		*ǫ*
e	*ə*	*o*
	a	

This system was simplified along divergent lines in the separate dialects[32].

IV.

It follows from the chronological analysis presented here that the system of nasal vowels which was established in accordance with the comparative method in section I above never existed as a synchronous phonemic system because the unrounded back vowel **yN* was lost at stage 13 while the rounded front vowel **öN* received the status of a phoneme at stage 15. The analysis is meant to be an illustration of the way the comparative method can be enriched with the principle of relative chronology.

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[31] Cf. M. van Wijk, Prace Filologiczne 14 (1929) 477–484, and Z. Stieber, A Historical Phonology of the Polish Language (Heidelberg 1973), 36–42.

[32] Cf. especially W. Vermeer, Proto-Slavonic **u* in Kajkavian, Zbornik za filologiju i lingvistiku 21 (1978) forthcoming.

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