

Linguistic Reconstruction and Indo-European Syntax

EDITED BY
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Paolo Ramat (ed.)

*Linguistic Reconstruction
and Indo-european Syntax*

LINGUISTIC RECONSTRUCTION
AND
INDO-EUROPEAN SYNTAX

Proceedings of the Colloquium of the
'Indogermanische Gesellschaft',
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P.R.

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PREFACE

Opening the Colloquium with few welcoming words I quoted a statement by the great Polish linguist and Indo-Europeanist, Jerzy Kuryłowicz, which deserves to be repeated here:

“general linguistic considerations seem to play an ever increasing role in historical reconstruction. We become more and more convinced that the apparently infinite variety of linguistic changes (...) can be broken down into elementary relevant stages and reduced to a certain number of types” (Kuryłowicz 1973: 63).

Problems of language change and language reconstruction have proved in turn to be of primary importance for any general theory of language, not only in the more traditional domains of IE studies such as phonology and morphology.

In recent years we have had conferences on diachronic syntax and syntactic change on the one hand, and conferences on linguistic reconstruction on the other (see references in Vincent, this vol.). A Colloquium especially devoted to the most sophisticated, ambitious but also uncertain part of the reconstructed IE language, namely, the confluence of the topics of the two approaches referred to here was long overdue.

In many ways this Colloquium represents the continuation of the Società di Linguistica Italiana Conference the writer organized in 1975 at Pavia University. On that occasion specialists in very different language families met with the purpose of discussing issues reflected in the Conference title ‘Problems of Linguistic Reconstruction’ (see Simone & Vignuzzi 1977). On this occasion participants were, of course, mostly Indo-

Europeanists.

But ‘exotic’ languages were by no means absent from the discussion in Pavia. A characteristic mark of current IE studies, especially in the domain of syntax, is that they are greatly affected by the theories of linguistic typology: Japanese, Chinese or Georgian facts are no longer merely regarded as ‘curiosa’; they are often considered capable of providing useful parallels to IE phenomena and sometimes of ‘explaining’ IE problems. Certainly, we must agree entirely with Chr. Lehmann’s warning when he states that “eine Anwendung der typologisch-vergleichenden Methode auf das Indogermanische setzt notwendig Ergebnisse der historisch-vergleichenden Methode voraus” (this vol.: 155) since the phonic shape (the ‘second articulation’) of the protolanguage forming the base for its morphosyntactic structure is recoverable only via historical comparison of the extant languages (cf. the ‘stoffliche Uebereinstimmungen’ alluded to by K.H. Schmidt, this vol.). However, the typologically oriented approach to PIE syntax is the area where present syntactic research reveals the most significant differences as compared with the classical works of Delbrück, Wackernagel, Hirt, etc.

As Uspenskij rightly points out (1968:17), “the [reconstructed] proto-language represents the totality of information on the typology of the given group of languages” since “if in all (the majority) of the languages of a given group a certain category exists, it is ascribed to the respective proto-language”. But beyond this, as it were, internal typology, the typological approach encompasses our reconstructions of protolanguages in a broader framework of comparative studies (see Dezső, this vol.):

“Typological studies make it possible to predict certain structural features of a reconstructed language and determine the typological probability of a certain reconstruction”, Uspenskij 1968: 16.

Nowadays we do in fact have an idea, based on sufficient data of what a language type can look like, so that it is possible to see whether a language (or a language reconstructed on the basis of ‘stoffliche Uebereinstimmungen’) is type consistent or not. The typological inconsistencies will prove to be pivotal to an understanding of dynamic changes.

The discussion at the Pavia meeting centered precisely around the problem whether we are allowed to induce from our extant knowledge of languages and language changes, general rules of typological value or whether such rules must be deduced from a general theory of language and language change (see below). The core problem is thus the methodo-

logical one of induction vs deduction — by no means a rare issue in linguistics: what is noteworthy, however, is that this problem has now reached also the technical sector of IE studies.

Many of the papers on this volume deal with the ‘dynamicization of typologies’ advocated by Greenberg (1974: 65), inasmuch as they infer changes of type from the attested IE languages thereby getting back to the reconstructed PIE or try to recover typological features of yet older speech situations (the ‘Typologie des Vorindogermanischen’ of K.H. Schmidt), where

“intergenetic comparison involves the testing of reconstructions not in terms of the typological plausibility of the proto-system as a static entity, but in regard to the plausibility of the dynamic succession of types posited by the same reconstruction. This involves comparison with similar developments in historically independent cases”, Greenberg 1974: 68).

The typological approach to problems of reconstruction by no means implies neglect of the historical dimension — it is on the contrary an application of the comparative-historical method at a higher level. There is no ‘jump’ from the tradition of comparative philology to the present typological approach — as a mere glance at the history of our discipline will easily prove.

Certainly, we need to be aware of the risk of an aprioristic procedure which argues that, since the typological premises are such and such, the (proto -) language under scrutiny *must* have had such and such features. This would simply mean reintroducing the ‘Ausnahmslosigkeit’ of the Neogrammarians at a different level: it is on the contrary well-known that nowadays the concept of ‘law’ is meant as the a posteriori ascertainment of a regularity of phenomena also in natural sciences, and not an a priori ‘must be’.

This is the basis of Lightfoot’s criticism of W.P. Lehmann’s typologically-based procedure in reconstructing PIE syntax, and of Vincent’s claim (in keeping with Lightfoot) that we need a theory of linguistic change prior to any reconstructive methodology in order to ascertain what changes can be regarded as possible and what are not (see Lightfoot, Vincent, this vol.). This theory-oriented position consequently attempts to establish universal patterns of syntactic drift, such as Vincent’s ‘grammaticalization’ (for instance Chin. *bǎ* “take hold of” > particle functioning as an object case marker).

The concept of ‘universal’ perhaps requires some comments. Typology is not concerned with true universals (i.e. properties of the human language ‘qua talis’, logically deriving from or implied in the very notion of language and its definition: see Ramat 1978: 142f.; 1979: 275f.) but with generalizations derived from empirical observation – which by no means implies that we have no theory of typology (cp. Ramat 1978b). The trend toward grammaticalization of full words, to take again the case made by Vincent, can certainly be observed in many, even genetically unrelated languages and can be semiotically labelled as a drift ‘motivated (iconic) → unmotivated (symbolic)’; but the reverse is also true: we need only think of the development of periphrastic forms in Romance and Germanic verb systems (*amabo* → *amare habeo*, *amor* → *amatus sum*).

From the point of view of typology both developments are possible – but neither on the other hand can be logically derived from a true universal of language.

Moreover, besides conservative mutations which substitute a (morphemic) category with a new one maintaining the same function, we also have innovating mutations introducing new categories with different functions as a result of shifts in type (e.g. definite articles, relative pronouns, etc.): Benveniste 1968. Both developments will usually show a drift toward iconicity: *amabo* → *amare habeo*, *homo* → *ille homo*, which in its turn can later become a grammaticalized (i.e. symbolic, unmotivated) device: *amerò*, *l’uomo*. Cyclic developments¹ make it impossible to predict one-way typological evolutions which would enable us a priori to be sure of what kind of linguistic forces are/have been at work. This cyclicity is the result of a dialectic relationship between grammatical organization and expression plane (speech act level): on the one hand, we have a trend toward economy and grammatical simplification avoiding unnecessary differences, not easy to be memorized; on the other hand, we are faced with the constantly renewed need for vivid (i.e. iconic) expressions imposing new expression strategies (cf. the case made by Hagège 1978 concerning the notions of subject – grammatical level – and theme – speech level).

This is why the diachronic typology briefly sketched by L. Dezső in this volume is data-oriented and based on induction. No theory of grammar can explain why the shifting SVO → VSO really does occur whereas

VSO → SVO is not backed up by any evidence. The explanation is probably to be sought in perceptual strategies and processing devices in relation to the morpho-syntactic surface structure type—that is at a psycholinguistic level and not at the level of formal representation of grammar. It is true that at present a general theory of language change is missing, but there is no shortage of evidence for language changes and this evidence can provide hints for a typology of the possible changes a language can undergo, and hence give reasonable cues for the typological reconstruction of a proto-language. In keeping with Carol Justus (this vol.: 184) we can safely affirm that revisions of our reconstructional statements will of course become necessary as generalizations of new typological facts lead us to new insights and new predictions. Moreover, as mentioned above, we have to take into account the possibility of different diachronic stages of PIE; but the method need not be abandoned if some previous predictions should prove to be wrong.

By typological reconstruction we mean not only the reconstruction of a given stock of morphemes along the patterns of the previously reconstructed phonological system but also the reconstruction of a type of language, say inflectional, with a series of characteristics of its own (Ramat 1977: 26f.). To quote the example discussed by Andersen in this vol.: it is well-known that IE languages can show the following scheme for comparative constructions:

Adj. + *-ye/os + Pivot + Standard: *major quam tu*

(we disregard here the question of word order). The particle serving as pivot can, however, be different from language to language. We have here the same function and the same construction, not the same form — i.e. a typological coincidence. The question whether we are faced here with parallel developments or whether one particular pivot can be ascribed to the proto-language cannot be answered within (typological) comparison²: comparison can merely confirm the existence in the IE languages of a type of comparison *major quam tu* and not, say, of the paratactic juxtaposition like ‘A is big, B is not’ meaning ‘A is bigger than B’.

The same holds true for the complementation patterns of subordinate clauses dealt with by W.P. Lehmann in this vol. Although individual IE languages have different devices to introduce subordinate clauses, the oldest evidence points to a kind of paratactic anteposition of the subor-

dinate clause, not yet introduced by a complementizer, so that ‘subordinate’ clauses show no formal distinction from principal clauses. This is consistent with the OV type and we argue for a subsequent typological development of the hypotactic clause, although we are unable to assign to the proto-language any particular pattern of complementation (see also the different complement clause types exemplified by C. Justus in this vol.).

W. Boeder considering the type *vr̥trāya hántave* ‘for killing Vṛtra’ (*Vṛtra*-Dat + *han*-Verbal Noun-Dat) raises the same question: ‘Wie ist der kategoriale Charakter der Verbalnomina bzw. des Syntagmas zu klären, dessen Kern sie sind?’ (this vol.: 217). The purpose of syntactic reconstruction will in this case not only be the form of inflected infinitives but rather, beginning with consideration of the morphosyntactic feature of ‘agreement’ (the Patient agrees with the verbal noun governing it), that of recovering a type of language where complementation patterns did not yet exist and expansions of the basic sentence were expressed by inflected verbal nouns in substantive clauses:

<i>Agnīṃ</i>	<i>dvéṣo</i>	<i>yótavái</i>	<i>no gr̥ṇīmasi</i>
Agni(acc.)	hostility(acc.)	to-ward-off (dat.)	us we-implore

“We implore Agni to ward off hostility from us” (W.P. Lehmann, this vol.) as well as

<i>tád</i>	<i>ásvam</i>	<i>ānetavái</i>	<i>brūyāt</i>
then	(a) horse(acc.)	to bring(dat.)	he-should-order

“then he should order to bring a horse” (but liter.: he should order a horse to bring: Boeder, this vol.: 216), where the Target of the PRED₂ “to bring” is ‘raised’ to the grammatical object of the higher PRED₁ “to order”.

The organizers were keen to ensure different positions were well represented in the discussion with regard to the general methodological problems as well as to specific points of IE syntax. This was also the reason for inviting Indo-Europeanists from both the sides of the Atlantic – not so common an occurrence in the tradition of IE studies. Our American colleagues are, so to say, more sensitive to the impact of general theories of language which have in recent years played an important role in the development of linguistics – and they are actually more susceptible in their working conditions to the influence of the ‘allgemeine Lin-

guistik', whereas the European tradition has concentrated more on technical research on specific problems.

The Pavia confrontation proved that positions are not so distant as one might at first believe or, rather, that a division is not to be drawn between American and European tradition, but rather between theory-oriented and data-oriented approaches: IE studies reflect the general situation of linguistic studies and can contribute to the general debate. As an example of converging problems and solutions one could quote in this volume Bednarczuk's inquiry into the IE conjunctions **k~~u~~e* and **u[̃]e* with far reaching conclusions about IE parataxis and hypotaxis. The shift from clitic postpositions to tonic prepositions that referred to the whole sentence and no longer to a single NP is consistent with the drift from parataxis to hypotaxis already alluded to; and this process in its turn is exemplified not only by replacing participial constructions with complex sentences and by the elimination of pure nominal clauses, as Bednarczuk rightly points out, but also, e.g., by the rise of true relative pronouns introducing relative subordinate clauses, whereas the older structure seems on the contrary to have more been a coordination of coreferential clauses by means of deictic forms – a characteristic of the SOV type, with preposed relative clauses (cf. W.P. Lehmann, this vol.).

Which leads us to the much debated question of the basic order of the reconstructed IE protolanguage. Here too, the Colloquium provided the opportunity of hearing very different opinions on this particular though central problem, ranging from the certainty that PIE was an SOV language to the denial of any sound possibility of reconstructing any order whatsoever.

In the Colloquium, which was spread over two days, six main reports and nine shorter communications, mostly dealing with the same topics as the main reports, were read. The order of presentation has been abandoned in these Proceedings in order to obtain a more gradually and internally coherent organisation of the topics. The difference between main reports and shorter communications is clear from the length of the articles. The division in two parts (Methodological questions, and Problems of IE syntax) is obviously somewhat arbitrary since contributions to Part II also deal with methodological issues and use IE topics as examples in a more general discussion.

In order to underline the full range of different opinions it seems advisable, after Dezső's general remarks on grammatical typology, to begin our volume, the purpose of which is to represent – certainly: to a limited extent – the state of the art, with a stern critical opponent of the possibility and legitimacy of reconstructing syntactic features of old, unattested speech situations.

It is not the task of the editors to express agreement or disagreement with the opinions expressed by the contributors to the volume. The reader will see the different views maintained throughout this book for himself and will certainly be able to develop his own opinion. Let me only report an objection against any exaggerated scepticism concerning the reconstruction possibilities and procedures which – especially in the domain of syntax – derives from too rigid a demand for 'scientific' (i.e. formalized) models of grammar. The objection was already raised during the discussion of the Colloquium but remained unanswered: we must not overlook the very fact that in our reconstruction of PIE syntax we have, fortunately enough, cumulative evidence. Certainly, we do not have at our disposal for the IĒ protolanguage a finite set of sentences in a finite set of cognate contexts; however, cross-linguistic comparison provides us with a large corpus of syntactic patterns. It is perfectly legitimate to compare Gk. *bou-kólos* not only with its phonetic correspondent Skt. *go-cara-* "pasture", but also with the whole set of verbal determinative compounds of the same OV type, *heri-togo*, *ponti-fex*, *vr̥tra-há* etc., ascribing this syntactic pattern to the grammar of PIE. Or, conversely, it matters much less that 'tatpuruṣas' like Gk. *despótēs* and Lt. *pronepōs* are scarcely attested in old texts than the fact that some of them turn out to be ancient and well-established (cp. Skt. *dām-pati-* and the whole *-pati-* series, *prá-ṇapāt-*, etc.): the possibility of building 'tatpuruṣas' can reasonably be traced back to the PIE grammar (see Hoenigswald, this vol., and his concluding remarks).

The *bou-kólos* type of compound is in keeping with a series of facts pointing to the conclusion that PIE must have used the OV type (see W. P. Lehmann 1972; 1977, and this vol.); on the other hand we know that VO compounds like Germ. *Wetz-stein* (Engl. *whet-stone*) are very unusual in the oldest texts (they represent a marked variant of the reverse type) and cross-linguistic parallels are practically unknown: thus, cumulati-

ve evidence makes it highly probable that verbal determinative compounds originally had the OV structure.

We can fully subscribe to Dezső's conclusions when he maintains that

“theories attempting to explain the phenomena of protolanguages must take into consideration not only the fact of the given protolanguage but also the typology of protolanguages”, and that “these suggestions must be checked against the data of various languages” (this vol.: 25).

The conclusion we can tentatively draw from the Colloquium is precisely this spirit of dialectic cooperation of induction and deduction, even in the specific domain of reconstructing the syntax of a protolanguage: not so trivial a conclusion when one thinks of the recent history of linguistics and of the divorce between theory and praxis – a split from which IE studies have suffered in recent years (cp. Lehmann 1977; Ramat 1979). The usefulness of such cooperation is also apparent when dealing with some particular problems which in this way become cases for the general theory. This is the case with the papers by Boeder, C. Justus, and Longobardi, who starting out with different approaches and different theoretical premises reach conclusions that can be reduced to similar frames of reference concerning the syntactic structure of PIE, namely a paratactic type, characteristic of OV languages (see also W.P. Lehmann's and Bednarczuk's contributions, already alluded to).

In the pattern *viśve maruto ye sahāsaḥ* ‘all (the) Maruts who strong’ the DET *ye* preceding the modifier (:apposition) of the head noun (:marutaḥ) is an anaphoric deictic element which can be translated by “who” as well as by “they”³. We are once more confronted with a feature which is consistent with a paratactic type.

Such a broad ‘consensus’ concerning both the results and the methods of the reconstruction procedure does not of course mean a fading ‘consensus omnium’ as if there were no longer any problems. But it must be left to the reader to judge whether beyond the differences on some particular points it will finally be possible to find some lowest common denominator. The task of the editors ends precisely at this point, offering the reader the materials for autonomous criticism.

One final point worth noticing: obviously the contributions cannot claim to represent an exhaustive picture of PIE syntax. Many aspects of the problems of the IE protolanguage have been necessarily neglected,

e.g. the IE ‘Dichtersprache’ – as J. Untermann, the President of the ‘Indogermanische Gesellschaft’, pointed out in his concluding remarks; and maybe some reader will be disappointed by this volume not finding in it this or that topic traditionally dealt with (consider also the limits of an exclusively syntactic approach to the typology of PIE underlined by Chr. Lehmann in this vol.). This criticism could certainly be valid: but it cannot be denied that comparative and typological IE studies have now taken new directions. The editors, who have not the desire to establish a hierarchy among different approaches and different problems concerning PIE, consider it important to present a volume that would have been impossible ten years ago and which testifies this shifting of interest and a useful *rapprochement* of IE studies and general linguistics.

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We give here a full account of the Colloquium program:

Sept. 6:

W.P. Lehmann, The Reconstruction of Compound Sentences in PIE

(Main report)

Chr. Lehmann, The Origin of the **kwi-/kwo-* Relative Clause

G. Longobardi, De quelques structures relatives dans les langues i.-e.

D. Lightfoot, On the Reconstruction of a Proto-Syntax (Main report)

L. Bednarczuk, Origin of the Indo-European Parataxis

K.H. Schmidt, Zur Typologie des Vorindogermanischen (Main report)

L. Dezső, Towards a Grammatical Typology of Protolanguages:

Methodological Considerations

Sept. 7:

C. Justus, Typological Symmetries and Asymmetries in Hittite and IE
Complement Structures (Main report)

P.K. Andersen, On the Reconstruction of the Syntax of Comparison in
PIE

H.M. Hoenigswald, Notes on Word Order, Hypotaxis and Stress in the
Typological History of I.-E. (Main report)

W. Boeder, Zur indogerm. Infinitivkonstruktion

N. Vincent, Iconic and Symbolic Aspects of Syntax: Prospects for Reconstruction (Main report)

J. Tischler, Zur den syntaktischen Grundlagen der Nominalkomposition: die Reihenfolge der Teilglieder

J.E. Rasmussen, Zur Morphosyntax des Themavokals

M. Meier-Brügger, Zur Verwendung von Konjunktiv und Optativ im RV.

Three contributions to the Colloquium have not been included in the Proceedings as not fitting in the general comparative framework of this book. We heartily thank the authors for their participation in the Colloquium.

NOTES

(1) Note that *amābδ*, too, is probably the synthetic result of a previous analytic form with the root **bhū-* ‘to be’ used as a kind of AUX after the main verb: **amā-bhy-ō*. On cyclic developments see Hagège 1978.

(2) It does not matter in this context that owing to other typological and comparative considerations we can assume that the PIE comparative construct was probably

Standard + Adj. + **-ye/os* (: *tē maior*)

and that a motivation for the rise of the construction with a pivot can be found along the lines sketched by Andersen. At the same time the possibility of reconstructing from the cross-linguistic evidence a comparison type *major quam tu* for the proto-language would show the limits of the comparative methods which, because of the lack of a general theory of language change, is not capable ‘per se’ of choosing between the two possibilities *major quam tu* and *tē maior*. If we didn’t have other criteria from cumulative evidence inferred from other facts (see below) we would be unable to make a choice, basing only on a theory of grammar and grammar change. On the other hand, the total disappearance of the *tē maior* - type in any IE language would make it impossible to reconstruct this type for the protolanguage: typological considerations and comparative philology are complementary tools for the reconstructive approach.

(3) Therefore, I cannot agree with Longobardi’s refusal of the hypothesis put forward by Benveniste that the oldest stage of IE relative clauses did not represent subordinate clauses. It is true that relative clauses are attributed to the common proto-language “par toute la tradition de la grammaire comparée” (Longobardi, this vol.: 178) but it is also true that such a tradition is made up by scholars whose mother tongue(s) show(s) definite relative construction(s), so that they have been ready to interpret as relative morphs, the *translation* of which was easily offered by relative pronouns. Yet, as a matter of fact, we do not find a unitary form of relative pronoun to be ascribed to PIE and we do observe in several histories of IE languages the rise of relative pronouns (see, e.g. the Germanic languages: cf. Ramat 1980: § 7.1.4.). What we positively find in the earliest stages of IE languages is a kind of topicalization and focussing via (preposed) deictic elements (cf. Justus 1978, and this vol.): this is consistent with a paratactic, not subordinating type with OV order, although postposition of relative clauses started very early in IE languages (maybe as the first step in the structural shifting SOV → SVO (and VSO): Antinucci-Duranti-Gebert 1979).

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PART ONE:

METHODOLOGICAL QUESTIONS

GRAMMATICAL TYPOLOGY AND PROTOLANGUAGES

LÁSZLÓ DEZSŐ

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1. **SOME METHODOLOGICAL CONSIDERATIONS.** The first point I want to highlight is that both typology and the reconstruction of protolanguages belong to the same complex discipline, to comparative linguistics. Typology represents its “broadest” variant without any limitations; reconstruction and historical comparative linguistics, in general, are restricted to the study of related languages. This is supposed to be a common place but is often forgotten because most linguists have no information about present developments in typology and underestimate its relevance.

One of the most important trends in typology is the tendency towards its integration. It requires the clarification of the theoretical and methodological questions of typology. This entails the reconsideration of fundamental problems of linguistics (theory, methods, laws, metalanguage, etc.) from the point of view of typology. A similar task was fulfilled by H. Paul in historical-comparative linguistics. The specific problems of comparative linguistics of related languages have been reconsidered from time to time.

The study of the relationship between typological and historical comparative linguistics is important from a methodological point of view. These two branches of linguistics represent two kinds of comparative linguistics and are relevant to concrete investigations and must use the

results of each other. This requirement is apparent to typologists but historical comparative linguistics are far less aware of it. Most of the comparative linguists who use the results of typology, however, are experts in the reconstruction of protolanguages (Indo-European, Uralic, Turcic).

I do not intend to deal with the theoretical issues of the relationship between the reconstruction of protolanguages and typology. There is a considerable amount of literature on this subject and even its brief review would go beyond the scope of my paper. I shall approach this question from its typological aspect and arrive at the problems of the typology of protolanguages and their history and make some suggestions concerning their study.

Since there is no typology of protolanguages, I shall, first, present the synchronic and diachronic types of languages and then "go back" to protolanguages. I have chosen three sub-systems of typology: sentence structure, word order and sentence stress because they are relevant for the typological characterization of Indo-European (for details see Dezső 1980).

The typological foundation in section 2 is based on the existing typological literature and on my grammatical archives containing the data of the various languages of the world. The latter will be replaced, I hope, by a new one stored in a form available for processing by a computer. I am inclined, however, to make my own typological statements, if they are supported by the facts of languages I am familiar with and for the non-familiar languages give evidence to confirm or refute them.

2. TYPOLOGICAL FOUNDATION

2.1 Types of sentence structure. G.A. Klimov's contentive (Rus. *contentivnyj*) typology combines sentence typology with the morphology of the devices marking the subject and the object and with the lexical classes of the verbs and the nouns (cf. Klimov 1976). He studied, in detail, the ergative and active contentive types (cf. Klimov 1973, Klimov 1977). I shall arrange his types, highlighting sentence structure, according to the principles of marking the subject or the object. The first main type in the order of Klimov's types, however, will be the one which does not mark either of them.

Main type A non marking consistently either the subject or the

object:

- (1) neutral type lacking morphological devices for marking,
- (2) class type, i.e. languages with noun classes, which differs from the other types in other aspects (cf. Dezsö 1980).

Main type B marking the subject:

- (3) active type marking all subjects,
- (4) ergative type marking the subject of transitive predicates.

Main type C marking the object:

- (5) nominative type marking the object, it has at least two sub-types: partitive and total, non-partitive languages (cf. Dezsö 1980).

Many languages have features of two types, but they can only combine the features of “neighbouring types” according to Klimov (Klimov 1976: 143-144) except the combination of active and nominative ones:

The double types:

- (1) and (2): neutral and class types,
- (2) and (3): class and active types,
- (3) and (4): active and ergative types,
- (3) and (5): active and nominative types,
- (4) and (5): ergative and nominative types.

One of the types can be the dominant one. The double types indicate the possible development from one “neighbouring” type to another. Then the following historical changes are possible (Klimov 1976: 144):

- (1) → (2), (2) → (3), (3) → (4), (3) → (5), (4) → (5).

The study of the history of various languages can prove if all these changes took place.

2.2 Types of word order. The types of word order can be grouped according to the order of subject and object in the basic variant according to my approach:

Main type A (S + O):

- (1) SOV, OSV, and VSO as a rare variant
- (2) SVO, OVS, VSO
- (3) VSO, VOS or SVO

These are the variants specific to a given type. All languages have the basic variant, some use the second, others the second and the third variants in specific contexts. There are languages admitting all six va-

riants. Their study is very important for the understanding of typological changes.

Main type B (O + S):

(4) VOS, the other possible variants are not clarified.

There are languages with double types of word order. They use two variants as basic ones depending on the structure of the verb. The factors determining the use of basic variants are aspect, determination and incorporation connected with determination.

Double type demonstrated in various language

(1) and (2): SOV and SVO

(2) and (3): SVO and VSO

(3) and (4): VSO and VOS.

The double type of SOV and SVO is characteristic of Hungarian: SOV is used in a sentence consisting of a non-perfective verbal and an object without an article; SVO used in a sentence consisting of a non-perfective verb and an object with an article, or in a sentence consisting of a perfective verb and an object with an article. The double word order SOV and SVO is observed in various African languages in e.g. in Ndzhangule (Tucker - Bryan: 241), Moru (Tucker - Bryan: 46ff.) and the use of a given variant often depends on the aspect of the verb. It is interesting that Chuckchee uses SOV in incorporated constructions as opposed to the SVO order of non-incorporated constructions. This fact corresponds to a tendency in Hungarian. In Sora, however, SVO is used in incorporation and SOV in "free" constructions.

The SVO and VSO double type is demonstrated by Zande, Barambu-Pambia (Tucker-Bryan: 151), Koalib-Moro (Tucker-Bryan: 287), Kudugli-Krongo (Tucker-Bryan: 311), Murle (Tucker-Bryan: 389), Nuer (Tucker-Bryan: 437). The use of variants usually depends on the aspect of the verb.

The VSO and VOS is demonstrated in Ge'ez (Dillmann: 444) where VOS is used if V and O is "meant as one notion". Such use corresponds to the use of the SOV variant in Hungarian. There is other possible evidence from other languages.

Historical change from one type to another is possible if the given language has one or more variants in common with an other type. E.g. the development from SOV to SVO was possible only in those Uralic

languages which had free word order because in languages with limited word order, with variants SOV and OSV, there is no possibility for changing to SVO and OVS. The evidence of historical typology of those languages with double type impose further limitations on the possible changes of word-order types.

The following changes are supported by evidence from various languages (with examples):

- SOV → SVO (Indo-European, Uralic)
- SVO → VSO (South-East Pacific area)
- VSO → VOS (South-East Pacific area).

These changes can be complemented by new data. It is striking, however, that so far the changes have a definite direction:

- (1) → (2)
- (2) → (3)
- (3) → (4)

There is no evidence for the change to SOV from any other type. There are, however, protolanguages which had SVO as their basic order (e.g. Bantu); SOV, therefore, cannot be regarded as the “ancient” word-order type of all protolanguages. On the basis of limited geographical distribution and the correlation with VSO type, I suspect, that VOS is a result of a later development. Because of the lack of data, one must be cautious concerning the extrapolation of historical data.

These changes are not obligatory, and there are languages families which have preserved the word-order type of their protolanguages, e.g. the word-order type SOV in all Turcic, SVO in all Bantu languages. Others show a variety of changes, e.g. from Proto-Indo-European, languages with SOV, SVO and VSO type have developped.

I have no room for the analysis of the languages of mixed types; e.g. in Bari SVO basic order is complemented by OSV emphatic order (Tucker - Bryan: 488). I have to disregard the sentences consisting of an indirect object; and/or an adverbial, the types of sentence stressing, etc. (cf. Dezsö 1980). I must, however, call attention to the complex ordering types connected with word-order types. They are implied by Greenberg’s famous universals (Greenberg 1963) and best known in the formulation given by Lehmann (cf. Lehmann 1974). The SOV type is correlated with the OV type of ordering. The VSO is connected with the VO

type of ordering. SVO languages which can be derived from an SVO protolanguage (e.g. Bantu) consistently preserve VO type, but those which originate from an SOV protolanguage (e.g. Indo-European) are inconsistent, applying rules of both types. (The consistency is, of course, relative in typology.) If a VSO language had belonged to an SVO type, this could leave very few or no traces in its VO type of ordering because of a general tendency toward VO type in SVO languages which is particularly strong in the case of a change from SVO to VSO. If the change from SOV directly to VSO were possible, then a language undergoing such development could preserve the traces of OV ordering, but I do not know of such phenomena.

The implications between the types of sentence structure and word order typology are not known. There is only one implication between ergative structure and SOV type (cf. Klimov 1973: 260) which is frequental: ergative type implies SOV order but not vice versa.

2.3 Correlation and Changes of Sentence Stress in Types 1 and 2.

The most important word order variants are usually described in the studies of individual languages and typology could formulate their regularities. Very little is known about the principles of sentence stress. What I am going to say should be considered only as tentative and temporary statements based on the data of Uralic, Altaic and some Indo-European languages.

The usual place of sentence stress and hence that of comment is the position immediately preceding the verb in Type 1. If this position is occupied by an element that cannot be moved from this place (e.g. an indirect object in absolute case in Turcic languages), an element further before the verb can be stressed. If X denotes a stressed element, Y an unstressed one, then:

- (i) XV (ii) XYV

demonstrate the variants of the preverbal stressing principle. In languages having no elements with fixed preverbal position (e.g. in Hungarian) only variant (i) is possible, i.e. a stressed element must immediately precede the verb. In languages with both variants, a stressed element may be separated from the verb under special conditions.

In rigid SOV languages, no element can follow the verb. If a post-verbal position is admitted, the element following the verb is either unstressed or has an emphatic or contrastive stress denoted by X:

(iii) VY (iv) VX

In SVO languages, the usual place of the focused element is after the verb, either in an immediately postverbal position or after an unstressed element:

(i) VX (ii) VYX

In a preverbal position the stress expresses emphasis or contrast in SVO languages with free word order:

(iii) XV

Thus, the principles of stressing the comment are the opposite in SOV and SVO languages. The rules of individual languages are more complicated because they are often of mixed type. If in SOV languages an element does not stand close to the verb, as a result of the semantic cohesion principle, but is stressed, it moves to an immediately preverbal position, if it can do so, and instead of $SAO^{id}O^{dV}$ one finds:

$SAO^{d}O^{id}V$ or $SO^{id}O^{d}AV$ or $AO^{id}O^{d}SV$ etc.,

where O^{id} , A, S are stressed. (Of course, the position of unstressed nominal elements may vary.) If the immediately preverbal position is occupied, the stressed element stands before the verb and tends to the closest possible position; e.g. $SO^{id}AO^{d}V$ or $AO^{id}SO^{d}V$ etc.

The verb may be stressed in any position, but if it is in sentence initial position, it has an obligatory emphasis or contrast.

If a language changes its type of word order from SOV to SVO, it has to change the principles of sentence stress as well. The restricted subtypes of both types have no common variant:

(1) SOV (2) SVO
OSV OVS

and no change of type is possible. The existence of a common variant VSO is of no relevance. A change of type is possible in cases of free word order, when the basic order and the stressing principles of the two types differ but the untypical variants are also used:

(1) SVO (2) SOV
OVS OSV

The untypical variant of SOV languages has one (or, in cases of 4 elements, several) elements in postverbal position. They may be either unstressed or contrasted but they cannot have simple focus.

An SOV language must change its stressing principles to those of

SVO:

SOV: (i) XV (ii) XYV (iii) VX or VY
 to SVO: (i) VX (ii) VYX (iii) XV

The postverbal position must also be a focused one instead of an unstressed or in addition to a contrasted position of SOV type. The preverbal position cannot be used in cases of focus, but it will preserve its emphatic or contrastive status (that was not specially mentioned earlier).

The main principle of preverbal stressing changes to a postverbal one. Thus one arrives at the principles of Russian. Proto-Indo-European and Proto-Slavic were SOV languages, Modern Russian belongs to SVO languages with free word order. It has preserved the stress on the sentence initial verbs: VSO instead of VSO with a stressed S in such an originally SVO language, like Swahili.

3. **ON THE TYPOLOGY OF PROTOLANGUAGES.** In this brief synchronic and diachronic presentation of the three sub-systems of syntax I drew attention to some points relevant to protolanguages. What we have at our disposal at present is apparently unsatisfactory, both from a substantive and a methodological aspect. The research in this field faces the following tasks:

- (1) The completion of synchronic and diachronic typology of various languages and language families,
- (2) the typological characterization of protolanguages, and their history,
- (3) the typological comparison of protolanguages,
- (4) the clarification of the methodology of typology in respect of protolanguages.

At present, one can only make suggestions concerning the typology of protolanguages, more precisely, some types of them.

(a) A protolanguage could be of SOV, SVO and VSO type and have their systems of sentence stress.

(b) A protolanguage with SOV word-order type and with VO type ordering cannot be derived from any other type except the impact of a strong substrate.

(c) An ergative or an ergative-active protolanguage, probably, had SOV order.

(d) The further development of the type of sentence structure and

that of word order and sentence stress in a protolanguage followed the regularities outlined in the previous section.

(e) The history of a protolanguage was also subject to the same regularities.

(f) Theories attempting to explain the phenomena of protolanguages must take into consideration not only the facts of the given protolanguage but also the typology of protolanguages.

I must add that these suggestions must be checked against the data of various languages.

Indo-European belonged to the (B) main type of sentence structure (cf. Klimov 1977: 209ff.). Its development from an active or active-ergative type to a nominative one can be correlated with a similar development in other languages generalized by typology. The characteristics of main type (B), especially those of active languages must be considered in the study of earlier stages of Indo-European. Main type (B) could be the result of a development from main type (A), i.e. from neutral or class type. Early IE could have SOV word-order type and VO type ordering with the corresponding type of sentence stress. May be, none of them resulted from a change of an earlier stage.

These suggestions must be accompanied by precautions. My presentation was extremely brief but even more complete descriptions of these typological sub-systems seem to simplify the diversity of language. Our picture is too beautiful to be true, more precisely, what is known is true but not the whole truth. Human language is an ocean and the typologists of our time can be compared to the early discoverers. Their maps are distorted and full of white spots and they have simple instruments for navigation. They, however, have the spirit of discoverers and the results of their new voyages will correct the old maps.

Moreover, I feel some doubt about the extrapolation of typology into the early stages of protolanguages. We know only the last stage in the long development of human language. This calls for further study but can be the subject of another paper.

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ON RECONSTRUCTING A PROTO-SYNTAX

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It seems to me to be profitable to view the historical evolution of languages in the context of changes in grammars. I take a grammar to be a formal system which characterizes certain structures and certain sentences as well-formed, and relates phonetic form to the logical form over an infinite range of sentences. Such a grammar of a particular language must accord with various restrictive principles which define grammars of natural languages and constitute a theory of grammar, or what is sometimes called Universal Grammar. I also assume that properties of Universal Grammar hold independently of linguistic experience and constitute part of the a priori knowledge which children have and which enable them to master the language to which they happen to be exposed.

Taking this view and confining our attention to syntax, one can show that grammars undergo any different kinds of change in the course of time. A new category may be introduced; new phrase structure rules may emerge; a transformation may be lost, introduced or re-formulated; likewise for a lexical rule; lexical exception features and strict subcategorization frames may change. And so on, together with various combinations of such changes. I have given examples of this kind of thing in Lightfoot 1979a, 1980.

In my own work on syntactic change I have paid particular attention to radical re-analyses, where a variety of simultaneous changes in

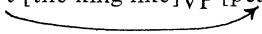
permissible surface structures can be traced to a single change or a small number of closely related changes in a grammar. Such radical re-analyses often illuminate the proper description of some stage of a language and even the proper form of the theory of grammar.

Consider the story of the English modals (Lightfoot 1979a, ch. 2). *Can, could, may, might, must, shall, should, will, would*, once had no properties which distinguished them as a class from other verbs of the language. But in the course of late Old English (OE) and Middle English (ME) changes took place in various parts of the grammar, apparently unrelated but with the effect that these verbs became an identifiable class with distinct properties. Then in the early sixteenth century the exceptionality of this class of verbs was 'institutionalized' by a re-analysis in the grammar: a new category (Modal) was introduced, so that there was a new phrase structure rule (together with a concomitant and necessary reformulation of certain transformations involving negative and interrogative sentences). The evidence for the re-analysis lies in some simultaneous surface changes, all of which can be shown to be manifestations of such a unitary change in the abstract grammar; so the singularity of the change in the grammar follows from the simultaneity of the surface changes (and, of course, certain notions about the class of available grammars). The effect of the change was that earlier exceptionality was eradicated in such a way that the new grammar was more readily attainable, i.e. required a less elaborate triggering experience on the part of the child. The nature of this change, which I have sketched here only in the most general fashion (see Lightfoot 1979a: ch. 2 for details), suggested that the optimal grammar of Modern English should contain the category Modal (as opposed to treating, say, *can* as a verb with various exception features, cf. Ross 1969), and that the theory of grammar should be structured in such a way as to characterize the grammar of fifteenth century English as highly marked, i.e. requiring an elaborate triggering experience for the child to be able to attain it.

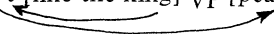
Consider also the change whereby a sentence such as *the king liked the pears*, once construed as object-verb-subject and with *like* meaning 'to cause pleasure for', came to be construed as subject-verb-object and with *like* now meaning 'to derive pleasure from'. In ME one finds sentences like *the king like pears*, where the plural verb form shows that it is

the post-verbal noun which acts as the subject. Such object-verb-subject sentences no longer exist in Modern English, and in fact they became obsolete at the end of the ME period. There is a natural explanation for this change in the framework of current versions of the so-called trace theory of movement rules (part of current versions of generative grammar).

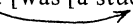
It is clear that early English had an underlying subject-object-verb order, i.e. phrase structure rules along the lines of $S \rightarrow NP VP$, $VP \rightarrow NP V$. Given such phrase structure rules, a sentence *the king like pears* would be derived via a postposing of the subject NP, which would leave behind a 'trace' under current proposals.

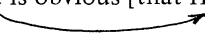
- (1) t [the king like] VP [pears] NP
- 

In the course of ME, these phrase structure rules were replaced by ones which generated an underlying subject-verb-object (for details of this change, see Canale 1978). Under the new phrase structure rules, the subject would be postposed, as in the earlier grammar, and the object would have to be preposed into the position vacated by the subject NP, obliterating the trace.

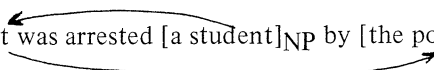
- (2) t [like the king] VP [pears] NP
- 

But such a derivation is impossible under the plausible assumption that traces can be erased only by a designated morpheme and not by a random NP. This assumption was motivated by Dresher & Hornstein (1979) and exploited by Freidin (1978), who labels it the Trace Erasure Principle. Dresher & Hornstein argue that sentences with the expletive *there* or *it* involve rightward movement, as indicated in (3) and (4), and that the trace is covered by a designated morpheme, *there* in (3) and *it* in (4).

- (3) t [was [a student] NP arrested] VP
- 

- (4) t is obvious [that Harry left] S
- 

These are legitimate derivations, but passive sentences cannot be derived by successive postposing of the agent NP and preposing of the object, as is often supposed (5), because again a trace would be erased by a random NP (here *a student*).

- (5)  t was arrested [a student]_{NP} by [the police]_{NP}

The Trace Erasure Principle is a plausible proposal because it guarantees that precisely one lexical item will be associated with each deep structure NP position, and that no second NP can be moved through a position vacated by another NP. If it is correct, it also guarantees that with the new subject-verb-object PS rules, *the king like pears* will no longer be generated, and that *the king liked pears* will no longer be construed as object-verb-subject. Such a sentence would no longer exist in its original meaning...unless, of course, there were a further change in the grammar, such as that case markings were reintroduced to distinguish subject and object NPs. A child, having abducted subject-verb-object PS rules and hearing a sentence *the king liked the pears* would assign it the only structural analysis, i.e. subject-verb-object. If the child also realized the intended meaning of the sentence, namely that the pears pleased the king, he would abduce that *like* meant ‘to derive pleasure from’; somebody of an earlier generation abducting subject-object-verb PS rules, would have a different meaning for *like*, ‘to cause pleasure for’. So the change in the form of the grammar here is that *like* has a different meaning, i.e. it is a change in the lexicon. There is also a change in the functioning of the grammar, in that the derivation of *the king liked the pears* no longer involves a postposing rule. This change is fully explained on the assumption that English developed new PS rules and that the theory of grammar is structured in such a way as to preclude the erasure of traces by random NPs. So the nature of this change suggests that there is something correct about a theory of grammar along these lines.

Under this approach to syntactic change, the point at which such re-analyses occur in the history of a language can illuminate the proper shape of a theory of grammar. One will seek explanations along the lines just illustrated, perhaps suggesting revisions and refinements to the existing theory of grammar in order to improve the available explanations. It is, of course, not the case that all historical changes will have this kind of explanation, because the history of languages is not fully determined. Languages can diverge and undergo individual changes. The fact Latin developed into a variety of different languages suggests that one should not demand of a theory of grammar that it be able to explain every change in

the way that we have just explained the obsolescence of *the king like pears*; some change will arise for idiosyncratic reasons, and so help to distinguish, say, French and Spanish. The history of a language involves by an interaction of factors of change and necessity. It may be a matter of chance, or at least a function of something other than grammatical concerns, that a language develops a certain rule or property; it may be a matter of necessity that a language develops another property. A theory of grammar, defining the class of available grammars, will play a crucial role in the account of necessity factors, as we have just seen, and therefore will play an equally crucial role in our account of historical changes.

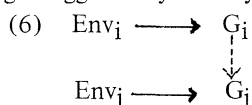
Conversely, facts about historical changes may cast light on the correct form of the theory of grammar, such that some particular historical change might be explained, i.e. be shown to result from some factor of necessity.

So I take a realist stance about grammars. There exists a theory of grammar, which constitutes part of our mental capacity, defining the human species, and I assume that its properties can be discovered. Evidence about its properties will come from a variety of domains, such as the distribution of morphemes, the scope of quantifiers, perhaps even from the way in which linguistic capacities are lost in the event of brain damage, and from historical change. The evidence from historical change will be a function of the ability of the theory to shed light on and explain re-analyses, as with the change in the meaning of *like*. In this way, data from historical change will play a role alongside data from other domains, and the study of linguistic change will be integrated with other aspects of the study of grammar.

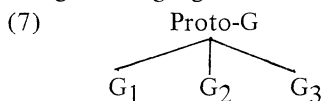
This is a very brief sketch of an approach to syntactic change that I have pursued in recent years. The fact that grammatical re-analyses occur is central to this approach and the source for the insight on grammatical theory. These re-analyses are like catastrophes in the sense of René Thom 1972, 1973, etc. Changes may occur in fairly piecemeal fashion as people introduce new constructions and phrase-types, but there may come a point where something more radical happens, involving a change in the abstract grammar with various surface manifestations, as with the English modals. One might visualize the build-up to such changes rather like a body of water which drops steadily in temperature with no overt change until a point where it change to ice rather suddenly—a Thomian

catastrophe.

The existence of such re-analyses suggest that there can be no formal theory of change in the sense that one might try to define formal constraints on a mapping of one grammar on to another. Grammars emerge in children on exposure to some linguistic environment. If the environment is slightly different, perhaps after one generation, the grammar triggered may be very different, perhaps involving a new category and associated transformational rules. Conversely, it is possible that one might have quite different environments which would trigger grammars differing in only one parameter. There is no one-to-one relation between the difference in triggering environments and the difference in the resulting grammars. While environment_i may trigger grammar_i and, one generation later, environment_j (slightly different from environment_i) may trigger grammar_j, there is no reason to seek rules relating grammar_i and grammar_j(6). Both grammars must conform to the theory of grammar, but, within those limits, may have quite different properties, even though triggered by a fairly similar experience.



Since the grammars are formal objects, for any given pair one can, of course, write rules mapping one into the other, but there is no reason to suppose that those rules will have any interesting properties or meet any interesting formal constraints. Attempts to provide such a typology of change have always been highly permissive; witness Kiparsky 1968, who identified loss, addition, re-ordering and reformulation of rules, and changes in underlying representations—all the logical possibilities of the theory of phonology which he was pre-supposing. If no such formal theory of change is forthcoming, one will not be able to examine the grammars of three related languages and, on the basis of the formal properties of those grammars, infer something about the necessary properties of the grammar of the unattested proto-language from which the three daughter languages have descended.



The problem is that the kinds of re-analyses that I have mentioned constitute cut-offs to historical recapitulation. When exposed to a particular linguistic environment, a child develops the most readily attainable grammar. If that grammar involves PS rules generating verb-object order, the earlier history of the language is irrelevant and the child has no access to whether the language had object-verb order, at some earlier stage.

With this perspective on syntactic change in mind, let us now turn to the issue of reconstruction. I do not suggest that this is the only approach that one can take to the study of syntactic change, but it will provide a useful point of *reference* and a basis for evaluating some of the assumptions on which reconstruction work sometimes proceeds.

A traditional view holds that reconstructed systems play a role in comparative work in that they express relationships precisely. If Greek and Latin are ‘related’, that relationship is expressed by the properties of the parent language from which they descend and the specification of the changes which led to the two daughter languages.

Meillet (1973) expresses this view forcefully (the emphasis is Meillet’s):

la seule réalité à laquelle elle ait affaire, ce sont les correspondances entre les langues attestées. Les correspondances supposent une réalité commune, mais cette réalité reste inconnue, et l’on ne peut pas s’en faire une idée que par des hypothèses, et par des hypothèses invérifiables: la correspondance seule est donc objet de science. On ne peut restituer par la comparaison une langue disparue: la comparaison des langues romanes ne donnerait du Latin vulgaire ni une idée exacte, ni une idée complète... ce que fournit la méthode de la grammaire comparée n’est jamais une restitution de l’indo-européen, tel qu’il a été parlé: c’est rien autre chose qu’un système défini de correspondances entre les langues historiquement attestées.

More recently, a different view has emerged, that one can use changes between a reconstructed system and the daughter languages as a ‘data-base’ for investigating the nature of change, for learning something new about historical change. I wish to show here that this view, while currently fashionable and probably motivating this conference, has no merit.

Over recent years there has been a considerable increase in work on syntactic change, and most of this work has been based on changes affect-

ting unattested proto-languages; witness almost all of the papers in anthologies like Li 1975, 1977 and Steever, Walker & Mufwene 1976.¹ It is remarkable that there has been virtually no serious discussion of an appropriate methodology for syntactic reconstruction, despite the self-evident lack of parallelism with phonological work... and despite the fact that Friedrich 1975, Jacobs 1975 and Lehmann 1974, have offered book-length studies on particular reconstructed systems. Sometimes one sees hints of nagging doubts in the minds of these authors: Friedrich (1975: 6) makes a remarkable reference to ‘the problems, some of them insuperable, of reconstructing proto-syntax at all’, but the insuperable problems are not specified and, whatever they are, are ignored in the remainder of the book—which deals with a reconstructed syntax! Other writers are more subtle, like Dixon (1977:393), who introduces his account of Proto-Australian with the observation that ‘the methodology and data on which these reconstructions are based have not yet been published; only the conclusions are summarized here’; this seems to be usual practice, and three years later the relevant publication, as far as I know, has still not appeared. In general, there seems to be a tacit assumption that syntactic reconstruction can be done in more or less the same fashion as reconstruction of phonological systems – and that if there are methodological differences, their validity is not worth discussing explicitly. All this ignores the questions raised repeatedly about the validity of syntactic reconstruction by Allen 1953, Anttila 1972:355ff, Collinge 1960, Dressler 1971, Hoenigswald 1960:137, King 1969:140, and others.

In many cases it is even unclear what the authors claim to be reconstructing, whether sentences or (fragments of) grammars of the proto-language. Thus a claim that Proto-Indo-European was SOV might be a claim about the underlying order of initial (deep) structures, or a claim about statistical probabilities of surface structures or of sentences. These are quite different things; German has subject-verb-object as its most common word order pattern, but this says nothing about the underlying order, generated by the PS rules, and a good case can be made that underlying order is subject-object-verb.

Reconstruction depends crucially on a concept of possible/impossible changes, and this keeps hypotheses within the bounds of plausibili-

ty. So the neo-grammarians developed notions about natural changes based on their study of changes involving two or more attested stages of some historical development, and they postulated only those kinds of changes when relating their reconstructed proto-systems to the attested daughter languages. In syntax, many authors base such a concept on Greenberg's 1966 work on implicational universals, and 'typological' concept turn out to play crucial role. It is assumed that the proto-language is of a 'consistent' type, and that it is progressing along definable lines to another consistent type. Thus a distinction is drawn between 'consistent' and 'transitional' languages, as if all languages are not in transition from one stage to another. The 'definable lines' by which a language of some consistent type progresses to another consistent type constitute the theory of change distinguishing possible/impossible changes.

A theory of change is assumed to prescribe a universal 'slope', down which languages may slide at varying rates. It specifies the order in which a language of type *a* will lose the properties of that type and acquire, again in a prescribed order, the properties of type *b*. So if one spots a mixed language, with some properties of type *a* and some of type *b*, one can tell whether it is an example of a type *a* language changing to type *b* or vice versa; one can therefore deduce what the parent language must have looked like, where it is no longer attested, simply by listing the properties of the relevant type.

This methodology, used most notably by Lehmann, and referred to several times by him as an **EXPLANATORY** historical syntax (what is being explained?), has been correctly criticized by Friedrich as a 'misuse of typology'; and Watkins (1976:306) notes, in a powerful critique, that Lehmann's theory 'elevates some of Greenberg's extremely interesting quasi-universals to the dubious status of intellectual straitjacket, into which the facts of various Indo-European languages must be fitted willy-nilly, rightly or wrongly'. This sometimes manifests itself in a cavalier treatment of facts. For example, almost all Indo-Europeanists agree on the presence and the precise shape of a relative pronoun (**yo*) and a comparative morpheme (**-tero*) in the parent language. But Lehmann assumes that PIE was of an 'SOV-type' and such languages are alleged to usually lack relative pronouns and comparative morphemes; therefore he does not postulate these items in his reconstruction. For detailed criticisms of

this Procrustean methodology, see Jeffers 1976, Lightfoot 1979a: ch. 3, and Watkins 1976.

One fundamental problem for the whole enterprise is that there is no real data-base for the theory of change as defined, i.e. for establishing the order in which a language of type *a* will develop the properties of a type *b* language. Since the logic is one of induction, the data-base would have to consist of a fairly large set of languages which have shifted from one type to another and where one has documents illustrating the various stages by which that shift took place. Such a data-base does not exist. Latin is a moderately good SOV language by Lehmann's criteria and French a reasonably well-behaved SVO language and the attestation for the intervening stages is about as good as one is ever going to find. This represents one of our best histories, while for the vast majority of the world's languages we have data for no more than 200 years. In the absence of a good number of comparable histories, it would be foolish to assume that the manner in which the Romance languages developed their SVO properties represents what must always happen. Unfortunately such an assumption is rather common.

We know that French has developed from a SOV language and that it now has SVO order. But if the direct object is pronominal, one finds SOV order; compare (8) and (9).

- | | |
|---------------------------------|----------------------|
| (8) <i>j'aime les danseuses</i> | 'I like the dancers' |
| (9) <i>je les aime</i> | 'I like them' |

Some writers assume that this represents a necessary state of affairs and that whenever a language changes its word order type, full lexical NPs will manifest the new order before pronominal NPs; such a statement is incorporated into their theory of change and is used as a basis for reconstructing proto-languages; whenever one spots a language with different word order patterns for lexical and pronominal NPs, one knows that the order of the pronouns represent the earlier state of affairs. Such a data-base is pathetically thin for such a general principle; it also happens to be disconfirmed by another language whose history is attested. Modern Greek has patterns like French, with different orders for lexical and pronominal NPs (10a, b), but here we know that the pronominal order (10b) is a fairly recent innovation.

- (10) a. *o kinigòs skótose tòu líko* 'the hunter killed the wolf'

- b. *o kinigòs tòñ skótose* 'the hunter killed it'

Consequently, it seems fair to say that a theory of change along the lines adopted by Lehmann has no factual base and therefore one is not surprised to find such wildly conflicting claims about the nature of PIE on the part of workers presupposing such a theory; Friedrich claims it to be SVO, Lehmann SOV and Miller 1975 VSO. If this is the state of affairs in the reconstruction of PIE, where at least there is a long tradition of scholarship, a large number of well-described daughter languages (some of which long antedate others), one can imagine the reliability of reconstructed ancestors of American Indian languages (see Li 1977 for several papers pursuing this goal).²

I have suggested that grammars can undergo radical restructuring from one generation to the next, and that there appear to be no formal constraints on the ways in which a grammar may differ from that of the preceding generation, beyond constraints imposed by the theory of grammar; i.e., both grammars must satisfy the limits on a possible grammar of natural language. The grammars must also be triggered by linguistic environments which would normally be fairly similar. The absence of further formal constraints should not be surprising.

Grammars are not transmitted historically, but must be created afresh by each new language learner. Each child hypothesizes or 'abduces' a grammar; this enterprise is quite independent of what his parents hypothesized when they were hypothesizing their grammars one generation earlier. Two slightly different linguistic environments may trigger quite different grammars; conversely, slightly different grammars may generate mutually unintelligible outputs. If two dialects are similar in the class of sentences, it does not follow that their grammars are equally similar; there is no one-to-one correspondence in similarity of grammars and outputs. Therefore, when one considers how languages are learned, one would not expect a child's grammar necessarily to bear any closer formal relationship to that of his parents than what is required by their both falling within the class of possible grammars.

If this is correct, one can deduce very little about the form of a proto-grammar merely through an examination of the formal properties of the daughter grammars. Therefore it is also fallacious to claim (cf. Anttila 1972:358) that, when three or even all the daughter languages show a

particular rule, that rule can be assigned to the proto-grammar. The grammar of a proto-language, like that of any other language, can be constructed *ONLY* on the basis of an interaction between a theory of grammar and the structure and meaning of sentences of the language. Thus claims about a proto-grammar can be made only if a sufficient body of proto-sentences is first established. But we shall see that successful reconstruction of proto-sentences must be very limited, given the nature of syntactic change, and it is most unlikely that there could ever be a sufficient data-base to make interesting claims about the proto-grammar.

The traditional tools for reconstruction are the comparative method — by far the more important — and internal reconstruction; these may be supplemented by various philological techniques and principles of dialect geography, to establish which forms and constructions are innovations and which are relics. Internal reconstruction can do a certain amount of work and assist inferences about an earlier syntax of a given language, if one admits certain assumptions. Lehmann (and many others) work on the assumption that there are universal diachronic principles such that certain changes will take place before others, as discussed above. Other workers assume that morphological patterns will partially recapitulate the syntax of an earlier stage of the language in a consistent way.

Givón (1971) translates the latter assumption into a slogan: 'yesterday's syntax is today's morphology'. The problem with the assumption as a probe into prehistory, is that morphology is notoriously slow to adapt to changing syntax, and may reflect syntactic patterns of such antiquity that the assumption becomes vacuous and untestable. Consider French verb morphology, which forms most tenses with post-stem suffixes: *nous aimons*, *aimions*, and the future *aimerons*. Classical Latin also had suffixes in all tenses: *amamus*, *amabamus*, *amabimus*, *amavimus*. However, the French perfect tense has a pre-verbal auxiliary: *nous avons aimé*, which can be accounted for as preserving the innovative Late Latin pattern *habemus amatum*. The problem is that the Late Latin future was also formed with a pre-verbal auxiliary, *habemus amare*; but this is not preserved in modern French verb morphology. Not only is morphology very slow to adapt to syntactic changes, but it also mirrors earlier patterns only in a selective way. Therefore it is a most unreliable way of reconstructing earlier syntax; each of the individual forms reconstructed

may be accurate, but there is no reason to suppose that they all reflect the same earlier stage – they may each reflect the syntax of 500, 1000, or 2000 years ago (for another illustration, see the discussion of English compound nouns in Lightfoot 1979a: 160).

Internal reconstruction cannot be ruled out a priori, even if the principles actually used as a basis for it are highly questionable. But one must always bear in mind that in any case (as often noted), internal reconstruction is not a genuinely historical method. As Anttila (1972:273) puts it, ‘whatever can be captured on the basis of one language is synchronically present in that language. All we get is a higher level of abstraction...’ Therefore, as a matter of practice, scholars usually apply internal reconstruction simply as a prelude to the comparative method, eliminating the effects of recent changes before the real work begins.³

Turning now to the comparative method, we can begin by emphasizing an obvious but often forgotten point: the items compared should be similar kinds of animals. Watkins (1976) has compared relative sentences dealing with athletic contexts in Hittite, Vedic Sanskrit, and early Greek, and he concludes that ‘the syntactic agreements are so striking and so precise, that we have little choice but to assume that the way you said that sort of thing in Indo-European could not have been very different’. One may factor out the effects of more recent changes by distinguishing archaic from innovative structures and by applying, where possible, the method of internal reconstruction; one may thus arrive at identical structures in the daughter languages, and then apply the comparative method with some confidence.

However, problems arise when the most archaic patterns are not alike in the daughter languages. The success of the comparative method in phonology is a function of the putative regularity of sound change. In genetically related languages, a finite set of phonological segments has regular correspondences, occurring in parallel positions in a finite set of cognate words which are transmitted historically. The alleged regularity of the correspondences permits application of the comparative method; but the method breaks down, as often noted, with analogical changes.

There is no equivalent basis in syntax for the comparative method; there is no finite set of sentences occurring in parallel positions across languages in a finite set of cognate (presumably discourse) contexts. The

sentences of a language are not listable in the way that the inventory of sounds is, and they are not transmitted historically in the same way.⁴

The problem in syntax is that there seem to be no principles (independent of a theory of grammar) which formally define possible changes; syntactic change is in large measure analogical, based on a re-analysis or 're-grammatization' of old surface structure patterns, levelling former distinctions or creating new ones. Such analogical processes will cause as much interference for the usual methods of reconstruction as they do in phonology and other areas of grammar; but in syntax such changes are the normal type, and therefore the methods will be particularly limited.

Jeffers 1976 points to some desperate problems for reconstruction: what does one do when related languages show parallel syntactic patterns with different meanings, or patterns which defy correspondence, or corresponding syntactic patterns without cognate lexical material? For example, what could a comparativist conclude from a demonstration that Hittite had underlying SOV order, Germanic SVO, and Celtic VSO? In phonology Hitt. *p*, Gmc. *f* and Celtic *Ø* allow him to deduce a proto-phoneme; but SOV, SVO and VSO allow no deductions. Again, the IE passive has almost as many formal expressions as there are languages. But even if one cannot reconstruct a morphological realization for the PIE passive, Jeffers asks whether one can fail to recognize a grammatical category which occurs in almost all the daughter languages.

Given the lack of an independent and constrained definition of possible syntactic change, and the consequently limited applicability of internal reconstruction and the comparative method, it will be possible to reconstruct very few proto-sentences. Reconstruction will be possible via the comparative method only where the daughter languages show identical constructions, either in attested forms or in internally reconstructed abstractions. Consequently it is most unlikely that there will ever be a sufficient data-base of proto-sentences to make responsible claims about the proto-grammar.

If there are no formal constraints on possible re-analyses, imposed by a theory of change, then we cannot use such things as a basis for claiming historical reality for our reconstructions; nor is the mapping of one grammar into another of any interest in itself as a method of illuminating possible changes. Reconstruction is not, pace Jeffers (1976:1), 'an

important tool in the investigation of language change' (emphasis supplied); it is the exploitation of acquired knowledge to express genetic relations. The knowledge is acquired from a study of actual changes, where both the earlier and later grammars can be deduced in the usual way from a stock of sentences attested for both stages. We can exploit our knowledge of diachronic syntax or phonology, by applying it to the comparative work of expressing the precise relationship between languages; but we can never discover anything new about the nature of change by examining the relationship between attested languages and our reconstructed abstraction - which we arrive at by internal reconstruction and the comparative method, with all their limitations, well-known in phonology and extensive in syntax. Therefore the mapping between a reconstructed language and its attested daughters is not an appropriate basis for illustrating types of changing, much less for acquiring insight into the nature of change. For this one must look to analyses where one has two attested stages of a language, where one has sufficient recorded sentences to make responsible claims about a plausible grammar or fragment of a grammar.

In the light of this, it is not an appropriate goal for work on syntactic change to try to formulate 'possible diachronic processes' or to reconstruct a proto-syntax. Rather, it is productive to examine historical re-analyses and to show how the point at which they occur might follow from a reasonable theory of grammar. In this way we gain some insight into the nature of change and work on historical change can illuminate the proper form of the theory of grammar and thereby be integrated with work on grammar from other points of view.

FOOTNOTES

- (1) Much of what follows appeared in different form in Lightfoot 1979a or b.
- (2) Not only does a theory of change along these lines have no factual base, but it is also irrelevant for many kinds of changes since it deals only with word order harmonies. There is much that can change in a grammar other than word order harmonies, as illustrated by the sketch of changes affecting the English modals and the meaning of *like*.
- (3) The limitations of internal reconstruction are often illustrated by the phenomena of Lachmann's Law in Latin. In Classical Latin, the stem vowel of the participle of *ago* was long (*āktus*) although it was short in the non-participial forms; this was not true for the participle of *fakyo*, where the stem vowel was short for both kinds of forms. By internal reconstruction one might infer that *ag* + *tus* underwent vowel lengthening in front of the voiced consonant and then assimilation of voicing to give the surface *āktus*. However, we know by the comparative method that voicing assimilation was an old rule, presumably of PIE (since it affects all the daughter languages), whereas vowel lengthening was a later rule specific to Latin. In such cases of conflict between the results of internal reconstruction and the comparative method, the latter wins out as a matter of general course.
- (4) That is, it is reasonable to suppose that a child calls a dog a dog because that is what his parents call a dog. But children do not express a given idea in the way that they do simply because their parents expressed that idea in that way; they may never have heard anybody express that idea before. Here the relationship between their experience and the knowledge they eventually attain is more indirect, being mediated and enriched by the principles of grammar with which the child is endowed a priori. Put differently, it is reasonable to suppose that ME *chapiter* is, in some sense, the same word as NE *chapter*; but it would be bizarre to say that ME *the king like pears* is the same sentences as NE *the king likes pears* or *pears please the king*. This relates to neogrammarian approaches to language change. The neogrammarians held that sound change was *phonetically* conditioned and therefore they wrote rules which mapped the surface phonetic forms of one stage of a language into those of another, later stage. Not all changes can be described in this manner (e.g. the Lachman's Law phenomena of note 3) and some require reference to a more abstract 'morphophonemic' level of analysis. Nonetheless it was a natural approach and allowed scope for a vast amount of useful work cataloguing regular correspondences. But an analogous view of syntax made no sense and the neo-grammarians did not write rules mapping the sentences of one stage of a language into those of a later stage. The lack of a syntactic legacy in any way comparable to what the neogrammarians left to phonologists can be viewed as a consequence of the theory of language which they presupposed. See Lightfoot 1979a for discussion.

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ICONIC AND SYMBOLIC ASPECTS OF SYNTAX:
PROSPECTS FOR RECONSTRUCTION

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1. INTRODUCTION. After years of relative (not to say comparative!) neglect, the study of historical syntax has in the last decade, and more particularly in the last quinquennium, become a central focus of discussion, research and theoretical debate, controversy and speculation. Apart from the increasing number of contributions in the learned journals, there have been several symposia on the topic - see Steever, Walker and Mufwene 1976, Li 1976, 1977 - and monographic treatments. The present Colloquium and its Proceedings will add another item to this rapidly growing list. It will, however, be unique in one respect, namely its concentration on reconstruction rather than change. The dual aspect of all historical work has not been reflected equally in the volumes already referred to above. For example, in Steever *et al* 1976, of 27 contributions, only two (those by Klaiman and Watkins) are specifically on the problems of reconstruction, and surely not coincidentally both of these are concerned with Proto-Indo-European. In another instance, Li 1977, the very title of the symposium meant an exclusive attention to processes of syntactic change. By contrast, a recent volume devoted to problems of reconstruction, Simone & Vignuzzi 1977, contains no study whose specific focus is syntactic. Of course, there is at least one sense in which the elementary statistics just quoted are misleading. Many of the

papers in the volumes mentioned deal with processes of syntactic change in language families such as Niger - Congo, Austronesian, Yuman, and others, where the attested time-depth is minimal, and thus the earlier stages have had to be reconstructed before the process of change can be examined. There is, however, a risky circularity here, as Lightfoot (1979a: 227-8) has recently re-emphasised. If we are dependent on our reconstructive techniques for the details of the change, then first the change is only as reliable as the reconstruction, and second there are obvious dangers in extrapolating from these cases to others. *Ignotum per ignotius* is always a dangerous move. Indo-Europeanists are fortunate in having available a number of well attested but ancient languages against which to measure and evaluate the output of reconstruction, and for this reason the results of this Colloquium should also be of relevance to those working in other language families.

In fact, the relationship between change and reconstruction is a complex one, and one that will concern us at a number of points in the present paper. On a naive view it may seem that they are merely two ways of looking at the same thing - a hill is still a hill whether you are going up or down, so to speak. Lass (1978), however, effectively scotches that notion for phonology, and in ways that provide pointers for the domain of syntax, as will emerge more clearly in the next section. Suffice it to say at this juncture that the relation of reconstruction to change is that of the particular to the general. A theory of change is an account of one aspect of the human language faculty, and therefore a universal field of inquiry. A given reconstruction will involve the application or extrapolation of this theory, in order to achieve greater knowledge about the previously existing state of affairs in one language or language family. The input to the theory of change is provided by those cases where earlier historical stages are available to us without the aid of reconstruction; its results may then legitimately be extrapolated to those languages and families where the historical remains are less ample. In other words, a theory of change is logically prior to a well-established reconstructive methodology. It is in recognition of this need for a secure theoretical base that Lehmann (1974:5) writes:

Recent syntactic studies however have given us the possibility of providing syntactic explanations based on syntactic universals, and accordingly it is now possible to undertake an explanatory syntax of PIE.

Interestingly, one of Lehmann's sternest critics, Lightfoot (1979a:10) expresses a similar opinion:

Given this lack of clear ideas on what constitutes a natural change in syntax, there will be obstacles to reconstructing proto-forms or elements of a grammar of an unattested parent language.

The difference lies in the fact that Lightfoot takes a less optimistic view of the typological method upon which Lehmann largely bases his reconstruction.

It is because of our concurrence with the general attitude expressed by these two scholars that a good part of the present paper will be concerned with theories of change (see section 4), since it is only from an understanding of these that we can proceed to a consideration of the logic and viability of reconstruction. Indeed, the appearance of Lightfoot 1979a has served sharply to focus many of the issues dealt with here, since he is extremely pessimistic in his evaluation of the prospects of syntactic reconstruction and highly critical of other theories of change which have claimed greater success in the reconstructive domain. Our aim, then, will be to try to show that, while some of Lightfoot's critique is well taken, particularly in regard to typology, but less so as far as grammaticalization is concerned, his pessimism is excessive, and that some aspects of his own model coupled with some basic semiotic principles permit us to be more hopeful than he would allow in tackling the task of recovering lost stages of linguistic history. We shall develop our argument in the following way. In the next section we briefly recapitulate the situation in phonology where, for reasons to do with the differing semiotic functions of phonological as opposed to syntactic units, the prospects for reconstruction are considerably brighter. At the same time it will prove possible to extract some methodological pointers which may be applied to syntax, *mutatis mutandis*. The semiotic consideration of the problem is then taken up in section 3. With this much of necessary background, we proceed to the two main sections of the paper, respectively on the theory of change - and in particular grammaticalization - and on reconstruction in syntax. Exemplification will in general be drawn from the Romance languages on the traditional grounds that the success of reconstruction can be checked here against a well-attested, parent or near-parent language.

2. RECONSTRUCTION IN PHONOLOGY. The feasibility of reconstruction in

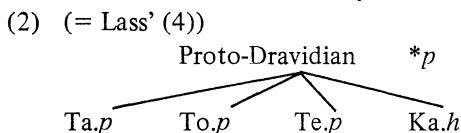
phonology rests primarily on two assumptions, the first being the arbitrariness of the linguistic sign. Since, if we exclude cases of onomatopoeia, there is nothing in the content of a sign which will determine its form, it follows that similar phonetic forms in a number of languages associated with a given concept or meaning will provide input to the comparative method (always, of course, assuming a satisfactory means of eliminating borrowings). It is important in this context that not only do we assume arbitrariness for all the signs of the attested languages upon which we base our reconstruction, but we also assume it for the reconstructed end-product. Indeed, we could scarcely do otherwise without violating the uniformitarian hypothesis that proto-languages are just as much members of the class of natural languages as their daughters. There is no objection in principle to compounding the results of reconstruction by comparing proto-languages, and indeed such a methodology is actively recommended and pursued by Haas 1969. In other words, the techniques of phonological reconstruction do not bring the two halves of a linguistic sign any closer together. It will become clear below that such is not the fortunate case in syntax. Any piece of syntactic reconstruction will necessarily stop at the point where all the elements in the pattern can be motivated in the semantics of an earlier stage.

The procedure is further shored up by the second assumption, that of the regularity of sound change, whereby we can be confident that, having once identified an example of a particular set of sound correspondences, further examples will soon follow. In these two assumptions we see the working of a principle already mentioned in our opening discussion, namely that a theory of language and language change must precede reconstruction. The arbitrariness of the linguistic sign is a finding of general linguistic theory, the regularity hypothesis is a part of most theories of sound change.

A third factor in guiding the historical phonologist is a series of universal constraints emerging from (a) the detailed phonetic investigation of the human vocal tract, and (b) typological generalizations about existing phonological systems in the languages of the world. The significance of these in both change and reconstruction has recently been emphasised by Roger Lass (1978) in an important article. He takes as a simple example the following Dravidian forms (Lass 1978: 246ff):

(1)	Tamil	Todu	Telugu	Kannada	
	<i>pōku</i>	<i>pīx</i>	<i>pōgu</i>	<i>hōgu</i>	‘go’
	<i>pāl</i>	<i>po:s</i>	<i>pālu</i>	<i>hālu</i>	‘milk’
	<i>pal</i>	<i>pas</i>	<i>palhu</i>	<i>hallu</i>	‘tooth’

On the basis of this data we may construct a tree as follows:



Lass is surely right in observing that no reconstructionist would propose a Proto-Dravidian **h*, the obvious reason being that we can motivate a change $p > h$ in terms of our accumulated understanding of sound change, whereas $h > p$ is much less likely, and such a reconstruction would accordingly be required to be supported by a great deal more evidence. However, the matter cannot rest there. Although the reconstructive leap, or ‘projection’ in Lass’ terminology, from *h* back to **p* is acceptable, the corresponding sound change $p > h$ is too great to be taken in a single stage. All the evidence suggests at least one intermediate stage, say *f* or Φ , and perhaps more. The task of the theoretician of change will be to provide an account of the possible stages by which sounds may evolve, including answering such thorny old questions as whether changes are gradual, or even whether the question as such is poorly posed. However, the fact that reconstruction depends on a theory of change, though with its own limitations imposed by the quality and extent of the attestations, means that its output may be regarded as potentially a ‘real’ linguistic unit or system, and hence we may reasonably ask of it whatever we ask of actually occurring language data. This is the view expressed by Jakobson in a well-known remark:

A conflict between the reconstructed state of a language and the general laws which typology discovers makes the reconstruction questionable. (Jakobson 1958: 304)

This is in essence what we mean by the uniformitarian hypothesis.

Lass embeds his treatment of the ‘historicity’ (his term) of reconstruction in the context of a discussion of a celebrated passage from Meillet:

“... ce qui fournit la méthode de la grammaire comparée n’est jamais une restitution de l’indo-européen, tel qu’il a été parlé: *ce n’est rien autre chose qu’un*

système défini de correspondances entre les langues historiquement attestées."
(Meillet 1937: 47, emphasis his)

But, as Lass (1978:248) remarks:

"... this is surely too black-and-white a view: one can believe that reconstructions are more than empty cover-symbols without accepting the absurd proposition that they represent a protolanguage 'tel qu'il a été parlé.'"

The interest of this quotation in the present circumstances is that precisely the same words are cited by Lightfoot (1979a: 165-6) in defence of his view of syntactic reconstruction. His argument runs roughly along the following lines: syntactic reconstruction is a difficult, in some respects if not impossible, task, but that need not worry us, because, even when successfully carried out, reconstruction is merely a way of summing up what we already know, it tells us nothing new. Since the reason he feels this way is because he is sceptical of the value of sequences of 'natural' changes and typological universals in the domain of syntax, and since it is precisely these aids in phonology which validate Lass' rebuttal of Meillet's unduly pessimistic and defeatist position, we should perhaps look a little more closely at the logic behind a 'realist' argument in phonological reconstruction to see what lessons can be learnt for syntax.

As one example we shall take Hopper's (1973) attempt to provide a phonetic characterization of the PIE stop system. Building on the widespread view that the PIE stop system should be reconstructed to contain a three way opposition between *Tenuis*, *Aspiratae* and *Mediae*, conventionally labelled **t* **dh* **d*, Hopper argued that this phonological skeleton could be given phonetic flesh most plausibly if it was accepted that the phonetic values for the three symbols were as follows:

- | | | |
|---------------|------|-----------------------|
| (3) <i>*t</i> | [t] | voiceless unaspirated |
| <i>*d</i> | [t'] | ejective |
| <i>*dh</i> | [d] | murmured |

The reasoning here is that if this reconstruction is admitted, then certain traditionally recognised asymmetries in PIE can be encompassed by independently available generalizations in phonetics and phonology. Thus, the long-standing difficulties attending **b* are easily handled if this segment is assumed to be [p'], since Greenberg (1970) established the generalization that in languages of the world having a glottalized series the one that is most commonly missing is the labial member. Second, the cross-linguistic rarity of sequences of glottalized segments in the same

word or morpheme serves to explain the restriction in PIE forbidding two occurrences of mediae in the same root.

We shall argue below that this same method is applicable in the area of syntax, and that therefore there is hope for the realist approach to reconstruction in that domain too. Indeed, in case the reader was beginning to wonder whether syntax was ever going to re-enter the present discussion, let me insert here a syntactic case closely parallel to Lass' Dravidian example cited above as being the kind of area where syntactic reconstruction might hope to get a foot hold. Consider the following data:

(4)	Italian	Spanish	French
	<i>casa</i> (n.)	<i>casa</i> (n.)	<i>chez</i> (prep.)
	'house'	'house'	'at the house of, among'

Here we have three lexical items which phonological investigations tell us are cognate, but where the syntactic category in French, a preposition, differs from the nominal forms in the other two languages. The question therefore arises as to whether the change has proceeded from noun to preposition or vice versa (provisionally excluding the possibility of a third category from which both are derived). Now it is my bet that just as historical linguists, given the choice between postulating a sound change $p > h$ and its converse $h > p$, would opt for the former, so here they would prefer to regard the French preposition as the innovatory category. And, of course, in the present instance history tells us independently that they would be right so to do. The grounds on which their decision can be justified will concern us in sections 4 and 5, but first we shall turn our attention to the principal semiotic differences between syntax and phonology.

3. SEMIOTIC INTERLUDE. Crucial to the development of our argument will be the distinction between icons and symbols, classically presented in terms of the relationship between the *signifiant* and the *signifié*. In the case of an icon, this connection is one of 'geometrical similarity' (Hockett 1958: 577), or, as Lyons (1977: 105) puts it:

If the relationship is one of form and meaning and the general principle is resemblance of some kind, the form may be described as iconic.

In Pierce's own words, a sign is iconic in that it "may represent its object mainly by similarity" (Buchler 1940: 105). There is, of course, dispute

over the notion of similarity involved here, but even those who question whether icons actually share the properties of their denotata would accept the idea of a parallelism between sign and object. Thus, Eco (1972: 1):

Iconic signs do *not* possess the properties of the represented or denoted object, but may at most be supposed to *reproduce some* of the properties of such object. (emphasis his)
(cf. also the discussion in Eco 1976: 191ff.)

Symbols, on the other hand, exhibit no such parallelism between sign and object, the relationship being rather an essentially conventional one. We associate a symbol with its object because we have learnt to do so. A symbol, then, presupposes someone who is provided with the key to the symbolic relationship, who knows in advance the code by which the symbol is to be interpreted. An icon, in its purest form, is by contrast a sign for which the interpreter can reconstruct the bond between *signifié* and *signifiant* on every occasion of the sign's use. An iconic relationship thus reduces the load of memory, whereas a symbol is in essence an act of memory. Where a symbol is opaque, an icon is transparent in the sense that its interpretation can be constructed by someone who is not privy to its secrets. Compare here Wescott (1971: 416):

... icons, or signs whose correspondence with their referents would presumably be evident even to signalers not privy to the code comprised by the sign .

With these considerations in mind, we can now investigate the respective statuses of syntax and phonology, particularly in the historical dimension. A lexical item is, of course, an arbitrary pairing of sound and meaning (if we exclude the cases of onomatopoeia and sound symbolism), and as such has to be learnt as a whole. The word can be regarded as being passed on in its entirety from one generation to the next, and it is for this reason that it can, going backwards, be reconstructed by the historical linguist, as indicated in the previous section. If we do not know a word, we cannot guess its meaning in isolation because, in Wescott's words, we are 'not privy to the code comprised by the sign'. In the case of sentences, however, the situation is exactly the reverse. They are not (except for certain formulaic utterances) transmitted as unanalysable wholes, but are produced and interpreted anew on each occasion via the rules of grammar. In this sense the relationship between rules of syntax and the meaning of a sentence is an iconic one. The iconicity here is of a

special kind referred to by Peirce as a diagram or logical icon. He writes:

But in the syntax of every language there are logical icons of the kind that are aided by conventional rules. (Buchler 1940: 106)

As a similar example he cites equations in algebra:

Every algebraical equation is an icon, in so far as it *exhibits*, by means of algebraical signs (which are not themselves icons), the relations of the qualities concerned. (Buchler 1940: 107, emphasis Peirce's)

The algebraical signs are not icons, just as the words in a sentence are not, but the structure of both equation and sentence is iconic.¹

Now, this conclusion, if it represents the whole story, is fully supportive of Lightfoot's pessimism regarding the feasibility of syntactic reconstruction, since iconic structures do not permit the *a priori* elimination of independent origin which is an essential preliminary to the comparative method. Fortunately, however, some hope remains. Syntax may be ideally iconic, but a well recognised direction of historical change is from the iconic to the symbolic. Wescott (1971) notes two examples of such a movement in the evolution of symbolic writing systems from pictographic (i.e. iconic) origins, and in the development of conventionalised gestures in the various sign languages of the deaf. The latter case has recently been treated in more detail but in similar vein by Frishberg (1975). It will be our contention here that the synchronic syntax of any language will be a combination of its present iconicity, arising from linguistic and communicative universals, and that symbolicity which is, as it were, the decayed state of past iconicity. By fixing on the latter and working back we can hope to reconstruct at least as far as the former stage of iconicity, which, in favourable cases, may be several stages removed from the present. In other words, in syntax as in phonology, the clue to the past lies in the arbitrary parts of the structures under investigation. The difference lies in the fact that whereas this constitutes almost the entire domain of phonology, in syntax we have the twofold problem of deciding what counts as syntactic arbitrariness and knowing in advance that any given stage of the language will only a relatively small percentage of arbitrariness for the analyst to work on. It is to a discussion of some of these problems that we now turn our attention.

4. GRAMMATICALIZATION. We have already argued (p. 48) that successful or reliable reconstruction presupposes a theory of change, and, in the area of syntax, the type of change which seems most likely to permit

the sort of retracing of steps discussed at the end of the previous section is that traditionally known as grammaticalization.² We begin, therefore, with a descriptive and (partial) theoretical characterization of this venerable concept - at least as old as Meillet's classic paper 'L'évolution des formes grammaticales' (1912), from which we take the definition of grammaticalization as 'le passage d'un mot autonome au rôle d'élément grammatical' (p. 131), and the example of Latin *passus* 'step' yielding French *pas*. The process which seems to be operative here is a kind of semantic 'bleaching' whereby *passus* loses its independent semantic content and acquires its new and more general meaning from the syntactic environment *ne* in which it occurs with increasing frequency. Now, there are undoubtedly theoretical problems involved in making explicit the covert distinction here between grammatical and lexical meaning, an issue to which we intend to return more fully in future work. Something similar, however, is often assumed in generative grammar in the distinction between syntactic and semantic features. Thus, to take an example suggested by my colleague Paul Werth, English words like *to drink*, *smoke*, *talk*, *walk*, etc. are characterizable both syntactically (as verbs) and semantically (in terms of the actions denoted in each case). In expressions such as *to have a drink*, *smoke*, etc. or *to take a walk*, *rest*, *bath*, etc. the semantic content of the verb is, intuitively at least, greatly reduced or transferred to the noun. In terms of the constructs of generative grammar, grammaticalization can be seen as involving two things:

- a) an expansion of the selectional restrictions pertaining to the item in question. As a form becomes grammaticalized, it will increase the range of items with which it may co-occur. Thus, the Archaic Chinese verb *bǎ* 'take hold of' is classified in a modern grammar such as Chao 1968 as a preposition, and by Li & Thompson (1974: 203) as 'a particle functioning as an object case marker'. In the course of this development, *bǎ* has lost the selectional restriction limiting it to co-occurrence with concrete nouns, and it may now be combined with nouns of any semantic class.
- b) a reduction or limitation in the sub-categorization frame. In the case already mentioned, Chao (1968: 749) notes that *bǎ* loses all its verbal properties except the ability to co-occur

with a noun. Similarly, the negative particle *pas* in French is limited to a set of grammatical environments different from, and narrower than, those for the continuing lexical item *le pas*. These two points, particularly the latter, need considerable elaboration before they can be regarded as theoretically secure, but they will suffice to indicate the directions in which we might go in search of a formal account of grammaticalization. I turn now to a consideration of this mechanism of change in relation to the typological accounts of change which have been much to the fore in recent work in diachronic syntax, and which are exploited reconstructively in the present volume by Lehmann and Justus.

Grammaticalization is often discussed in connection with the typological method (TM), that is to say the threefold process of (a) establishing a series of linguistic indices - word order, case marking, nature of morphological processes, etc.; (b) elaborating implicational relations (bi- or unilateral) between these indices; and (c) applying these implications in the work of reconstruction, or in the explanation of linguistic change. The example of Chinese *bǎ* mentioned above is taken from a paper by Li & Thompson (1974) which is a classic exemplification of this approach. There are, however, a number of difficulties with the TM (for critiques, see Jeffers 1976b, Watkins 1976, Lightfoot 1979a and Smith (forthcoming)), in particular those deriving from the statistical nature of the implications which form the basis of the method. Thus, *most* SOV languages have prenominal relative clauses, but, how, then, are we to know that PIE was not one of the minority which did not have prenominal modification? Furthermore, a recent, study by Langdon (1977) suggests that the Yuman languages, which are unanimously SOV but with postnominal relatives, are not exceptional in this regard or internally inconsistent, but that, given other patterns within the languages in question, it would be very difficult for them to be any other than the way they are (and presumably always have been). In view of this, and other, to my mind potentially damaging, criticisms of the TM, it becomes important to separate the process of grammaticalization from its use in typologically oriented approaches. To return to the Chinese case, *bǎ* as a modern case-marker is very plausibly the grammaticalized reflex of an Ancient Chinese full verb regardless of whether one accepts or rejects the frame-

work of typological change within which Li & Thopson (1974) see it. The distinction is doubly necessary because of the superficial similarity which chains of grammaticalization such as the widely recurrent shift from noun or verb to preposition bear to the typological implications and cycles set up by, for example, Li & Thompson (1976) and Venne-mann (1974). They may, however, be distinguished in two principal respects:

(i) *Whereas typological implications are regarded as holding both synchronically and historically, grammaticalization chains are constructs exclusively in the diachronic domain.* Hence, the question of grammatical consistency and the problem of 'inconsistent' languages' (cf. Smith (forthcoming)) does not arise. Ultimately, of course, one hopes to be able to show that syntactic change proceeds along lines of 'grammatical least resistance', just as sound changes seem generally to follow paths determined by the physiology and anatomy of the vocal tract, in which case there would in some sense be correlates of grammaticalization in the structural patterns of languages observable synchronically. The question of the motivation of any change remains, however, a separate one, both in phonetics and syntax (cf. Lass (forthcoming) for some relevant discussion).

An important corollary here is that, once any claims regarding typological consistency in languages are dismissed, then the dubious teleology involved in the idea of target configurations towards which languages can move through time falls away at once. As Lightfoot (1979a: 123) puts it, "grammars practise therapy, not prophylaxis". (On the question of teleology more generally, see Vincent 1978).

(ii) *Chains of grammaticalization are unidirectional or unilateral* - i.e., put at its most general, lexical items may be grammaticalized, but grammatical items do not become lexicalized. Thus, Lord (1976) shows how a verb of saying in a number of Kwa (and other) languages has developed into a complementizer (compare English: *if/say you fail your exam, what will you do?*). The principle of unidirectionality predicts that there will not be cases of complementizers becoming verbs: the route is one way only. The same point is made by Janson (1979: 116) when he writes, "lexical items may become grammatical items and thereby enclitics, but the reverse does not happen."

Admittedly, it is also the case that some typological relations are also unilateral. For example, it is often claimed, following Greenberg, that SVO order entails prepositions rather than postpositions, at least in 'consistent' languages. However, the converse cannot be true, since VSO languages are also characterized by a preference for prepositions. My point is simply that the strongest typological generalizations will be bilateral, but that grammaticalization is by definition unilateral. Indeed, it is precisely this unilaterality which provides predictive power in the area of reconstruction, as we shall argue more extensively in the next section.

Before proceeding, however, to the question of reconstruction, let us pause briefly to consider grammaticalization in the context of the other main contender in the field of historical syntax, the generative model, particularly as developed most recently in Lightfoot 1979a. The relevance here is that Lightfoot, as we have already mentioned, is pessimistic about the possibility of any solid achievements in syntactic reconstruction. The reason is that his theoretical conception of syntactic change as a series of minor and independent changes leading up to a major 'catastrophic' reanalysis does not readily permit the recognition of diachronic chains of the kind we have been referring to under the label of grammaticalization, and which, when reversed, will allow us to achieve some success, at least, in syntactic reconstruction. He argues (1979a: 227-8, cf. also 1979b), against Givón and others, thus:

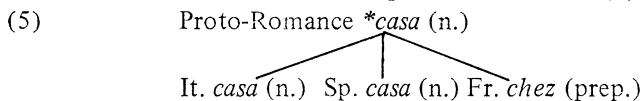
It is sometimes said that we can be guided by supposed 'universals of change' whereby certain kinds of changes are natural and other impossible. However this is specious for two reasons: (a) universals of change can be based only on cases where there are sufficient records to postulate a grammar of an early stage of a language, and this is possible for only a tiny percentage of the world's languages. 'Universals' based on changes in some Indo-European and Semitic languages, Chinese, Tamil, and a few others, should be treated with great caution; (b) even within these language groups, actual changes seem to involve an extremely wide range of formal characteristics and it is by no means clear that there are any limitations, other than those imposed by the theory of grammar.

Both the arguments he offers fail to convince, however. To take the second first, there is something either perverse or overly cautious in the refusal to accept generalizations based on languages of such typological diversity as Indo-European (which of itself subsumes a whole variety of word orders, morphological systems, relative clause patterns, etc.), Semitic, Chinese, Tamil - to which we might add Japanese, Finno-Ugric and

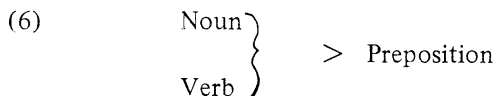
Aztec as having somewhat more than trivial historical attestation. Clearly, we would always like to know more, but in the meantime the aforementioned languages and families would seem to provide a fair basis for generalization. Lightfoot's first point would be refuted if it could be shown that there are principled differences between the kinds of category shift that recur in the histories of languages and the full range of word formation processes that characterise the synchronic state of a given language. Thus, he objects to Givón's analysis of serial verbs in some Niger-Congo languages as being a stage in the development from an earlier full verb to a preposition (as evidenced in some of the related languages) on the grounds that the change might equally well have involved an old preposition being 'upgraded' to a full verb via the serial verb construction, in other words the exact reverse of Givón's claim. As an example of a verb derived from a preposition he cites the English idiom *to up the ante*, to which we can add *to down a drink*. The point is a valid one, and to refute it we need to show that formations such as the English ones are of a different order from the historical cases of verbs turning into prepositions (cf. the case of Chinese *bǎ* mentioned earlier and Romance examples such as Italian *tranne* 'except' from a form of the verb *trarre* 'to pull' plus clitic *ne*). Evidence in favour of this view comes from an interesting study by Baron (1974) in which she shows that children's creativity in word formation during acquisition proceeds along a number of determined paths, and that formations like *to up* and *to down* are characteristic only of adult and not child usage. Naturally, more studies of this kind are necessary to establish the case conclusively, but child language does seem to offer an area where independent confirmation of the historical patterns may be obtained, and thus provide a warrant for the position that there are certain 'natural' directions of syntactic change, which in turn may be reversed when one undertakes reconstruction.

5. **RECONSTRUCTION IN SYNTAX.** If the arguments we have set forth in the preceding section go through, then we can reasonably claim to have established the existence, if not the details of, universal patterns of syntactic drift; what we have called chains of grammaticalization. Examples of these would include the development of prepositions from nouns and verbs, or the kinds of directionality suggested by Givón (1977) and Ultan (1978). Consider once again the case of French *chez* versus Spanish/Ita-

lian *casa* cited as (4) above and repeated here in tree (5):



The warrant for this tree is no longer simply the general intuition of professional reconstructionists (valuable as that is), but the chain of grammaticalization given in (6) and reversed in achieving (5).

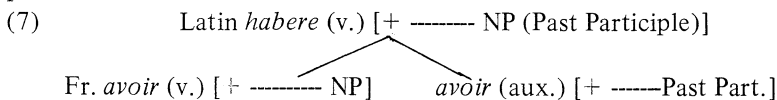


(6) in turn is an inductive generalization drawn from data in the attested histories of a number of languages, both inside and outside the Indo-European family. One of the tasks of future work in historical syntax will be to test this against new data and to establish other similar hierarchies in the interests of providing as rich as possible a framework within which and against the background of which reconstruction may proceed. One may also hope that it will prove possible to integrate these results with work on the synchronic analysis of linguistic categories such as that of Jackendoff (1977: Ch. 3). Only such integration with synchronic theories will allow us to take the important step of turning the inductive generalization into a deductive one. In the meantime, there is still much to be done in the diachronic field.

One problem which immediately presents itself is how to identify the relevant chains in practice. For example, given a preposition in a language under investigation, how do we discover whether its ancestor was a noun or a verb or some other category from which prepositions are known (or indeed not known!) to descend. Compare the case in phonology. Suppose that we come across a language with a segment /h/. Now it is obvious that we cannot reconstruct etyma for all the words containing /h/ simply by replacing /h/ with */p/. It is true that [p] and [h] are end points of a natural phonetic evolution, but at least two other natural sound changes abutting in [h] spring to mind, viz. *s* > *h* and *k* > *x* > *h*. It is also possible (though in this particular instance less likely - cf. Lass 1976: 159) that the sound in question was always /h/. The historical phonologist has two classic alternatives to help decide these questions. On the one hand he may seek for cognates in related languages, on the grounds

that if some of these have, say, /s/ where the language first investigated has /h/ then the relevant sound chain must be $s > h$ and so */s/ is reconstructible. This is, of course, the comparative method, and (5) exemplifies it in relation to syntax. Prepositions may come from either nouns and verbs (and a number of other categories, for that matter), but since the cognates of *chez* are nouns, then it too must have been a noun at some stage.

The other technique available is internal reconstruction (IR), and in this instance it is not so immediately clear how to proceed when we come to syntax. The input to IR traditionally consists of morphophonemic alternations, but if the method is to be generalizable, the problem is to establish a parallel notion of alternation in syntax. A first guess might be to try and come up with a case of complementary distribution within some grammatical construction in the same way that in phonology we look for morphophonemic alternations based on complementary distribution within a paradigm. A case in point here would be the alternative auxiliaries *être* and *avoir* in the French periphrastic perfects, and their cognates in other Romance languages. Excluding examples such as *il a monté les valises*, it can reasonably be argued that the two verbs are in complementary distribution with respect to the set of verbs in the language. This in turn would allow us to establish some more abstract grammatical pattern AUX + STEM + Past Participle Ending, but there would be very little else we could do in the way of analyzing the processes of grammaticalization by which *être* and *avoir* came to be used in these syntagms. An alternative, and in our view more profitable, way of tackling the question would be to look for cases of syntactic split on the assumption that it is split which historically gives rise to the alternations which we can reconstruct from. In other words, rather than looking at the two auxiliaries in our Romance examples, we should concentrate on the process by which auxiliary (and grammaticalized) *avoir* has become dissociated from lexical *avoir*. We may diagram the changes which have taken place as follows:



Here the Latin verb *habere*, which could originally enter into a subcate-

gorization frame with NP's followed by an optional past participle - e.g. *habeo litteras scriptas* - now has split into separate items, both with their own subcategorizations, the first being a main verb co-occurring with a following NP, the second a grammaticalized auxiliary obligatorily combining with the past participle. (Note, incidentally, that a full account of the history here would require mention of the further grammaticalization of *habere* evidenced in the future inflections of many of the modern Romance languages. We omit such a consideration at this point to simplify the exposition, though we shall return to the question of the futures below.)

To reconstruct a situation like the one exemplified with *habere* requires us in the first instance to identify the alternating lexical/grammatical pair, not always a straightforward task if the two forms have undergone differential phonetic evolution, and/or the grammaticalized form has become a clitic or even an inflectional affix. However, once the internal cognates have been set up, then on the principle outlined on p. 58 that grammatical items derive from lexical items and not vice versa the earlier stage is reconstructible via the lexical member of the alternation.

It is possible at this point to see the relevance of the semiotic principles discussed in section 3. Recall that we argued there that the contribution of the parts to the syntax of the whole sentence was a diagram (in Peirce's sense) of the way the meanings of the parts could be built up into a semantic interpretation/representation for the sentence. Thus, the role of the lexical *habere*, though it is itself an arbitrary symbol, is an iconic one - it is a part which contributes to the meaning of the whole. By contrast, in a grammaticalized construction such as *avoir* + past participle as the expression of the past tense, the *avoir* is simply an arbitrary part of a larger semantico-syntactic unit. Such a symbolic state is ripe material for input to reconstruction, but once the earlier stage of iconicity, at which all the parts can be motivated in terms of the whole, has been reached, no further reconstruction is possible. Similarly, we can produce an etymon for the Romance futures such as French *aimerai*, *-as*, *-a*, etc., but we cannot reach behind the reconstructed *amare habeo* to the Latin inflectional futures like *amabo*, etc. Indeed, it is at least possible in principle that Latin had no overtly marked future at all, as is the case in modern Sicilian.³

One way of demonstrating that, although we can and do reconstruct the syntagm *amare habeo* for Latin, this was not the Latin way of expressing the future - whatever the latter might have been, is by a combination of internal reconstruction and the internal method. Having established say for French or Italian that the future goes back to a construction with *habere* and the infinitive, we can examine ways of expressing the future in related languages. If these, as they do, reveal the same pattern but with non-cognate material - e.g. *voleo amare* underlying modern Rumanian, and *debeo amare* in parts of Sardinia - it is legitimate to include that we are dealing with an innovation, a new pattern existent but differentially realized in the daughter languages. This last point is, of course, by no means original, but it has arisen more than once in the recent literature. Lehmann (1974) adopts a similar line in arguing that it is not possible to reconstruct the passive as a *grammatical* category in PIE, a view which Lightfoot (1979a: 165, note 1) criticizes, though on no very good grounds. Jeffers (1976a: 6) voices some relevant queries in this domain. The present paper can be seen as the beginnings of an answer to some of those questions. Future research, I would suggest, need to concern itself with, among other things, the properties associated with lexical splits and alternations between lexicon and grammar. Two major pre-requisites for such work would seem to be:

- a) to establish inventories of possible grammaticalization chains, perhaps in the context of a universal theory of semantic and morphosyntactic features;
- b) to establish inventories of possible construction types - e.g. passive, comparative, conditional, etc. - and their various realizations in the languages of the world.

A start is made on this latter enterprise in many of the articles in the four volumes of *Universals of Language* recently edited by Joseph Greenberg. In the more specifically diachronic context we may cite the contributions to the present conference of Justus and Andersen, the latter arriving at very similar conclusions to our own, though having departed from a particular study rather than a general methodological attack on the problem such as we have tried to mount. It is to be hoped that, as elsewhere in the study of language, the combination of both these approaches will yield new insights into the linguistic past.

FOOTNOTES

(1) I was not able to consult until after the composition of this paper a recent lengthy article by Friedrich (1979) on (non-) arbitrariness in natural languages, and also the extremely interesting reflections contained in Bates *et al* (1979).

(2) More extensive treatment of the question of grammaticalization is to be found in Givón (1979), and the issues were insightfully discussed by Elizabeth Traugott in a paper presented at the English Historical Linguistics Conference in Durham, England, Sept. 24 - 27, 1979.

(3) I am grateful to Lorenzo Renzi for reminding me of this point in discussion.

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NOTES ON RECONSTRUCTION, WORD-ORDER, AND STRESS

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It has recently been said that 'historical syntax must be pursued as has been done for historical phonology', and that 'to develop historical syntax in this way the same methods are available as for historical phonology: the comparative method and the method of internal reconstruction'. Lehmann, from whom these words are taken, goes on to call these methods 'simple [but] perhaps deceptively so'.¹ With this one must agree. Part of the 'deceptiveness' lies in the fact that into the application of these methods even on their home ground there enter more hidden assumptions than are apparent. Nor are these assumptions 'purely formal'; they have content, and the content is typological or universals-oriented. In syntax, typological assumptions play not only a much larger but also a different role, and one of the reasons why comparative syntax has, to everybody's vociferous and long-standing complaint, lagged behind to such an extent is that the formalism of the two classical procedures, such as it is, cannot simply be repeated mechanically on the so-called higher level. However, while mechanical extension is one thing, a true profoundly congenial counterpart would be quite another.

The objects which the classical comparative method reconstructs are morphs and morph sequences – that is, the phonological shapes of roots, affixes, etcetera. The means whereby this is done is that of making phoneme correspondences from descendant language to descendant lan-

guage into allophones of the proto-language. For example, a simplistic account of the reconstruction of the classical * ∂ says that it represents the correspondence of Skt *i* with Lt *a* and that * ∂ is separate from **i* and from **a* because the correspondences *i/i* and *a/a* each contrast with this *i/a*. The remarkable thing about this particular reconstruction is that it makes the proto-language typologically different from its descendants, and furthermore that this conclusion is not reached -- ostensibly at least from any typological preconceptions, either. For instance, there is no presumption that vowel systems are always shrinking, or that excess vowels of some sort such as this are necessarily doomed. In fact, the reconstruction was done in contravention of a very powerful unspoken preconception under which a proto-language ought not to look too different from its daughter languages. It is clear, in any event, that such preconceptions can be contradictory and arbitrary, since they might allow us to argue both (1) that a six-vowel system is plausible even if the descendants have fewer vowels (because such a system is 'unstable'), and (2) that a six-vowel system is **not** plausible as the ancestor of a collection of lesser systems (because of the consensus among the latter). The comparative method, on the other hand, is neither contradictory nor arbitrary so far as it goes. For this reason we follow it wherever it leads us.

The strictly formal core of the comparative method (as distinct from its total application) addresses itself to the establishment of contrast or non-homonymy. The three proto-phonemes instanced above are defined as different from one another; what their features were may be seen, if one wishes, as a separate question. It is in dealing with this question, especially in cases where there is no physical consensus among the descendants, that typological maxims, explicit or implicit, are resorted to. It is customary here to impose two constraints: the proto-language must be typologically acceptable in a static sense, and the sound changes must be typologically acceptable as processes.

Now let us look at some successful, generally accepted examples of higher-level reconstruction, where the objects are on the other side of the *double articulation*, i.e. in semantics and in grammar. It is immediately obvious, as we have said, that there can be no mechanical duplication. For this it is sufficient to remember that on the lower level the traditional comparative method (and internal reconstruction with it) rest on

semantic identity and translatability: it is indeed the Indo-European word for this and that – say, ‘father’ or ‘2’ – which we claim we can retrieve. There can be no such appeal from the higher level in turn, because there is nothing to appeal to. What, then, are higher-level reconstructions like?

First, take the neuter plural subject with its singular concord, as in Gk. *tà zôa trékhei* ‘animals run’. The construction is mandatory in Attic and optional in Homer (perhaps with a semantic nuance). In other Indo-European languages there are only uncertain traces. The phenomenon is generally considered a retention. Why? First of all, to put it crassly, it looks unmodern: it is outlandish and is found in a relatively old body of texts, secondly, and more specifically, the more or less linear descendancy of Attic, namely later Greek, lacks it. Both arguments are typological. The second, in particular, appeals to a peculiar but ubiquitous and in actual fact almost unavoidable conviction, to wit, that lines of descent are monotone; we look for directionality, and we permit ourselves to extrapolate backward. If singular concord for neuters plural is a relative archaism in historical times it should not figure as a relative innovation in prehistoric times. [It is instructive for a moment to switch back to the more secure lower level of submorphemic reconstruction. Philosophical-minded observers from the early days to Bonfante and Samuels² have said or implied that sounds should not revert – as if, say, $\beta > t$ in part of Scandinavian were not precisely such a reversal with regard to the earlier $t > \beta$. Now, the comparative method enables us to ignore such pseudo-problems as irrelevant; we disdain to recognize the feature-wise sameness of the Indo-European ‘*t*’ and the Scandinavian ‘*t*’ as well defined.]. Another question that ominously occurs to us has to do with the mixed record in Homer. Is his usage impure? Is it perhaps necessarily ‘transitional’, and hence a testimony to the separate position, in a family tree, of the epic dialect? None of this reasoning is strong, and none of it, incidentally, resembles the core of the comparative method in the least. The clincher, as we know, is very different. It consists in Johannes Schmidt’s idea that the inflection of neuter plural and the collective derivation by means of - *a* (or rather -e-H₂)³ were once identical. If we analyze his reasoning it seems to say that there are certain descendant languages (I) with a distinction between neuters plural and singulars feminine – say, Latin, with *tēla*, gen. *tēlōrum* ‘missiles’ and *tēla*, gen. *tēlae* ‘tissue’ – and

there are certain other descendant languages ('II') -- Attic Greek -- in which the two come close to not being distinct syntactically. Which class of languages is the retaining, which the innovating class? Typology aside, there is a bit of formal evidence. Even the class-I languages, with their greater and more institutionalized grammatical diversification exhibit a pair of homonyms or, if we prefer, a polyseme. Unless the homonymy is an accident (for example, a phonological accident), it points to the same earlier morphemic-semantic homogeneity that our class-II language shows. It so happens that for very interesting reasons this is not quite enough as internal reconstruction (within class-I languages taken by themselves -- the early history of our field is strewn with empty examples of speculative identification of homonymous morphs). But in a framework of comparison it has strenght. The example is labored and artificially simplified. But the simplification aims to bring out the essential part of Schmidt's reasoning and its affinity to the lower-level comparative method we know.

Contrariwise, consider Indo-European case morphology and syncretism. (Once again, we schematize to the point of excess). It is a fact, on the ordinary level of the paradigm that there is a 'trend' toward the reduction case categories -- a typological judgment if there ever was one. Once again, this judgment can be reinforced and elevated into something like a reconstruction by extrapolation backward. Let us single out Italic. The Romance languages have become 'analytic'. On the plea that a language with a certain number of case suffixes is halfway between an even more richly synthetic and an analytic one, we might be tempted to look for even more case distinction in the prehistory of Latin. Sanskrit or the oldest Slavic have indeed a larger number of such distinctions. It is, therefore, natural to think of Sanskrit as retentive and of Latin as innovating. But, once more, the most powerful support comes from the structure of the morpheme. Not only is it found that Sanskrit locative expressions are different from ablative expressions while the two are on the whole alike in Latin, but the Latin 'ablative' morpheme is both polysemous and suppletively allomorphic, in that both *pede* (with the suffixal morph *-e*) and *hortō* (*d*) (with *:-d*) can be 'ablatives of place' as well as 'ablatives of separation'; the two endings are merely predictable from the stem and have nothing to do with grammatical distinction. The added

fact that they are suppletive rather than morphologically related suggests a back-ground of syncretism, as a bit of quasi-internal reconstruction, while the crowning comparative argument is furnished by the discovery of the etymological identity of the Latin allomorphs with the two contrasting morphs of Sanskrit, *-i* ‘locative’ and *-t* (*-āt*) ‘ablative’. In some ways this example is the converse of the preceding, where the reasoning was from homonymy rather than from suppletion. It is as if both homonymy and suppletion were classified as freakish conditions, representing somewhat unnatural extensions from the central way in which morphemes function. Both must be suspected of being innovations, even on internal, non-comparative grounds. The quasi-comparative procedure which we have sketched here confirms certain homonyms as due to the kind of innovation which one would call differentiation (*tēla* **telā*), and it confirms certain suppletions as due to the innovation called syncretism.

What we have here is, to be sure, a rather precarious device, in at least two respects. First, it rests on untypical, even marginal phenomena such as suppletion. It is, in sober fact, quite easy to construct change processes which will never leave a configuration from which to reconstruct. Secondly, our reconstructions, insofar as they are ‘purely formal’ and not typological are curiously superficial and almost miss the point – they cannot shake off their morphic, subdistinctive nature. In deep syntax, it is rightly argued, Latin still has a larger number of case categories and it may likewise be said that the category of collectives (with *-H₂*) maintained a retrievable relationship with the plural and that the splitting off occurred only in the surface. It is perhaps true that just as the classical comparative method recovers proto-morphs primarily, and the phonological ‘system’ only secondarily as it emerges from the accumulation, so the quasi-comparison which we have adumbrated here leads to a recovery of the essential syntax and morphology only indirectly. Of course, historians very largely deal with surface features. If universals are all they are cracked up to be, our commitment to uniformitarianism dooms us to do just that.

A great deal could be learned from scrutinizing other convincing instances of syntactic-semantic-morphemic reconstruction in Indo-European and elsewhere, such as the rise of the definite and the indefinite article, the history of subordination; the relationship about types of com-

pounds also comes to mind. I shall not attempt to go on in full, but a few observations on one or the other of these may be in order.

Compounds (to start with the last-named topic) with non-verbal second components show, in Indo-European languages, a trend from exocentricity to endocentricity; for instance, from *bahuvrīhi* to *tatpuruṣa*. The trend could be interpolated backward; there is neither reward nor penalty for doing so. Universals-oriented thinking may or may not help; the fact seems to be that endocentric compounding is a rather common thing so that the reconstruction of a state in which it did not exist might even be mildly counterindicated. An interesting line of thought could, however, be pursued by exploring the possibility of an implicational typology, chronologically staggered in the sense that long after the coming to fruition of endocentric compounding endocentric suffixation also makes its appearance in Indo-European: diminutives, pejoratives, comparatives of adjectives and whatnot come to be viable in some subfamilies. On the whole, this typological goal is reached in quite different ways. But there are exceptions: some particular *tatpuruṣas* recur in old text from one language to another in such a fashion as to form a telling consensus (among the famous examples are, of course, Gk *des-pót(ēs)* Skt *dām-pati-*; Lt *pro-nepōs* Skt *prá-ṇapāt-*, etc.).⁴ To convert this consensus into a reconstruction involves very little inference beyond the ruling out of independent identical innovation. It also casts a pall over the idea of statistics since it matters much less that *tatpuruṣas* are few than that some of them turn out to be ancient and well established.

On occasion it has been possible to show that this very fact enables us to put our finger on the particular machinery which led to the spread of *tatpuruṣas*. It may also show us a way of sharpening the concepts whereby we hope to marshal our typological judgments. Are we so sure that merely to distinguish exocentrica from endocentrica is enough, when the record suggests that **SOME** of the latter—namely those forming the ancient core of *tatpuruṣas*—were quite different from others? What was the essence of the analogic processes which bring it about in retrospect that the ‘type’ has spread? After all, in recording a typological upheaval we may sometimes wish to recognize a collection of diversified mechanisms converging, providentially as it were, toward a simple and uncontroversially formulated target.

By way of summary it would be fair to say that some of the reasoning employed in grammatical, semantic, and syntactic reconstruction simply employs a consensus; some of it is a little analogous (though not crudely and mechanically so) to the ‘comparative’ method we know from the lower phonemic level; and some of it is typological. Consensus, as Dressler would say, is mere *parole*, whereas we are interested in *langue*, or so he hints.⁵ The quasi-comparative approach is of great interest, but it depends on idiosyncratic traits like homonymy and suppletion. This means something that no one needs to be told at this point, namely that the typological argument looms immeasurably larger. For better or for worse, our reconstruction can be little better than the state of our typological competence. Some people find this dismaying; others find it challenging.

I propose to turn to a particular area in which, to say this right away, quasi-comparative reasoning fails us altogether and in which it is, therefore, necessary to concentrate on the typology and see whether it is strong enough to keep the historical interpretation from becoming arbitrary or mistaken. This is the area of stresses and intonations.

There are some famous examples of past work in which stresses and innovations are somehow involved. Wackernagel reconstructs an earlier meaning ‘why’ for Lt. *quia* ‘because’.⁶ This, perhaps, is again a bit of *parole*. By a simple kind of internal reconstruction (from *quianam*, and also from the other occurrences of the stem *qui* - and the ending - *a*) the fact of semantic change is quickly established. Latinists, from the early beginnings to Löfstedt, have pointed out ‘parallels’ – that is, typological evidence – galore, and Wackernagel made the observation that in Sanskrit ‘die Ankündigung einer Begründung durch eine Frage... sehr beliebt ist’.⁷ But there is still something missing here. To Leumann, the machinery of the shift is in the nature of a discontinuité from speaker to hearer; a stylistic device consisting in a bit of parataxis is interpreted – misinterpreted, in fact – as hypotactic. That much is spelled out. In the background, however, there are other implications. One, perhaps, is that hypotaxis is generally, or at least typically, reducible to parataxis. Another one is surely present in the mind of anyone who has thought about such cases in a concrete way (though it has been made explicit only rarely, as by Samuels and by Lightfoot).⁸ It concerns intonation and other supra-

segmentals. The question is this: What must the prevailing sentence and phrase intonations have been like in order for it to be plausible that a paratactic, rhetorically charged sequence: statement - question addressed to self - statement, be picked up by some interlocutors as one complex sentence? It could be a case of subtle dialect borrowing. It could also be that such a switch presupposes the existence of particular conditions in which an intonational distinction is neutralized, and hence ambiguous. The fact that our texts do not show such intonations should not give us licence to accept them as if our problems were graphic and not linguistic ones – a reflection which should be superfluous but which is, in fact, only too necessary.

It seems that we are at a peculiar juncture. Synchronically, a good deal of effort has lately been expended on problems of sentence stress, to which I shall refer as **STRESSING** (to distinguish it from lexical stress). Though limited to very few languages, alas, the work should lead to some typological insights. One likely aspect of this consists in implicational connections between types of stressing and word ordering. Unlike stressing, word ordering is not only not hidden from the historian; if anything, the evidence is over-rich because it is so much on the surface. Word-order has not exactly been neglected by students of universals and typology; but the going has been rough, and there is a suspicion that the counters chosen – say, O V S.. – are not by themselves entirely congenial to the phenomena. In the end, of course, the appropriateness of our elementary concepts can emerge only in circular fashion from the processes themselves.

Let me, therefore, propose that we start by considering some word-order problems and some stressing problems in conjunction. This could be a case of approaching *ignotum per notius* and might lead to interpretations of word-order that meet at least some of the criticisms that are being leveled in some quarters against a word-order typology conceived of as absolute and self-contained. Here one cannot help thinking of Wackernagel's ideas on German word-order, its overt relation to subordination, and its reconstructed relationship with enclisis – a connection which was not quite fully elaborated and which seems to have turned out to be factually untenable into the bargain. The idea was, of course, that verbs come in second place in main clauses and at the end in dependent

clauses. In the Vedas verbs in main clauses are unaccented, and as ‘enclitics’ could have been expected to stand in second place. In dependent clauses they are at any rate non-enclitic. There seemed to be a further understanding (though systematic interest began to flag at this point) that end position could mean stressed position. There is a correlation here between word-order, enclisis, and perhaps syntactic stressing on the one hand, and the functioning of the Vedic tone. At first blush there seem to be two dependent questions: one, whether this is a case of consensus which could be used as a basis for reconstruction, and, second, whether or not there is evidence in the recorded history of Modern German which simply vetoes such a reconstruction. The latter seems to be the case – apparently, the word-order distinction between main clauses and dependent clauses is not a lone holdover in Modern German but an innovation.⁹ This is, then, one of those embarrassments that are so well known in historical reconstruction. It is possibly for very good typological reasons that German has recreated a standard syndrome – by accidental duplication, as any good neogrammarian is duty-bound to put it. There is, in fact, a third question, and it is this: is the peculiar correlation between a given sentence structure and a given order-plus-stressing datum (not just a given datum!) a going type?

It would be rash to claim that the universals of stressings and intonations are well understood, even as a matter of synchrony. Bolinger’s reader, *Intonation*, bears this out, in spite of the flashes of insight which it contains, and in spite of the excellent ideas which have been contributed further by Bierwisch, Bresnan, Chomsky and Halle, Heidolph, Kiparsky, Schmerling, Szwedek, and many others.¹⁰ Speaking of Indo-European languages, and of German in particular, how would we go about recording some of their properties in such a way as to serve the purposes of typological comparison, and ultimately also those of ‘comparative’ and internal reconstruction?

Before we start we must, incidentally, realize that there is of course nothing sacrosanct about SENTENCES. Whatever their transformational status may be, sentences may be seen as stretches with one defining simple, low-level property: a sentence is a stretch such that there occurs over it a minimum free intonation.¹¹ It is surely in this way and only in this way that the stretch *He didn’t feel well, but he went anyway* is one

sentence, while *He didn't feel well. But he went anyway* is two sentences. It is not impossible that this is a true universal. Sentences are not the same thing as one-sentence discourses -- a confusion which can make itself felt whenever the acceptability of a putative sentence is up for judgment.

There is probably much more diversity with regard to the stretches which 'phonemically' (that is, in terms of 'autonomous' notation) carry one loudest stressing. In a good many language structures, including German, there seems to be a hierarchy of distinct stress levels in existence, with some of them representing the downgrading of loudest stresses in the course of embedding processes. There is no reason to believe that this state of affairs is uniform among the languages of the world, though equivalents of an interesting sort may be widespread and even general.

Then, by way of another non-universal, German has lexical stress, so-called. Moreover, some constructs have relatively well-fixed lexical stresses. For instance, compounds like *Viehzüchter*, *Aufstand*, *Strassenkind*, but also *Stéhauf*, have their stress on the first member, something which is transformationally probably quite unconnected with the stressing in the underlying phrases *züchtet Vieh*, *steht áuf*, *Kinder áuf der Strásse*.

There is a widespread impression that there is an 'unmarked' stressing in German, under which loudness appears as far to the rightward as certain overriding rules will allow. Apparently, such stressing is indeed appropriate in certain one-sentence discourses -- sentences 'in isolation' -- not because it is devoid of any contextual implication, but because one-sentence discourses, as a matter of descriptive fact, carry certain particular implications by preference. This may be quibbling; but the term 'unmarked' is simply not a happy choice, since a sentence like *Ein Mánn steht an der Tür* (perhaps in answer to the question *Was ist los?*, or in sequence with the statement *Etwas is nicht geheuer*) is surely not 'marked' either in the sense of being 'abnormal' or even only in the sense of being loaded with 'extra of information'. In some complicated way this must be a question 'new' versus 'given' material. It seems that, in some languages, phonemic phrases (or stretches characterized by the presence of one loudest or otherwise defined stressing) tend to be constructed in such a way that most of the material is anaphoric (or 'given' in some

other fashion). It may be **OVERTLY** anaphoric (when earlier material is repeated), or it may be **DISTINCTIVELY** anaphoric (when the lack of a stressing **SHOWS** that it is considered as ‘given’). In either case the stressing falls on the non-anaphoric remainder. At this point certain word-order provisions come into play, with the effect that there are at least two kinds of sentence (to use this term despite its too narrow connotation). In **ONE** class the non-anaphoric item, with its stressing, is placed as far to the right as the word-order provision will allow. The most telling cases are perhaps those where the rules still leave a choice and where, in addition, something like non-anaphoric standing is marked redundantly, as by an indefinite article: *Er hat das Geheimnis einem Kollégen anvertraut*, but *Er hat seinem Kollegen ein Geheimnis anvertraut*. Also, inversion tends to function in this way. It has been pointed out that *An der Tür steht ein Mánn* (and also *Natürlich steht an der Tür ein Mánn.*) is in order when the question was *Wer steht an der Tür?* or the like. In particular, if the verb phrase as a whole is the non-anaphoric item, it is the ‘object’ (the term to be taken much more widely than is usually done in S: V: O: schemes) which receives the stressing. Some types of morpheme, such as many pronouns, are habitually anaphoric in keeping with their lexical meaning; but their status is best considered as a special case of the broader anaphoric condition, rather than as an intrinsic property.

Sentences of the **OTHER** class also conform to the word-order rules, of course, but their stressing is not on the rightmost location. On the whole this will have to be interpreted as an indication of how anaphoric and non-anaphoric material is distributed, though the complications are many and it is not always clear to what particular constructions the term ‘emphatic’ has under these circumstances been applied. If it were said that *Mein Váter kauft ein Haus* (with a non-anaphoric (?) but unstressed *ein* cooccurring) is emphatic in a sense in which *Mein Váter kauft das Haus* is not, it could probably be retorted that the difference is rather in the fact that in the first sentence the verb phrase as a whole is non-anaphoric, while in the second case, the object has a separate non-anaphoric status within the likewise non-anaphoric verb phrase. What it sometimes distinguished from emphatic stress and called ‘contrastive’ stress proper seems to involve the cutting up of a phonemic phrase or clause into two, as in both *Mein Váter | kauft ein Háus* and *Mein Váter | kauft das Háus*

with two loud stresses each. Perhaps it needs repeating that we are seeing a language here in which there is no such thing as a distinctive or emphatic stress level so far as bi-unique phonemics is concerned. Emphasis and contrastive stresses are the ordinary loudest stresses with a special location.

It is indeed characteristic of this language that the verb itself (as distinct from the verb phrase) takes a beating. It rarely gets a loud phonemic stress except when the word order rules make it occur rightmost and the remaining material is either anaphoric (as can be tested with pronouns as objects) or zero (as for instance with intransitives). It is worth noting that where there is a separable preverb and an object and where the object is anaphoric, it is of course the preverb that gets the phonemic stress (*er macht die Tür auf* = *er öffnet die Tür*, but *er macht die Tür auf* = *er öffnet die Tür*, whereas *er macht die Tür auf* amounts to a mere metalinguistic spelling-out as when lexically stressless syllables are artificially restressed).

It also seems that most matters of stressing are preserved in conditions of both inversion and embedding; *an der Tür steht ein Mann*, *natürlich steht an der Tür ein Mann*, *wenn an der Tür ein Mann steht*; *ein Mann steht an der Tür*, *natürlich steht ein Mann an der Tür*, *wenn ein Mann an der Tür steht*; etc., including occurrences of emphatic stressing.¹² Thus the stresswise weakness of the verb perpetuated even in dependent clauses.

Apologies are in order for this attempt at summarizing data of such complexity in such a brief compass. The facts mentioned are introduced merely as diagnostic indications with hints at their systematic setting. The best justification of such a characterization is that it may be useful as a first typological approach. Let us suggest -- we cannot do more -- how this may be so. As a simple illustration, let us take the proposition that Modern German 'is SVO'. Perhaps so; but just possibly it may be more concretely relevant that it is essentially SVO AND (in dependent clauses) SOV, with non-emphatic stressing attached to the O¹³ rather than to the location in the string as such (the stressings are distinctive rather than automatic) and that it allows emphatic as well as contrastive stressing on a morphophonemic level, that is, without providing a special extra loud level in the phonemic 'inventory'; and further that it is a language with

distinctive lexical stress as well. We may also ask, in passing, such questions as this: how significant is the datum that attributive adjectives precede nouns unless we add the information that when this happens, the noun gets the stressing in non-emphatic usage? What remains to be studied is the possibility that a phrase like *sein krankes Kínd* is underlain both by *sein Kínd ist krank* and by *sein Kind ist kránk*.

It is not difficult to see that there is great variety in this respect both within Indo-European and without. In Modern French there is no distinctive lexical stress, 'phonemic phrases' have automatic intonations including stress-like features, there is no emphatic or contrastive stressing.¹⁴ Word-order is 'fixed' and the principal devices for topicalization and the like seem to be the well-known contrivances which will put the underscored item at the end of the phrase. In some other Romance languages lexical stress is phonemic, and word-order is freer, but still there is neither emphatic nor contrastive stressing. Szwedek has compared Polish and English word-order and sentence stress, and while it is not always easy to follow his reasoning one gleans some interesting details.¹⁵ In situations in which English would give us *A b6y came in* (*Ein Mánn steht an der Tür*), Polish (which is declared to have a 'free' word-order) requires, according to Szwedek, the ordering VS; *W pokoju siedzała dziewczyna. Wszedł chłopiec*. 'There was a girl sitting in her room. A b6y came in'. In the sequence 'There was a boy sitting in the room. The boy went out', the Polish order is SV. In general, Szwedek says, ordering is from 'given' to 'new'. His description of stressing in this 'free' - wordorder language deserves quoting.

Since normal sentence stress is placed at the end of the sentence and simultaneously it marks [a] 'new' noncoreferential noun, it is the 'new' noun that is placed in sentence-final position under stress, regardless of the function the noun has in the sentence. This explains the freedom of word-order in Polish.¹⁶

It would seem that French and Polish represent two extremes, with languages like German (and English) in the middle. How does an 'SOV' language behave – a question which has special interest with regard to the options open to the reconstruction of Proto-Indo-European? Hindi, to name one, has no phonemic lexical stress –loudness is attracted by heavy syllables. It also has a general SOV order, though its simplicity

and rigidity is less than Greenberg implies. Masica gives us a cautious judgment and hints at correlations between aberrant word-order constructions (some of which are quite frequent) and what he calls 'special intonation and stress patterns'.¹⁷ This goes, apparently, for some of the non-Indo-European languages in the area as well. There is no emphatic or contrastive stressing of the German-English sort in existence in Hindi. One has the impression that there is an automatic loudness somewhere in the phonemic phrase into which the non-anaphoric items are manipulated but that this prominence definitely does not reside in the final verb. More often than not it shows on the 'object' (Ó) preceding the verb: anyone who has watched the dismay produced in natural speakers of Hindi by English-based pronunciations of Hindi sentences knows how central this feature is. Despite Lehmann's warning against expecting simple retention in a geographic area in which outside OV influence was rife, it is as though we had here the ancient Indic enclisis combined with Delbrück's end position.¹⁸ (There is no hint of a different treatment in dependent clauses.).

The statement that proto Indo-European was generally OV, though not consistently so, is still chiefly a statistical assertion, recalling, even after all our exertions, the concept of 'free' word-order. To understand the situation better we simply need more implicational data – data, perhaps of the kind at which authors like Szwedek hint. While the prospects are still precarious, there is no need to persist in an absolutely pessimistic attitude with regard to stressings and the like. The synchronic typology, at least, is accessible both within and outside our language family. Even the texts might yield a little more. Enclisis is occasionally recorded; what remains to be evaluated is the standing of classical enclisis somewhere between lexical accent and sentence accent (not to mention the relationship between 'pitch' and 'stress' in the Vernerian prehistory of Germanic). Finally, there is poetry. Delbrück had a prejudice against it; he distrusted meter as a destructive and distorting influence and thought, for instance, that the *Brāhmaṇas* being 'prose', and prose being presumably something like 'ordinary language', they were a purer source than poetry. Braune thought the exact opposite. The question cannot be decided in this general form; it takes specific interpretive analysis to uncover what particular distortions have on occasion or habitually oc-

cured. *Dichtersprache*, however, offers enough examples which illustrate the opposite—namely preservation of features precisely in metrical texts. It would not be difficult to find examples, and syntactic examples at that. It could be, in fact, that prose of the kind that is committed to writing in an archaic Indo-European setting is in some respects more highly and more destructively stylized. Let us remember that it was one of Wackernagel's most intriguing ideas to have seen the optional use of the augment in poetic language, as it contrasts with mandatory use in prose, as an inherited mark of style.¹⁹ Whatever we think of the refinements which have been added further to these observation,²⁰ and however we interpret the Mycenaean facts, we may well feel that the 'poetic' usage happens to be, at the same time, the more archaic phenomenon as well. Furthermore, we may even look to meter itself for the kind of testimony which we so sorely miss. W.S. Allen has of course made the suggestion that a quantity-regulated automatic word-stress (somewhat of the Hindi kind) existed even in Greek and that some of the most puzzling and seemingly arbitrary rules of word collocation (in the line of verse) could find their explanation in the old principle of harmony between that accent and the 'ictus' of poetry.²¹ Whether such relationships may be expected only for lexical accents or for stressings as well is a question that might at least be asked.

I would summarize all this by suggesting that mere V-and-S-and-O typology leads the Indo-Europeanist all too often into a situation in which has to fall back on reporting relative frequencies of occurrence. Our *SYNCHRONIC* experience prompts us to enrich our findings with data on innovation and, particularly, on sentence stress. The prospects of retrieving the proto-typology are still precarious as long as the specifically *DIACHRONIC* universals of syntactic change (if they exist) escape us. We may, however, be able to attempt half the job with somewhat greater assurance.

NOTES

- (1) 1979: 67.
- (2) Bonfante 1947: 348f.; Samuels 1972: 25, unless too much is read into S.'s use of the term 'irreversible'.
- (3) 1889: 4 and L. Bloomfield's pointed comment, Hockett 1970: 287.
- (4) Risch 1974: 214; Hoenigswald 1977.
- (5) 1971: 21.
- (6) 1953: 1245.
- (7) *Loc. cit.*
- (8) Samuels 1972: 21ff.; Lightfoot in this volume.
- (9) Lehmann 1974: 152.
- (10) Grateful use is here made of Bierwisch's and Heidolph's examples.
- (11) Hoenigswald 1960: 1, n. 1.
- (12) The situation referred to at the end of the next paragraph is, of course, different.
- (13) The symbol covers far more than direct objects in the traditional sense; see Masica 1976: 21.
- (14) Garde 1968: 44f.
- (15) 1976: 51f.
- (16) 1976: 83f.
- (17) 1976, esp. 17 ff.
- (18) Lehmann 1974: 250; 31 ff.

(19) 1953: 187 ff.

(20) E.g. Blumenthal 1975.

(21) 1973: 260 ff.

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PART TWO:

PROBLEMS IN INDO-EUROPEAN SYNTAX

ZUR TYPOLOGIE DES VORINDOGERMANISCHEN

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Zur Erleichterung des Verständnisses stelle ich diesem Beitrag eine Gliederung in 5 Teilen voran:

1. Typussirrelevante vs. typusrelevante Rekonstruktion
2. Zum Begriff des Indogermanischen (Idg.)
3. Zur arbeitshypothetischen Verwendung des Begriffes *Vorindogermanischer Sprachtypus* (VS)
4. Merkmale des VS
5. Ergebnisse

Im Zentrum meiner Ausführungen stehen die Teile 3 - Darlegung einer Arbeitshypothese zur Verwendung des Begriffes VS - und 4 - Behandlung praktischer Beispiele. Die Teile 1 und 2 dienen der Begriffsklärung (typussirrelevant, typusrelevant, Rekonstruktion, Idg.); diese Teile sind ausserdem als Einführung gedacht, während in Teil 5 die wichtigsten Ergebnisse zusammengefasst werden.

Ad 1. TYPUSSIRRELEVANTE VS. TYPUSRELEVANTE REKONSTRUKTION.

1.1 Da der Nachweis von Sprachverwandtschaft nur durch die Erschließung der konkreten sprachlichen Einheiten (Phoneme, Morpheme, Lexeme, Syntagmen) der unbelegten Grundsprache mit Hilfe der historisch bezeugten Nachfolgesprachen möglich ist, steht die typussirrelevant Rekonstruktion durchaus im Zentrum der historischen idg. Sprachvergleichung. Die Untersuchung erfolgt induktiv, unabhängig davon, ob man sich - wie z.B. Meillet (1903=1969:47) - mit dem Nachweis von Sprach-

verwandtschaft als Forschungsziel begnügt oder - wie z.B. Krahe 1970: 40; 42 - die Rekonstruktion der idg. Grundsprache für möglich hält. Zur Illustration gebe ich zwei Zitate:

a) Meillet: “ ... ce qui fournit la méthode de la grammaire comparée n'est pas une restitution de l'indo-européen, tel qu'il a été parlé: *c'est un système défini de correspondances entre des langues historiquement attestées*”;

b) Krahe: “die idg. Grundformen gewinnt man durch Subtraktion (bzw. Annullierung) aller als einzelsprachlich und damit als sekundär erkannten gesetzmässigen Lautveränderungen” (p. 40); und an anderer Stelle: “in den grossen und allgemeinen Zügen lässt sich eine idg. Grundsprache in Lautstand, Formenbau, Wortschatz und Syntax rekonstruieren” (p. 42).

In Parenthese sei bemerkt, dass sich Meillet und Krahe mehr in der Wertung des Materials als in der Methodik unterscheiden: Meillet betrachtet die zwischen den Einzelsprachen aufzustellenden Gleichungen (*correspondances*) bereits als positives Ergebnis, während Krahe darüber hinaus sozusagen die Grundsprache aufdeckt, indem er die diese einzelsprachlich überlagernden Schichten entfernt. Der ‘Aufdeckungsprozess’ erfolgt durch Feststellung der Lautgesetze sowie der übrigen Sprachwandelprozesse, die in den überlieferten Nachfolge - oder Tochtersprachen zu einer ‘Verfremdung’ der Grundsprache geführt haben.

1.2 Während demnach für den Nachweis von Sprachverwandtschaft eine typusirrelevante Betrachtungsweise ausreichen sollte, stellt bereits die Rekonstruktion einer Grundsprache gleichzeitig die Rekonstruktion eines nach typologischen Prinzipien einzuordnenden Modells dar. Dies wurde schon von Benveniste (1966: 107) bemerkt, der dazu ausführt: “Les identifications matérielles entre les formes et les éléments des formes aboutissent à dégager une structure formelle et grammaticale propre à la famille définie. D'où il suit qu'une classification génétique est aussi typologique”. Zusätzlich zu dieser Feststellung gibt es eine Reihe weiterer Gesichtspunkte, die man zugunsten einer typusrelevanten Rekonstruktion ins Feld führen kann; dazu gehören vornehmlich:

- a) die Wissenschaftsgeschichte der vergleichenden idg. Sprachwissenschaft,
- b) das Faktum, dass sich der Typus einer Sprache ändern kann,

c) die Verwendung der Typologie als Hilfswissenschaft für die genetisch-historische Rekonstruktion, und damit eng zusammenhängend,

d) die Frage nach der typologischen Einordnung von Merkmalen, die dem Voridg. zuzurechnen sind. Da der später zu behandelnde Punkt d) im Zentrum meiner Ueberlegungen steht, soll zunächst mit einigen allgemeinen Bemerkungen auf die Punkte a, b, c eingegangen werden.

a) Wissenschaftsgeschichte: zur Illustration verweise ich auf Schleicher und Trubetzkoy, deren bekannte Positionen in der Nachfolge jedoch keine allgemeine Anerkennung gefunden haben. August Schleicher¹ verknüpfte die Sprachtypologie insofern mit dem genetischen Sprachvergleich², als er die dogmatische Ansicht vertrat: “das Indogermanische z.B. ist vorhistorisch von der isolierenden zur agglutinierenden und von dieser zur flektierenden Stufe vorgeschritten”³, eine These, die durch die Fakten nicht bestätigt wird. Andererseits definierte Trubetzkoy (1939=1968: 214) das Idg. auf Grund von 6 Strukturmerkmalen: 1) keine Vokalharmonie; 2) Konsonantismus im Anlaut nicht ärmer als im In- und Auslaut; 3) Wortanfang und Wurzel brauchen nicht zusammenzufallen; 4) Formbildung nicht nur durch Affixe, sondern auch durch vokalische Alternationen innerhalb der Stammorpheme; 5) grammatischer Konsonantenwechsel; 6) Subjekt des trans. fällt zusammen mit Subjekt des intrans. Verbuns. Trubetzkoy übersah dabei allerdings, dass ausschließlich ‘stoffliche Uebereinstimmungen’ die grundlegende Kriterien für den Nachweis von genetischer Sprachverwandtschaft sein können (vgl. besonders Benveniste 1952/53=1966:99ff.).

b) Das Faktum, dass sich der Typus einer Sprache ändern kann, ist auch aus der historisch belegten Indogermania wohlbekannt. So hat z.B. die flektierende Kasusform des Altidg. in einzelsprachlicher Entwicklung einerseits - wie etwa im Ossetischen, Neuarmenischen oder Tocharischen agglutinierende Züge angenommen, während sie andererseits - bekannte Beispiele sind das Englische oder Französische - Merkmale des analytischen Modells bekommen hat (vgl. Schmidt 1975). Um ein zweites Beispiel, diesmal aus der Syntax, zu nennen, weisen die Inselkeltischen Sprachen im unmarkierten Aussagesatz die *Basic Order* Verbum - Subjekt - Objekt auf, die noch nicht für die älter überlieferten festlandkeltischen Sprachreste gilt oder für das Rekonstruktionsmodell der idg. Grundsprache⁴.

c) Auf die Verwendung der Typologie als Hilfswissenschaft für die genetisch-historische Rekonstruktion ist wiederholt hingewiesen worden⁵. Die diachrone Typologie wird in dieser ‘Sekundärfunktion’ zur Kontrollinstanz für die typologische Wahrscheinlichkeit von Rekonstrukten, die den Ergebnissen der typologisch nachweisbaren Modelle nicht widersprechen dürfen. Gleichzeitig dient sie der Aufdeckung von invarianten Gesetzmässigkeiten beim Sprachwandel.

Ad 2. ZUM BEGRIFF DES IDG.

2.1 Da die Behandlung von Punkt d), d.h. die für unsere Betrachtung zentrale Frage nach der typologischen Einordnung von Merkmalen, die dem Voridg. zuzurechnen sind, im Hauptteil meines Beitrages erfolgen wird, sollen hier zunächst einige Bemerkungen gemacht werden zum Begriff des Idg., bei dessen Interpretation man wissenschaftsgeschichtlich zwischen 2 Auffassungen unterscheiden kann: 1) Ausgangspunkt ist die idg. (als Variante die indo-heth). Grundsprache; 2) Ausgangspunkt sind verschiedene Komponenten, die sich in einem historischen Prozess sekundär zur idg. Grundsprache vereinigt haben. Prominente Vertreter dieser zweiten, von mir abgelehnten, Auffassung sind z.B. Vittore Pisani und N. S. Trubetzkoy.

2.2 In einer seiner Arbeiten jüngeren Datums geht Pisani 1974: 14 davon aus, “dass das *Protosanskrit*⁶ die Sprache einer herrschenden Schicht war, die sich aus türkisch sprechenden Reitern der südrussischen Steppe und Priestern kaukasischen Ursprungs zusammensetzte. Zur ersten Komponente sollte die regelmässige Flexion und Wortbildung zurückgehen, zur zweiten Erscheinungen wie der Ablaut, die sogenannten Laryngalen, das heisst das Schwa..., das Suppletivwesen, die ergative Konstruktion und das damit zusammenhängende Perfekt, beziehungsweise das Medium ...” Zu den Schwierigkeiten, die sich dieser Argumentation entgegenstellen, gehören: 1) das Fehlen von überzeugenden Wortgleichungen zwischen dem ‘Prototürkischen’ und dem ‘Protokaukasischen’ auf der einen, dem Protoidg. auf der anderen Seite; beim ‘Protokaukasischen’ ergibt sich ausserdem das zusätzliche Problem, dass die Existenz einer den derzeit rekonstruierbaren Einheiten West-, Ost-, Südkaukasisch vorangehenden protokaukasischen Grundsprache nicht bewiesen ist⁷; 2) das Faktum, dass wir nichts wissen über die Strukturen der angenommenen Komponenten (Prototürkisch bzw. Protokaukasisch) in

protoidg. Zeit.

2.3 Ähnliche Argumente lassen sich gegen Trubetzkoy 1939=1968, 215 anführen, der es für denkbar hält, “dass die Vorfahren der indogermanischen Sprachzweige ursprünglich einander unähnlich waren, sich aber durch ständigen Kontakt, gegenseitige Beeinflussung und Lehnverkehr allmählich einander bedeutend genähert haben, ohne jedoch jemals miteinander ganz identisch zu werden”. Auf Grund der eben gegeben 6 Strukturmerkmale “muss” nach Trubetzkoy, l.c.: 220f. “das Gebiet, wo die ältesten indogermanischen Dialekte entstanden sind, irgendwo zwischen den Gebieten der ugrofinnischen und der kaukasischmediterranen Sprachen situiert werden”. Zur Kritik an Trubetzkoy's Methode verweise ich zusätzlich auf den bekannten Aufsatz von Benveniste 1952/53=1966

2.4 Dem von Gelehrten wie Pisani oder Trubetzkoy vertretenen *polygenetischen* Standpunkt stehen die verschiedenen *monogenetischen* Theorien gegenüber, die von einer einheitlichen *idg.* oder *indo-hethitischen* Grundsprache ausgehen. Die *dualistische* indo-hethitische Ausgliederungstheorie basiert auf der Annahme, dass der anatolische Zweig aus der *idg.* Grundsprache ausgegliedert wurde zu einer Zeit, in der die nicht-anatolischen *idg.* Sprachen noch eine Spracheinheit bildeten. Hauptvertreter dieser bereits von Emil Forrer⁸ geäußerten Ansicht war E.H. Sturtevant, der auf Grund heth. bzw. anatolischer Besonderheiten (Laryngale, Ausbau der Heteroklisie, binäre Modus- und Tempus-Differenzierung *hi*-Konjugation, Genus Commune, Fehlen des Pronomens **so*, *sā*, *tod* u. a.) in seinen Arbeiten zwischen 1926 und 1951 den Terminus *idg.* (*Indo-European*) auf die nichtanatolischen *idg.* Sprachen einschränkte (vgl. Cowgill 1974: 557); eine der letzten Bearbeitungen der Hypothese erfolgte durch Cowgill 1974, der lediglich den funktionalen Unterschied zwischen heth. *hi*-Konjugation und *idg.* Perfekt als Kriterium für die indo-heth. Theorie gelten lässt⁹. Da das *idg.* Perfekt jedoch in der *hi*-Konjugation aufgegangen sein kann¹⁰, sollte auch dieses ‘Kriterium’ und mit ihm die indo-heth. Theorie mit Zurückhaltung aufgenommen werden.

2.5 Im übrigen stellt sich die Sturtevant'sche Hypothese anderen dualistischen Interpretationen der *idg.* Ausgliederungstheorie an die Seite; ich verweise in diesem Zusammenhang auf Beispiele wie Schleichers 1861 veröffentlichten Stammbaum mit den Komponenten *slawodeutsch* vs. *ariograecoitalokeltisch* oder W. Meids *östliches* vs. *westliches* Spätidg. von 1975. Allen diesen - wie auch anderen - monogenetischen Ausglieder-

rungstheorien gemeinsam ist die Annahme eines grundsprachlichen Kerns, den man *Ur-, Gemein-, oder Protoidg.* genannt hat¹¹.

Ad 3. **ZUR ARBEITSHYPOTHETISCHEN VERWENDUNG DES BEGRIFFES VS**

3.1 Auf der Grundlage der Prämisse, dass infolge des ständigen Wandels der Sprache auch ihre typologischen Merkmale der Veränderung ausgesetzt sind (*typusrelevanter Sprachwandel*) schlage ich für die arbeitshypothetische Verwendung des Begriffes VS folgende Definition vor: Der VS lässt sich durch Interpretation des idg. Rekonstruktionsmodells erschliessen, wobei Prinzipien der synchronen und diachronen Typologie zur Anwendung kommen. Zu den typologischen Prinzipien gehören besonders die sog. *Implikationsuniversalien*, bekannt z.B. in der Formulierung durch R. Jakobson 1958=1971: 526 “the presence of A implies the presence (or on the contrary the absence) of B”, und die Aufdeckung von *invarianten Gesetzmässigkeiten beim Sprachwandel* (Schmidt 1977: 85). Infolge der grossen Zeitdifferenz, die zwischen dem voridg. Modell und dem Einsetzen der realen Ueberlieferung liegt, ist die *interpretierende Rekonstruktion* (IR) des *Voridg.* jedoch hypothetischer als die auf der Basis realiter existenter Nachfolgesprachen erfolgende *direkte Rekonstruktion* des *Protoidg.*, obwohl bei dieser die typologische Interpretation des Materials als bisher eine durchaus geringere Rolle gespielt hat. Ungenauer erschliessen lassen sich bei der IR insbesondere sowohl die Sprachwandelprozesse (Transformationen), die vom Voridg. zum Protoidg. geführt haben, als auch die ineinandergreifenden Korrelationen des voridg. Sprachtypus.

Ad 4. **MERKMALE DES VS**

Auf der Grundlage der soeben gegebenen Definition sollen nunmehr einige Erscheinungen überprüft werden, auf die sich die Etikettierung *voridg.* anwenden lässt. Der Zielvorstellung unseres Kolloquiums Rechnung tragend, wurde das Material vornehmlich den morphologischen Kategorien und der Syntax entnommen.

4.1 Der Vollständigkeit halber seien jedoch einige Bemerkungen zur *Phonologie* an den Anfang gestellt. Hierher gehört zunächst die Feststellung, dass das Protoidg. und natürlich auch das Voridg. über ein stärker ausgebauten Konsonanteninventar verfügten, während der Vokalismus weniger entwickelt war. Diese Feststellung lässt sich belegen: 1) durch die rekonstruierbaren drei Reihen der Palatale, Velare und Labiovelare,

denen einzelsprachlich in der Regel nur 2 Lokalisierungsreihen entsprechen, d.h. Labiovelare vs. Velare (*Centum*-Entwicklung) bzw. Palatale vs. Velare (*Satem*-Entwicklung); ich sehe hier ab von Sonderfällen, wie sie etwa in der Differenzierung der Labiovelare und Velare im Armen. (Palatalisierung nur bei labiovelarer Tenuis und Media) oder Albanischen vorliegen; 2) durch den Ansatz von 3 zusätzlichen Konsonantenphonemen, die unter dem Terminus Laryngale bekannt sind. Diese Phoneme, deren Reflexe u.a. noch in dem hethit. Graphem *h*, einem Teil der prophetischen Vokale des Griech. und Armen. oder der Schwundstufe langvokaliger Wurzeln vorliegen, konnten positionsbedingt offenbar die Färbung und Längung von Vokalen bewirken; 3) durch den Ansatz von Sonanten als einer eigenen Klasse von Phonemen, die in Abhängigkeit von der Silbenstruktur konsonantisch (*j, w, r, l, m, n,*) oder vokalisch (*i, u, r, l, m, n,*) realisiert wurden und auch den 'sonantischen Koeffizienten' von Diphthongen bildeten; 4) durch die Existenz archaischer Konsonantenkomplexe des Typus

1) $*k\tilde{b} \quad k\tilde{b} \quad k^w\tilde{b} \quad \tilde{g}h\tilde{o} \quad gh\tilde{o} \quad g^wh\tilde{o}$ 12,

deren genaue Analyse unklar bleibt.

4.2 Ganz in die Richtung qualitativer Unterschiede des voridg. Konsonantensystems weist die Hypothese von Gamkrelidze/Ivanov 1973, die rekonstruierbaren idg. Artikulationsartklassen (Serien) als das Ergebnis einer Lautverschiebung erklärt (vgl. auch Hopper 1973).

2) Protoidg. stimmhaft - stimmhaft aspiriert - stimmlos < Vorigd. glottoklusiv - stimmhaft aspiriert - stimmlos aspiriert.

Die Theorie stützt sich vornehmlich auf zwei Beobachtungen:

1) auf die Seltenheit des Phonems $/*b/$ innerhalb der idg. Etymologie, die schon Pedersen 1951 zu der Erklärung veranlasst hatte, dass dem $/*b/$ älteres voridg. $/*p/$ zu Grunde liegen müsse. Für $/*p/$ sprechen die typologischen Daten, aus denen hervorgeht, dass in der labialen Stimmbeteiligungskorrelation das stimmlose Oppositionsglied dem stimmhaften gegenüber markiert, d.h. funktional schwächer, ist (Gamkrelidze 1973);

2) auf die Beobachtung von Jakobson 1958=1971: 528, dass die Existenz der *Mediae Aspiratae* in einem phonologischen System grundsätzlich das Vorhandensein der *Tenuis Aspiratae* impliziert:

3) $\text{A L} \quad (\text{L Med. Asp.} \rightarrow \text{L Ten. Asp.})$

Es bleibt zu prüfen, ob ein solches System trotz des Fehlens der *Mediae*

Anspruch auf Wahrscheinlichkeit haben kann. Der besonderen Ueberprüfung bedarf darüber hinaus die Annahme von Gamkrelidze/Ivanov 1973: 153, dass zwischen der 'Lautverschiebung' von Sprachen wie Germanisch oder Armenisch und dem voridg. Ansatz eine direkte Verbindung besteht. Wenn z.B. die armen. *Tenues Aspiratae*, wie sie (neben *h/o < *p*) im Anlaut anstelle der *Tenues* der meisten anderen idg. Sprachen begegnen, als Reflexe der Grundsprache erklärt werden, so ist zu bezweifeln, dass diese Annahme durch nichtanlautende Positionen bestätigt wird (vgl. auch Bolognesi 1960: 19f.):

- 4) Armen. *ew* 'und' < **epi* (gegen **ephi* spricht armen. *ep'em* 'koche': griech. *hépsō*), armen. *ard*, Gen. *ardu* 'Form' (:ai. *rtú-*, griech. *artýs*; *sýntaksis* Hes.)
 < **rtu-* (gegen **rthu-* spricht armen. *ert'am* 'komme')

4.2 Morphologie:

4.2.1.1 Inwieweit das *binominale Genussystem*, bestehend aus unmarkiertem *Neutrum* und davon abgeleitetem *Genus commune*, als protoidg. oder voridg. anzusetzen ist, hängt ab von der Beurteilung des anatolischen Materials. Unabhängig von der zeitlichen Einstufung der im Hethit. vorliegenden Dichotomie weisen jedenfalls die morphologischen Gemeinsamkeiten von *Maskulinum* und *Femininum* (Differenzierung von Nominativ, Akkusativ und unmarkiertem Vokativ als altem *Indefinitus*) im Zusammengehen mit ihren erkennbaren semantischen Uebereinstimmungen als vorhistorische *Animata* auf ein binominales Genussystem in voridg. Zeit. Grundlage dieses Systems waren: einerseits das mit der unmarkierten Stammbildung zusammenfallende spätere Neutrum, das erst durch Verwendung des Kollektiv-Abstrakt-Formans

- 5) $\bar{a}/a$ ($*-e\bar{a}$ 2/*- $\bar{a}$ 2)

als zugehörigem Plural nach der Kategorie *Numerus* differenziert wurde; andererseits der alte *Casus indefinitus*, der nach Ausbildung der markierten Kasus Nominativ und Akkusativ auf die Funktion eines Vokativs beim Genus Commune, dem Vorläufer von Maskulinum und Femininum, eingeschränkt wurde¹³. Der Prozess entspricht typologisch dem Aufkommen eines besonderen Vokativs Plural der *o*-Stämme im Altirischen, wo die Übernahme des primär pronominalen Nominativ Plural auf **-oi* die Begrenzung des älteren Morphems **-ōs* auf vokativische Funktion nach sich zieht:

- 6) Nom. Vok. Pl. $\hat{*uir\bar{o}s} > \text{Nom. Pl. } \hat{*uiroi} \text{ (fir)}$
 vs. Vok. Pl. $\hat{*uir\bar{o}s} \text{ (firu)}$

4.2.1.2. Morphonologisch auffällige Merkmale des protoidg. Nomens sind *Heteroklisie* und starke *Introflexion* des Stammes durch Ablaut und Akzent.

Die Heteroklisie ist durch Suffixwechsel bei bestimmten Neutra und Kollektiva gekennzeichnet: Nomin.-Akk. ($\hat{*}\emptyset, l, j, r$) vs. Obliquus $\hat{*}(n)$ ¹⁴.

besonders häufig belegt ist der r/n typus, der sowohl in jüngeren Suffixkonglomeraten $\hat{*}(-\check{u}er/n, -mer/n, -ter/n, -ser/n)$, als auch als direkte Ableitung von der durch Ablaut differenzierten Wurzel begegnet. Zur Illustration verweise ich auf zwei unlängst von J. Schindler 1975 gegebene Rekonstruktionen:

- | | |
|----------------|--|
| 7) Nomin.-Akk. | $\hat{*}\check{u}éd-\bar{o}r$ (heth. $\acute{u}-i-da-a-ar$) ‘Wasser’ (kollekt.) |
| Schwache Kasus | $\hat{*}ud-n-$ (ved. $udn-$) |
| Lokativ | $\hat{*}ud-\acute{e}n$ (ved. $udán$) |
| Nomin.-Akk. | $\hat{*}\check{i}ék^w-\check{r}$ ‘Leber’ |
| Genitiv | $\hat{*}\check{i}ék^w-\check{n}-s$ |
| Lokativ | ? |

Typologisch könnte der Kern der archaischen heteroklitischen Suffixbildung Indiz sein für alte binäre Differenzierung von Nominativ-Akkusativ vs. Obliquus; der Obliquus stellt die Basis dar für die obliquen Einzelkasus, die in der Regel besonders markiert sind und in ihrem Ursprung ungeklärt bleiben. Die Regelung erinnert an das ‘Prinzip zweier Basen’ (*princip dvuch osnov*) in ostkaukasischen Sprachen, ohne jedoch damit typologisch identisch zu sein. Beim ‘Prinzip zweier Basen’ ist der von dem Nominativ abgeleitete Ergativ die Grundlage für die übrigen Kasus.¹⁵

- 8) Tabass. Nomin. $\check{k}ul$ ‘Kopf’: Ergativ $\check{k}ul-i$: Genitiv $\check{k}ul-i-n$, Dativ $\check{k}ul-i-z$.

Ein archaisches ‘Prinzip zweier Basen’ lässt sich auch innerhalb der pronominalen idg. Flexion erkennen; Nominativ und Restkasus sind hier etymologisch verschieden, d.h. suppletiv zu einem Paradigma zusammengefügt:

- 9) $\hat{*}e\check{g}(h)$: $\hat{*}(e)me$, $\hat{*}m\bar{e}$; $\hat{*}wei$: $\hat{*}n\check{e}s$, $\hat{n}\check{o}s$, $\hat{*}\check{n}s-me$; $\hat{*}y\bar{u}s$: $\hat{*}w\check{e}s/w\check{o}s$,
 $\hat{*}us-we$ u.a.

Im Gegensatz zu der Heteroklisie, deren Prototypen einen Hinweis auf

binäre Kasusdifferenzierung bei bestimmten Neutra in voridg. Zeit geben könnten, spricht die durch Ablaut und Akzent bewirkte starke *Introflexion* des protoidg. Nomens nicht mehr unmittelbar für schwächeren Ausbau des voridg. Kasussystems. Den Terminus *Introflexion* verstehe ich im Sinne von Skalicka 1966: 162, der dazu u.a. bemerkt: “Im introflexiven Typ kann das Morphem unterbrochen sein ... Das introflexive Konstrukt bildet die Wörter durch innere Flexion ... Im introflexiven Konstrukt würden die Ableitungen und die ‘Endungen’ gleichartig ausgedrückt sein ... Die wirklichen Sprachen haben hier eine starke Beimischung anderer Typen ...” Während Skaličkas Feststellung “Im introflexiven Konstrukt würden die Ableitungen und die ‘Endungen’ gleichartig ausgedrückt sein” voridg. auf die Heteroklisie als “paradigmatischen Suffixwechsel” (Terminus nach Rix 1976: 126) zutrifft, gibt es nur wenige Hinweise, die über die Heteroklisie hinaus für eine voridg. Periode schwächerer Differenzierung von Kasusdimensionen *durch Suffixe* in Anspruch genommen werden können; Beispiele dieses Typus sind etwa der suffixlose idg. Lokativ¹⁶, weniger der sog. ‘Direktiv’ bzw. ‘Terminativ’ des Hethitischen¹⁷. Die für das protoidg. Deklinationsparadigma anzusetzenden Akzentuierungstypen, die man heute manchmal als *holo-protero-*, *hystero-* und *amphikinetisch* klassifiziert findet (vgl. Eichner 1972: 91), reflektieren zwar noch starke *Introflexion* als Merkmal des Protoidg., jedoch nicht mehr den als Korrelation dazu voridg. zu erwartenden schwächeren Ausbau von Kasusendungen.

4.2.2.1. Einen morphonologischen Archaismus innerhalb der Verbalflexion stellen die drei *nasalinfigierenden Präsensstammbildungen* dar, deren dem Bau des Protoidg. widersprechende typologische Besonderheit in der *Infexion* (Terminus nach Skalička 1966: 162) eines n-Formans in die Wurzel besteht. Zur Illustration des Prinzips gebe ich ein Beispiel nach K. Strunk 1967: 42:

- | | |
|-----------------|--|
| 10) Ablautstufe | <i>*pleð₁-</i> , <i>*plē-</i> |
| Präsens | <i>*pl-n-eð₁-ti</i> , <i>*pl̥nēti</i> > ai <i>pr̥nāti</i> |
| Aorist | <i>*ep̥leð₁-t</i> , <i>*ep̥lēt</i> > ai. <i>ap̥rāt</i> , lat. <i>-plet</i> , griech. <i>plēt(o)</i> , <i>éplēt(o)</i> |

Neben anderen Präsensstammbildungen bezeichnet die (auch für das Hethitische nachgewiesene) Nasalinfigierung¹⁸ primär den imperfektiven Aspekt und spricht damit für das Vorhandensein der Kategorie Aspekt in

ausserordentlich früher Zeit. Die *n*-Infixation gehört zu den Merkmalen des introflexiven Sprachtypus und verhält sich damit korrelativ zu den bereits besprochenen archaischen Zügen der voridg. Deklination¹⁹.

4. 2. 2. 2. Grundlegend für die voridg. Diathese war die Differenzierung der Verben in *Handlungsverben* vs. *Zustandsverben*. Die Handlungsverben oder *aktiven* Verbalstämme konnten auf Grund ihrer Semantik implizite *transitiv* oder *intransitiv* gebraucht werden (z.B. *schlagen* vs. *gehen*), die Zustandsverben oder *inaktiven* Verbalstämme waren immer intransitiv. Die alte Dichotomie von *Handlung* vs. *Zustand* wird in historischer Zeit in der Flexion vornehmlich durch die Opposition von *Aorist-Präsens*-System vs. Perfekt-System reflektiert. Bei der semantischen Rekonstruktion des protoidg. Perfekts ergeben sich 2 Typen: 1. das sog. *Perfektum intensivum* präsentischer Sinngebung, im Griechischen erhalten “in solchen (iterativ-) intensiven Bildungen, die unwillkürliche Zustände und Triebhandlungen eines Subjekts *bezeichnen*” (Schwyzer 1950: 263):

11) Hom. *bébryche* ‘er brüllt’, att. *kékrage* ‘er schreit’, hom. *ódōde* ‘er duftet’, *gégēthe* ‘er ist voller Freude’ u.a.

2. das “zunächst ebenfalls intransitive *Perfekt des erreichten Zustandes*, das die Fortdauer oder Nachwirkung des durch den Verbalinhalt bezeichneten abgeschlossenen Vorgangs in der Gegenwart des Sprechenden bezeichnet” (Schwyzer, 1.c.). Dieser Typus entwickelt sich einzelsprachlich über das sog. *Resultativperfekt* (Wackernagel 1904 = 1953) zum Präteritum und ist für spätere Sprachstufen von grosser Bedeutung, während er für die vorgeschichtliche Periode ausser Betracht bleiben kann.

Als primär stativa Verbalklasse hat das protoidg. Perfekt den aktiven Verbalstämmen des Aorist-Präsens-Systems gegenüber ein Defizit an Kategorien: es steht ausserhalb der Aspekt-Kategorie und zeigt immer intransitive Diathese. Die Defektivität der stativen Verbalklasse lässt sich typologisch stützen: in einer Sprache wie dem Adygeisch-Tscherkessischen, deren Verbalssystem durch die Opposition dynamischer Handlungsverben vs. statischer Zustandsverben bestimmt ist, fehlen der statischen Klasse eine Reihe von Kategorien: Aorist, Futurum, Imperativ, verschiedene Aktionsarten (vgl. Rogava/Keraševa 1966: 101ff.).

Kehren wir zu der idg. Perfektkategorie zurück, so lassen sich für deren

funktionale Entwicklung folgende Zwischenstufen ansetzen²⁰:

- 12) *Statische Verbalklasse (Zustandsflexion) > sog. Perfektum intensivum (bewahrt in Typus 1 als Restkategorie des präsensischen Zustandes) > Perfekt des erreichten Zustandes (Typus 2) > Resultativ-perfekt usw.

Von den formalen Mitteln zur Bildung des idg. Perfekts (1. Reduplikation, 2. Ablaut: *o*-Stufe bei *e*-stufigen Wurzeln, Dehnstufe, 3. eigenes System von Personalendungen) kann nur Nummer 3 als protoidg. gelten. Die Reduplikation, die sich im Hethit. in der Regel mit iterativer oder intensiver Semantik verbindet und “mit den *hi*-Verben als solchen nichts zu tun [hat]” (Risch 1975: 250), ist vornehmlich im Indo-Iranischen und Griechischen als grammatisches Mittel besonders ausgebaut worden; im Lateinischen und Germanischen neigen Ablaut und Reduplikation zur allomorph-komplementären Verteilung (Meid 1977: 124); der *o*-stufige Ablaut fehlt dem Perfektum intensivum (Schwyzer, 1.c.).

Relikte der Zustandsflexion sind später auch in andere Kategorien eingegangen, wie in das *Medium* durch Uebertragung auf das Aorist-Präsens-System oder in den jetzt sog. ‘Stativ’²¹ der besonders archaischen Flexionsresten des Indo-Iranischen (3. Sg. *-*o*, 3. Pl. *-*ro*) zu Grunde liegt, 13) Ai. 3. Sg. Ip. *asay-a-t* ‘lag’, Prs. *say-e*, 3. Pl. Ip. *ase-ra-n*, Prs. *še-re* = av. *sōire* < **ēkei-o-t*, **kei-o-i*, **ēkei-ro-n*, **kei-ro-i*,

dessen Reflexe man aber darüber hinaus auch in überwiegend impersonal-passiven Formantien des Keltischen, Lateinischen, Oskisch-Umbrischen gesucht hat, sowie im Hethitischen und Tocharischen²².

4.3 Syntax:

4.3.1. Unter den syntaktischen Merkmalen des VS hervorzuheben ist das Verhältnis von transitiver zu intransitiver Konstruktion. Denn es gibt eine Reihe von Hinweisen, die dafür sprechen, dass das Vorige. eher den Typus einer *Ergativ-Sprache* (ES) oder älter *Aktiv-Sprache* (AS) reflektierte als den einer Nominativ-Sprache (NS). Zur Begriffsbestimmung sei bemerkt, dass in einer ES das direkte Ziel des trans. Verbums mit dem Subjekt des intrans. Verbums in einem (morphologisch weithin unmarkierten) Kasus zusammenfällt, während der Agens der trans. Konstruktion, sofern er markiert ist, entweder mit einem obliquen Kasus konstruiert wird oder in einem besonderen, Ergativ oder Aktiv genannten, Kasus steht²³.

Andererseits wird in einer AS die Opposition *trans./intrans.* durch die Opposition von aktiven vs. inaktiven Elementen beim Nomen, Verbum, Satz u.a. ersetzt. Klimov, der sich in den letzten Jahren besonders um die Erforschung des Typus der AS bemüht hat, hebt als Merkmale einer AS hervor: “Die Opposition einer aktiven und inaktiven Klasse bei den Substantiva, die Opposition aktiver und stativer Verben, die Korrelation aktiver und inaktiver Satzkonstruktionen, [d.h.] aktiver und inaktiver Reihen von Personalanzeigern beim Verbum und aktiver und inaktiver Kasus”²⁴. Von diesen Kriterien sind zwei im vorhergehenden bereits für das Voridg. in Anspruch genommen worden: 1) die Opposition von aktiver vs. inaktiver Klasse beim Substantivum, die dem Kern der idg. Genus-Differenzierung zu Grunde liegen dürfte:

- 14) Aktive Klasse (*animata*) vs. inaktive Klasse (*inanimata*) > Genus commune (später Masc./Fem.) vs. Neutrum;
- 2) die Opposition aktiver vs. stativer Verben als Vorstufe für die Dichotomie von Aorist-Präsens vs. Perfekt:
- 15) Aktive Verben vs. stativische Verben > Aorist-Präsens-System vs. Perfekt-System.

Ein drittes Kriterium wäre das Verhältnis von aktivem zu inaktivem Kasus, das sich auf folgende Formel bringen lässt:

- 16) Ergativ/Aktiv vs. Indefinitus > Nominativ (Masch./Fem.) als Agenskasus vs. (Akkusativ, Neutrum, Vokativ, endungsloser Lokativ).

Folgende Konsequenzen ergeben sich aus diesem Schema: 1) Der Agenskasus als Vorstufe zum späteren Nominativ ist nur von der aktiven Klasse der Substantiva, dem späteren Genus Commune bzw. Masch./Fem., ableitbar, die inaktive Klasse, später als Neutrum bekannt, kann dagegen syntaktisch keinen Agens stellen und erscheint deshalb lediglich im Indefinitus, d.h. dem unmarkierten Stamm; 2) der Zielkasus der aktiven Klasse sollte ebenfalls primär im Indefinitus stehen. Das Formans **-m* (bzw. kontextbedingt **-m*) des idg. Akkusativs erklärt sich daher am besten durch sekundäre Uebertragung, wobei der von Martinet 1962: 153 als Quelle vermutete *Allativ* (auf die Frage *wohin?*) als Ausgangspunkt diskutabel erscheint; 3) der endungslose Lokativ ist ein archaischer Relikt-kasus im Indefinitus; 4) der Vokativ entspricht ursprünglich dem alten Indefinitus der aktiven Klasse; 5) eine noch ungeklärte Frage ist die Herkunft des primär aktivischen oder ergativischen -s, der späteren Nomina-

tivendung; 6) das in den heteroklitischen Neutra erkennbare ‘Prinzip zweier Basen’ ist auf die inaktive Substantiv-Klasse eingeschränkt, während das gleiche Prinzip beim Pronomen die Differenzierung von Aktiv bzw. Ergativ und obliquen Kasus bewirkt.

Der Wandel von der voridg. Aktiv- oder Ergativ-Konstruktion zur proto-idg. Nominativ-Konstruktion erfolgte durch Uebertragung des Morphems */*s/* innerhalb der aktiven Substantiv-Klasse: das */*s/* wurde von markierten Aktiv oder Ergativ aktiver bzw. transitiver auf den unmarkierten Indefinitus inaktiver bzw. intransitiver Verbalkonstruktionen analogisch übertragen:

- 17) Voridg. Aktiv bzw. Ergativ (Agens aktiver bzw. trans. Verben) vs. Indefinitus bzw. Nominativ (Subjekt der aktiven Klasse bei inaktiven bzw. intrans. Verben; Ziel) > Idg. Nominativ (Agens trans. und Subjekt intrans. Verben) vs. Akkusativ (Ziel).

Für den Prozess liegt eine typologische Parallele im Aorist des südkaukasischen Mingrelischen vor; hier expandierte der Agens im Ergativ von der transitiven auf die intransitive Konstruktion (vgl. Klimov 1967; Schmidt, im Druck *b*):

- 18) Trans. Aor. *xuro-k* (Ergativ) *’ude* (Nomin.) *kodaagu* ‘der Zimmermann hat das/ein Haus gebaut’ > intrans. Aor. *tiši muma-k* (Ergativ) *dofuru* ‘sein Vater ist gestorben’.

Zeitgründe verbieten es, auf andere Probleme der Syntax (z.B. Wortstellung²⁵, Frage der Nebensätze, Frage der Modi) einzugehen. Stattdessen sollen abschliessend die Ergebnisse meines Beitrages kurz zusammengefasst werden.

Ad 5. ERGEBNISSE:

Nach Durchführung einiger Begriffsklärungen in den beiden einleitenden Teilen (typusirrelevante vs. typusrelevante Rekonstruktion; polygenetische vs. monogenetische Theorien zum Idg.) wurde in Teil 3 folgende arbeitshypothetische Definition des Terminus *Voridg. Sprachtypus* gegeben: “Der VS lässt sich durch Interpretation des idg. Rekonstruktionsmodells erschliessen unter Anwendung universaler Prinzipien der synchronen und diachronen Typologie”. In Teil 4 erfolgte anschliessend die Ueberprüfung von konkretem Material, aus der sich eine Reihe von Merkmalen für das Voridg. ergaben.

Phonologie: Stärker ausgebautes Konsonanteninventar, weniger entwik-

kelter Vokalismus. Hypothese über Lautverschiebung.

Morphologie: Binominale Genussystem als Zwischenstufe zwischen aktiver vs. inaktiver Substantiv-Klasse und den ausserhalb des Anatolischen historisch belegten drei Genera; morphonologische Heteroklisis als Indiz für alte binäre Differenzierung der Kasus bei bestimmten Substantiva der inaktiven Klasse; durch Suppletion bewirkte Dichotomie der Pronominalflexion; durch Akzent und Ablaut bedingte Introflexion in protoidg. Zeit lässt als Korrelation dazu schwächeren Ausbau der Kasusendungen in voridg. Zeit erwarten; innerhalb der Verbalflexion weisen die nasalinflektierenden Präsensstammbildungen morphonologisch auf Infixation als Korrelat der Introflexion; grundlegend für die voridg. Diathese war die Differenzierung der Verben nach *Handlung* vs. *Zustand*, die in historischer Zeit in die Dichotomie von aspektual differenziertem Aorist-Präsens-System vs. ausserhalb der Aspekt-Kategorie stehendem Perfekt-System übergegangen ist.

Syntax: Die Merkmale von aktiver vs. inaktiver Klasse beim Substantivum und aktiver (Handlung) vs. stativer (Zustand) Klasse beim Verbum lassen vermuten, dass es sich bei dem Voridg. um eine Ergativ- oder noch älter Aktiv-Sprache handelte. Ein drittes Kriterium für diese Annahme ergibt sich aus der voridg. anzusetzenden Kasusreaktion mit Übergang von voridg. Ergativ/Aktiv vs. Indefinitus zu Nominativ (Masch./Fem.) vs. (Akkusativ, Neutrum, Vokativ, endungslosem Lokativ).

Eine typologische Parallele für die Transformation von trans. Ergativ-Konstruktion zu trans. Nominativ-Konstruktion liegt im südkaukasischen Mingrelischen vor. In beiden Fällen (voridg.; mingrelisch) wurde das primär den Aktiv oder Ergativ anzeigende Morphem (voridg. *-s bzw. mingrelisch -k) vom markierten Aktiv/Ergativ aktiver/transitiver Konstruktionen auf den unmarkierten Indefinitus der aktiven Substantiv-Klasse bei inaktiven bzw. intransitiven Konstruktionen übertragen.

ANMERKUMGEN

- (1) Sprachvergleichende Untersuchungen, 2. Teil (Bonn 1850) 22f., zitiert nach J. Schmidt 1890=1966: 378.
- (2) Vgl. Austerlitz 1974: 101 “The early comparatists ... were generally content to treat typological questions as a prelude to their main business, comparison”.
- (3) Diese Reihenfolge findet sich 115 Jahre später wieder im Étalon-Modell von Uspenskij 1965, das allerdings auch den inkorporierenden Typus einschliesst, vgl. besonders 244f.: *amorph* > *inkorporierend* > *agglutinierend* > *flektierend*.
- (4) Zum Stand der Diskussion über ältere inselkeltische Wortstellungstypen vgl. z.B. Mac Coisdealbha 1976, Greene 1977, Wagner 1977.
- (5) Vgl. z.B. Jakobson 1958 = 1971, Gamkrelidze/Ivanov 1973, Schmidt 1975; 1977, sowie im Druck a; Strunk 1977: 15f., Hopper 1973.
- (6) Hervorhebung von *mir* (KHS); der Begriff steht faktisch für *Idg*.
- (7) Vgl. Deeters 1957: 12; Klimov 1965: 65ff. Methodisch verfehlt ist Mufti 1978 der das Tscherkessische als *idg*. Sprache identifiziert, ohne zunächst seine Stellung innerhalb des Westkaukasischen zu klären. Die Bibliographie auf S. 292 berücksichtigt nicht einmal Šagirov 1977.
- (8) Mitteilungen der deutschen Orient-Gesellschaft 61 (1921) 26ff. (zitiert nach Sturtevant 1962: 106³).
- (9) “..... the forms ancestral to the Indo-European perfect and the Hittite *hi*-conjugation must have a very different place in the system from both the Indo-European perfect and the Hittite *hi*-verbs: the Indo-European perfect, as we know it (‘now it’ ist Druckfehler) can have arisen only as an innovation common to the ancestor dialects of Indo-Iranian, Greek, Germanic, etc., during a period of several centuries after their split from the dialect ancestral to Hittite” (1.c.: 569); Cowgill 1979 führt *idg*. Perf. und anatolische *hi*-Konjugation auf ‘a third singular of nominal origin’ (: 34) zurück; die wichtigste Funktion der *hi*-Konjugation bestand nach Cowgill in der Bildung von Imperfektiva zu perfektiv gebrauchten ‘verbs of telic Aktionsart’ (: 36). Nicht überzeugend wird die Funktion von Suffixbildungen (*-*néu*-/-*nu*-, *-*ské*-/-*sko*-) eingeschränkt: “..... did not tend to become mere aspect markers, as in PIE, but rather retained full semantic value’ (: 36).
- (10) Vgl. Verf., Glotta 42 (1964): 13; 18 und besonders Eichner 1975; Risch 1975: Perfekt > Präteritum der *hi*-Konjugation; anders Cowgill 1979: s. Fussnote 9; noch anders Kuryłowicz 1979: 144f.: “Die *hi*-Konjugation ist nichts anderes als sekundäre Aktivformen, die von solchen *a*-Deponentien abgeleitet worden sind, zuerst mit kau-sativer Bedeutung”.

(11) Zur Interpretation dieser Begriffe im Sinne ‘verschiedener Abstraktionsgrade’ vgl. Schmidt 1978: 23f.

(12) Vgl. dazu letztlich Kuryłowicz 1977: 205-211; Schindler 1977; der Ansatz eines stärker ausgebauten voridg. Konsonantismus spricht grundsätzlich auch gegen die Reduzierung der idg. Gutturalreihen auf zwei, wie sie letztlich wieder von Steensland 1973 vorgenommen wurde.

(13) Zur Verwendung der Stammform in hethitischen Namen-Konstruktionen (e.g. Ni-qa ŠUM-ŠU ‘Niqa (ist) ihr Name’) und in idg. Komposition vgl. Neu 1979, der den *Casus indefinitus* in Vocativus [+ Appell] und Commemorativus [– Appell] differenziert (: 184).

(14) Vgl. bei Schindler 1975: 1: ved. *yūh*: *ṽśnāh* ‘bouillon’, idg. **seh₂ul*: **sh₂uēns* ‘soleil’, ved. *āsthi*: *asthnāh* ‘os’ als Biespiele für (ϕ, l, j).

(15) Zur Definition dieses ‘Prinzips’ verweise ich auf Magometov 1965: 97: “Ot imenitel’nogo padeža (padeža samogo po sebe neoformlennogo, kak i v drugich dagestanskich jazykach) obrazuetsja ergativ, a ergativ ležit v osnove kosvennykh padežej”.

(16) Vgl. Benveniste 1935: 87ff., besonders 98f.: “..... ce que l’on dénomme ‘locatif’ repose comme un bon nombre de nominatifs-accusatifs, sur un ‘cas indéfini’ qui avait en indo-européen la forme même du thème neutre”; 3 altheth. Beispiele bei Kammenhuber 1979: 123f.: *tagan* ‘zur Erde’ *šiuat* ‘am Tage’, *takšan* ‘in der Mitte’; vgl. auch Neu 1979: 490. Vgl. auch W. P. Lehmann in einer im Druck befindlichen Besprechung von J. H. Greenberg, *Universals of Human Language*; vol. 4, *Syntax*, der für den suffixlosen Lokativ folgende Erklärung erwägt: ‘These may be survivals of existential patterns, adapted for possessive use. The substantive then might have had a possessive value from initial position, with no inflection. Subsequently, when nominal inflection came to be more prominent, as at the time thematic inflection arose, a specific ending was added for the locative’ (freundliche Mitteilung von W. P. Lehmann mit Brief vom 12.9.1979).

(17) Vgl. dazu letztlich Schmid 1973; Starke 1977: 25ff; Kammenhuber 1979; Neu 1979: 187; E. Laroche, *RHA* 28 (1970): 46f. hatte diesen Kasus auf idg. **-ō-* (> heth *-a*) zurückgeführt und mit Richtungsadverbien des Typus griech. *ánō-kátō*, lat. *quōd-eō* verglichen.

(18) Vgl. die Stämme auf *-nin-* (5. Klasse) und *-nu-* (7. Kausativa) der *mi*-Konjugation bei Friedrich 1960: 94f. und 97f. und s. letztlich Strunk 1979.

(19) Um den Nachweis von Infixen bemüht sich auch Karstien 1971, doch bleibt bei ihm die Funktion der Infixe im allgemeinen unklar (vgl. meine Rezension: *ZCP* 33, 1974, 320ff.).

(20) Vgl. auch Verf., Festschrift Čikobava (im Druck c).

(21) Vgl. dazu Oettinger 1976; Rix 1977: 135f.; Meid 1979: 173ff. setzt für die heth. *hi*-Konjugation und das idg. Perfekt ein ‘frühidg. Medium’ als gemeinsamen Vorläufer an.

(22) Rix 1977: 135f. differenziert zwischen Medium (3. Sg. **-to*, 3. Pl. **-nto*, 2. Sg. **-so*) und Stativ (3. Sg. **-o*, 3. Pl. **-ro*, 2. Sg. **-i^ha < *th₂e*) und gibt an Belegen für die 2. Sg. Stativ: heth. *ar-ta-ri*, ai. *tanu-thās*, toch. *parksa-ta-i*, air. *cluín-te*.

(23) Die Frage des Idg. als ES ist wiederholt behandelt worden. Wegen der Sekundärliteratur vgl. Schmidt 1977 *a* und im Druck *c*.

(24) Vgl. Klimov 1977: 57: “Protivopostavlenie aktivnogo i inaktivnogo klassov substantivov, opposicija aktivnych i stativnych glagolov, korreliacii aktivnoj i inaktivnoj konstrukcij predložénija, aktivnogo i inaktivnogo rjadov ličnych pokazatelej glagola i aktivnogo i inaktivnogo padežej”.

(25) Vgl. die kritischen Beiträge von Watkins 1976; Strunk 1977 mit weiterer Literatur.

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THE RECONSTRUCTION OF NON-SIMPLE SENTENCES IN PROTO-INDO-EUROPEAN

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1. **INTRODUCTION.** More than a century ago Indo-Europeanists had come to the conclusion that simple sentences were fully developed before the separation of Proto-Indo-European into dialects; but there was no such agreement on the non-simple sentences. At the beginning of a long essay on the origin of the relative pronoun in the Indo-European languages Windisch states this conclusion: “Für die Syntax ergibt sich aus dem Gesagten das wichtige Resultat, dass wohl der einfache Satz bereits vor der Sprachtrennung ausgebildet war, aber nicht so das Satzgefüge” (1869:205). Like other scholars of the time Windisch set out to determine the origin of *Satzgefüge* or non-simple sentence structures, especially of relative clauses and their markers. The task is by no means complete, as this colloquium and this paper indicate. The structure of relative and other hypotactic clauses in Proto-Indo-European still needs attention.

At the time of Windisch's publication there was also general agreement that relative clauses developed out of paratactic constructions. Scholars who dealt with Germanic, for example, accounted for the relative pronoun as a development from a demonstrative pronoun. It would require only a change in intonation to shift from parataxis to hypotaxis, from a demonstrative pronoun to a relative pronoun in an example like the following, which for ease of reference can be given in modern English

or German as well as their earlier stages:

- (1) a. *He wanted the house. That is now his.*
 b. *He wanted the house that is now his.*

Yet even though such an explanation was plausible for Germanic, it was less satisfactory for the other dialects, and for the Indo-European dialects as a whole; the treatment of hypotactic constructions in subsequent Indo-European handbooks indicates the problems remaining to be accounted for.

Before reviewing the treatment in major grammars we recall the earlier view that reconstructed Indo-European was a primitive language. Primitive languages were held to differ from the contemporary languages of civilization in various ways, such as the use of parataxis alone. Moreover, as may be detected from Windisch's statement, even parataxis represented an advanced stage, in that utterances consisted of fully developed sentences. Accordingly there would have been a progression from utterances made up of words to utterances made up of structured simple sentences, and finally to compound and complex sentences as well. This view may also be detected among scholars dealing with Indo-European syntax after Windisch, as the treatment of the most influential syntactician of Indo-European studies, Berthold Delbrück, may indicate.

Delbrück as usual states his position clearly: "there was once a time, in which only principal clauses existed" (1900:412; my translation). Coordinated clauses arose from these in the early dialects. Coordination was indicated in various ways, by intonation, particles, anaphoric pronouns or particles. One example from Delbrück will suffice (1900:417):

- (2) *viśve devā adravan, maruto hainaṃ najāhuḥ*
 all gods ran-off Maruts ptc. him not-deserted

'All the gods ran away, but the Maruts did not desert him'.

Here the particle *ha* in the second clause indicates coordination. This analysis of coordinated *clauses*, or compound sentences, has been generally accepted. Meillet, for example, maintains it (1937:377). We need not then concern ourselves with the general problem of coordinated clauses in late Proto-Indo-European and the early dialects. We might note, however, that Delbrück and his successors regard PIE **k^we* 'and' and **ve* 'or' as phrasal, not clausal conjunctions. Clause conjoining in Proto-Indo-European accordingly was not carried out as in Classical

Greek, Latin or SVO languages; when a conjunction was used, it was placed after the first word in the second clause, as in (2).

From parataxis then hypotaxis arose; subordinated clauses developed somehow from coordinated clauses (Delbrück 1900:413). But as already suggested, neither Delbrück nor later Indo-Europeanists came to any agreement how this development took place or even when it took place. According to Delbrück, late Proto-Indo-European (*die indogermanische Grundsprache*) had developed beyond the oldest state, in which nothing but principal clauses were present, so that it “probably contained relative and adverbial clauses” (... *kannte wahrscheinlich schon Relativ- und Konjunktionssätze*). In this development hypotaxis initially lacked a specific indicator. First a relative marker was introduced, based on **yo-*. Subsequently conjunctions developed from these, to indicate adverbial clauses. Finally, as in early Greek and Latin, other devices were introduced, such as shifts in person and mode, and conjunctions from stems other than the relative (1900:406-47). The evidence cited by him, and by others, for the later stages of this proposed development is difficult to overthrow.

Hirt too assumes for Proto-Indo-European dependent clauses in the last volume of his *Indogermanische Grammatik* (1937:122-212). And like Delbrück (1900:295-406) he sees a gradual development, in which the relative clause is the oldest of dependent clauses: nonetheless he has questions about the identification of the relative pronoun. Delbrück had assumed on the basis of Indic and Greek that only **yo-* was so used in Proto-Indo-European; this also is Meillet’s view (1937:376). And following Windisch, Delbrück and Hirt assumed that the relative pronoun had its origin in anaphoric pronouns – a shadowy assumption until Justus’s brilliant analysis of Hittite (1976:213-45). But Hirt in his free-spirited manner goes on to say that any deictic pronoun may become anaphoric in the course of time; accordingly all four deictic pronoun stems of Proto-Indo-European: **e*, **yo*, **so*, and **to* are to be recognized “as particles used to mark relatives” already in Proto-Indo-European. Moreover, **k^wi*, **k^wo* is to be added to this set. Hirt’s general view then is similar to Delbrück’s, though Hirt is somewhat more definite about assuming subordinate clauses in Proto-Indo-European.

Just as we agree with Delbrück views on paratactic constructions in

Proto-Indo-European and the early dialects, we accept his position on the situation in Sanskrit, Greek and Latin of the first millennium B.C. The major problems remaining have to do with the determination of the development of subordinate clauses in Proto-Indo-European and the early dialects. We cannot agree that there was a primitive stage in the development of language when humans knew only simple clauses, that is, no subordination. Such a view lacks credibility, certainly in a period as late as the Proto-Indo-European community, even though we place this period earlier than did Delbrück and his contemporaries. Accordingly we must seek a more credible explanation for a Proto-Indo-European with no apparent indicators for subordination.

A description of the facts was given in a weighty article published at the turn of the century (Hermann 1895:481-534). This found no evidence whatsoever for assuming subordinate clauses in Proto-Indo-European. The article examined twelve possible devices for marking subordination; these are given in Chart I. Hermann found conclusive evidence for none of them in Proto-Indo-European, with the possible exception of clause accent.

Like other scholars Hermann pointed to the contrast in accent on verbs in Vedic; these lack accent in principal clauses, carry it in subordinate, thus differentiating between the two in intonation. Bonfante in an appreciative treatise following Hermann accepts this arrangement as certain for Proto-Indo-European, citing also evidence from Germanic (1930: 11). Meillet too admits only intonation as a signal for subordinate clauses in Proto-Indo-European (Meillet 1937:377). But he also discusses other devices used for indicating subordinate segments which are prominent in the early dialects: “various nominal forms” -- participles, nouns comparable to infinitives and nominal compounds (1937:373-6). But he did not relate these to the more prominent means used for subordination in the dialects, and he did not account for the development of subordination as such. It is one purpose of this essay to state specifically what the devices in Proto-Indo-European were and to indicate how they developed.

Before proceeding to such a statement it should be recalled that the problem is syntactic. Accordingly we cannot reconstruct morphological entities which are found in several dialects, and on the basis of these propose specific syntactic patterns for Proto-Indo-European. Sanskrit *yád*, Avestan *yaþ* and Greek *hó* may well be identical in form and thus

the basis for a reconstructed Proto-Indo-European **yod*; but such a reconstructed morpheme does not speak for the reconstruction of temporal, causal or conditional clauses indicated by **yod* in the parent language, or even the “loosely bound purpose” clauses for which alone some slight comparative evidence can be drawn from the *Rigveda* and Homer (Delbrück 1900:319-21, 328-9). Proto-Indo-European syntax must be reconstructed on the basis of syntactic, not lexical or morphological, patterns.

2. **TYPES OF SUBORDINATION.** Before examining the devices for indicating subordinate clauses in Proto-Indo-European and the early dialects, we may note briefly the classification of these in grammars. Three types of subordinate clauses are widely distinguished, as by Jespersen in accordance with his syntactic framework which includes three ranks. Subordinate clauses may be of primary rank – used as subject or object; these I refer to as substantive clauses. When used as objects they are commonly called complements.

(3) a. I expect (*that*) *he will arrive at six* (cf... his arrival).

Subordinate clauses may be adjuncts (clause adjuncts); these I refer to with the general term as relative clauses.

b. I like a boy *who speaks the truth* (cf.... a truthful boy). Subordinate clauses may be subjuncts or tertiaries (clause realignment); these I refer to as adverbial clauses.

c. I must go *when he comes* (cf. then).

Jespersen discusses these at great length, giving many further examples than those selected here (1924:103-7).

Other major grammars also distinguish these three types, though some prefer a twofold distinction, contrasting the second type (relative clause) with the other two. Some grammars treat subordinate clauses in accordance with morphological rather than syntactic criteria, in this way obscuring syntactic patterns. Whatever the classification and the terminology, linguists concerned with the syntax of any language, including reconstructed languages, set out to identify its subordinate clauses and its complex sentences.

3. **SUBORDINATION IN THE EARLY DIALECTS.** Our possibilities for reconstructing Proto-Indo-European syntax have been greatly improved since the time when Delbrück, Meillet and Hirt published their views. For subsequently an appreciable quantity of Anatolian texts have become avail-

lable as well as grammars for them. These provide evidence on the syntactic patterning of prose documents a millennium closer to Proto-Indo-European than were the *Brāhmaṇas* which Delbrück used for his initial conclusion on Proto-Indo-European syntax. The earliest Hittite texts should then be the primary basis for reconstruction of compound sentence patterns and other syntactic patterns of Proto-Indo-European, though as usual during use of the comparative method in consideration of the evidence in other dialects. Most weighty among these might well be Mycenaean Greek, though the make-up of its corpus provides little syntactic evidence. We will however draw on it, and on the other Greek material, as well as on Vedic Sanskrit and other dialects.

It is well-known that the parataxis is prominent in Hittite. Quotations are generally given in direct form, rather than as substantive clauses, whether subjects or objects; such quotations may have only a tenuous relationship with accompanying principal clauses (Sommer 1938:53-5). And some adverbial clauses may have no indicator at all.

(4) *memaḥḥi -ta kuit nu-mu GEŠTU-an parā ēp*

I-tell you what ptc me ear toward take

Literally: I will tell you something; (concerning that) give me your ear.
‘Hear what I tell you’.

Adverbial clauses indicating purpose and result simply use paratactic arrangement (Friedrich 1960:163).

(5) *nu taškupāi nu URU-aš dapijanza išdammašzi*
ptc cry ptc city in-entirety hears

Literally: Cry out; the entire city will hear (it).

‘Cry out so that the entire city hears it’.

Such patterns are in keeping with the conclusions of Hermann.

Before accounting for the prominence of paratactic constructions in Hittite we may recall the various patterns of subordinate clauses in Hittite, citing examples from the earliest texts.

(6) Substantive (object) clause: *kinun-wa-az nuwa SAL-MEŠŠU.GI[-uš]*
now-Quotation-Reflexive still old-women

[pu-nu-uš-ki-iz-]zi ŪL šaggahḥi

she - questions not I-know

Literally: Does she still question the old women? I do not know.

‘I do not (wish to) know whether she still questions the old women’

This statement of Hattusili, marked by *wa* as quotation, simply has the object clause preposed to the matrix verb (Sommer 1938:17).

A frequent pattern for relative clauses may be taken from the Anitta text; *kuiš* indeed signals the relative clause, though in a loose relationship as will be noted further below.

- (7) *kuiš ammel āppan LUGAL-uš kišari nu URUḪattušan āppa ašāši n-an*
 who me after king becomes ptc Hattusa again settles ptc him
nepišaš dIŠKUR-aš ḫazziēttu (Neu 1974: 12)
 of-heaven weather-god should-strike

Literally: Someone becomes king after me; he resettles Hattusa; him the weather-god of heaven should strike.

‘Whoever becomes king after me and resettles Hattusa, him the weather-god of heaven should strike’.

The Anitta text also provides an example of an adverbial clause.

- (8) *mān-aš appezzišana kištanziattat š-an dḪalmašuitti dŠiuš-miš parā paiš*
 when it later hungered ptc it to-Halmasuit Siu my over gave
š-an išpandi nakkīt dāḫḫun pēdišši-ma ZĀ.AḪ.Li-an anienun
 ptc it by-night forcefully I-took in-its-place but weeds I-produced
 (Neu 1974: 12).

‘When it (the city) later suffered from hunger, my god Šiu handed it over to Ḫalmašuit (the throne-goddess), (and) I took it by force at night, but in its place I planted weeds.’

When we examine the common characteristics of these three hypotactic clauses, we find that the subordinate clause is preposed in each sentence. The relative (7) and the adverbial (8) include a marker, but the substantive clause (6) does not. The subordinate clause of (6) then is like (5), which belongs to a type of adverbial clause (purpose and result) which in general have no indicators in Hittite. Friedrich in his Hittite grammar puts it differently. According to him Hittite did not form purpose and result clauses; instead, it simply arranged two clauses side by side with *nu*, as in the following example (Friedrich 1960:163).

- (9) *n-aš ŪL tarnahḫun n-an-kan ŪL kuennir*
 ptc them not I-let ptc him-ptc not killed

Literally: I did not permit them (and) they did not kill him.

‘I did not permit them to kill him’.

The other prominent types of adverbial clauses generally have a

marker, often a frozen form of the element used for relatives, *kuiš*. Causal clauses are indicated by *kuit* ‘because’. Temporal clauses may also be indicated by *kuit*, with *kuitman* ‘while’, or with *mān* ‘when’, subsequently *mahhan*. (*Mān* is apparently based on an obsolete relative marker). In the later language *mān* also signals conditional clauses; *takku* ‘if’ is often used in the early texts, or no conjunction is found (Friedrich 1960:165; Sommer 1938:182).

- (10) *ušaḏul kuelqa autti nu-za pankun EGIR-pa punuški*
 crime of-one you-see ptc Refl assembly ever you-ask

Literally: You see someone’s crime -- tell the assembly always.

‘If you see someone committing a crime, always tell the assembly’

The conjunctions *mān* and *mān-a* ‘although’ are used for concessive clauses. In sum, adverbial clauses are indicated by means of a conjunction in later Hittite, though in the early texts simple parataxis may be found.

The situation in Hittite then parallels that proposed by Delbrück for early Indo-European on the basis of his conclusions from Sanskrit and Greek. We note examples for these, to illustrate their syntactic pattern. The Sanskrit example includes three stanzas from an archaic hymn, which is descriptive; I assume therefore that it was little modified for rhetorical purposes though the meter may have brought about syntactic modifications. I select it also because the verb forms indicate its antiquity. The translation is based on Geldner and Hoffmann (Hoffmann 1967:164-5; 173-5). The hymn is *Rigveda* 5.45.1, 4, 6.

- (11) 1. *vidā divó viṣyánn ádrim uktháir*
 with-knowledge of-heaven opening rock with-sayings
āyatyā uṣáso arcíno guḥ
 approaching of dawn rays emerge
ápāvṛta vrajinīr út svār gād
 he-frees penned out sun comes
ví dúro mānusīr devá āvaḥ
 apart doors human god has-opened

‘When he opens the rock, with knowledge, with sayings of heaven,
 The approaching rays of dawn come out,
 He frees the penned (cows); out comes the sun;
 God has opened up the human doors’.

4.	<i>sūktébhīr</i>	<i>vo</i>	<i>vácobhīr</i>	<i>devajuṣṭāir</i>
	with-well-knit	for-you	with-words	with-god-pleasing
	<i>índrā</i>	<i>nv</i>	<i>ágnī</i>	<i>ávase</i>
	Indra	ptc	Agni	for-help
				to-call
	<i>ukthébhīr</i>	<i>hī</i>	<i>ṣmā</i>	<i>kaváyaḥ</i>
	with-hymns	ptc	ptc	wise
				well-sacrificing
	<i>āívāsanto</i>	<i>marúto</i>	<i>yájanti</i>	
	gaining-favor	Maruts	they-worship	

‘With well-made god-pleasing words,
I will now call on Indra and Agni for your sake for mercy,
For with hymns of praise the wise, sacrificing well,
Worship the Maruts, in order to gain their favor’.

6.	<i>étā</i>	<i>dhīyaṃ</i>	<i>kr̥nāvāmā</i>	<i>sakhāyo</i>
	come	prayer	let-us-make	friends
	<i>ápa</i>	<i>yá</i>	<i>mātām</i>	<i>ṛṇutá</i>
	ptc	which/these	mother	opens
				pen
				of-cow
	<i>yáyā</i>	<i>mánur</i>	<i>viśiśiprāṃ</i>	<i>jigāya</i>
	with which/this	Manu	Viśiśipra	conquered
	<i>yáyā</i>	<i>vaṇíg</i>	<i>vañkúr</i>	<i>ápā</i>
	with which/this	merchant	quick-moving	he-reached
				spring

‘Come, friends, let us produce a hymn,
Which as mother opens the pen of the cow,
Through which Manu conquered Viśiśipra,
Through which the rapidly-traveling merchant reached the spring’.

Syntactically the first stanza of this hymn is purely coordinating, with no conjunctions. If however we were to rephrase it in an SVO language, we would put the last line initially and might well give the following rendering: ‘God has moved apart the doors of humans by opening the rock of heaven with his knowledge, with his sayings, so that the beaming rays of the coming dawn appear; in this way he releases the penned up cows and the sunlight emerges’. The second through fourth stanzas maintain the same syntax, though in the fourth coordinating particles are used. In stanza six there are indicators to relate clauses. Translators take these as relative pronouns. On the basis of such clausal relationship in Hittite, however, we might well take them as demonstratives, translating: ‘this as mother opens the pen of the cow, through this Manu...’ In any

event, the hymn as a whole has simple coordinating syntax, well in keeping with Delbrück analysis for the early dialects.

For Mycenaean Greek we unfortunately have only listings, which by their textual structure may have coordinating sentences. An example nonetheless is cited here, Pylos Ep 704 (Ventris-Chadwick 1973:252)

- (12) *e-ri-ta i-je-re-ja e-ke e-u-ke-to-qe e-to-ni-jo e-ke-e*
 Eritha priestess holds she-claims and freehold holds
te-o /da-mo-de-mi pa-si ko-to-na-o ke-ke-me-na-o o-na-to
 god village ptc ptc says of-plots of-communal lease
e-ke-e to-so pe-mo WHEAT 3 T 9
 holds such seed

‘Eritha the priestess holds (this), and she claims that (her) god holds the freehold; but the village says that he/she (merely?) holds the lease of communal plots. So much seed: wheat 468 l.’

- (13) A parallel version, Eb 35 (Ventris-Chadwick 1973:256):

i-je-re-ja e-ke-qe e-u-ke-to-qe e-to-ni-jo e-ke-e te-o
 priestess hold ptc she-claims ptc freehold holds god
ko-to-no-o-ko-de ko-to-na-o ke-ke-me-na-o o-na-ta e-ke-e...
 plot-owner of-plots of-communal right holds

‘The priestess holds (this) and claims that the god holds the freehold; the plot-holder (claims that) he/she holds the leases of communal plots’.

These texts, like some of the Hittite have no subordinating conjunction to indicate the substantive clause with ‘claims’. The syntax is paratactic. Since the Mycenaean texts are so limited, we must base our conclusions on Homer. Admirable analyses exist, such as Chantraine’s, which includes many examples; his conclusions only will be cited here. The very title of Chantraine’s last chapter is significant: “La structure de la phrase complexe et la parataxe” (1963:351-64). In this chapter one finds conclusions similar to those of Friedrich for Hittite, for example, that of two coordinate clauses one is often “logically equivalent to a subordinate clause dependent on the other” (1963:357). Such clauses may be causal, temporal concessive and clauses of purpose. Chantraine also noted similar relationship for “relative clauses” (1963:243). His conclusions on Homeric syntax then support the view that early Greek syntax was strongly paratactic, paralleling in this way Vedic Sanskrit and Early

Hittite.

For Latin syntax we may examine the Agrarian Laws of about 111 B.C., such as the following passage (Warmington 1940:392):

- (14) *Quei ager publicus populi Rom. in terra Italia*
 what field public of-people Rom. in land Italy
P. Muucio L. Calpurnio
 P. Mucius L. Calpurnius
cos. fuit, de eo agro loco quem agrum locum populus
 C. was from that land place what
ex publico in privatum commutavit,
 from public to private changed
quo pro agro loco ex privato in publicum tantum
 what for so-much
modum agri loci commutavit: is ager locus domneis
 amount owners
privatus ita, ut ei quoi optuma lege privatus
 be as what fullest law
est, esto.
 let-it-be

‘In regard to the public land in the country of Italy belonging to the Roman people in the consulship of Publius Mucius and Lucius Calpurnius, any land or ground, out of such land or ground, which the people shall have made private by exchange from public, and has for the said land or ground received an equal measure of land in exchange from private to public, such land or ground shall be private land of the owners according to the fullest legal title to private land belonging to any owner’. (Warmington’s translation 1940:393). Here too the apparent relative particles do not solely represent subordination, as Warmington’s translation of the first may indicate: ‘In regard to the public land’ rather than ‘Whatever public land...’ Subsequent clause relationships might well be indicated by adverbial conjunctions like: ‘if (the people have converted)’. Even in such late material, admittedly legal and therefore archaic, the paratactic style which is characteristic of Hittite, Vedic Sanskrit and Greek, prevails.

Further examples might be cited from other dialects in support of the conclusion that the sentence structure of Proto-Indo-European is

Literally: This is the John-wrote-book.

‘This is the book that John wrote’.

(16) Adverbial clause: (*uti* ‘interval’ as nominalizer)

Kodomo ga nete-iru uti ni hon o yomimasyoo (Kuno 173:153)

children sleeping are interval in book let’s read

Literally: In the children-are-sleeping-interval let’s read books.

‘While the children are asleep, let’s read books.’

(17) Substantive clause: (*koto* as nominalizer)

Mary wa John ga kuru koto o kitaisite-ita (Kuno 1973:220)

TOPIC come fact expecting-was

Literally: Mary was expecting the fact: John is coming.

‘Mary was expecting that John would come’.

If an OV language of this structure were to change to VO, new devices would have to be produced to indicate subordination. Object clauses would have to follow verbs; accordingly some kind of devices like conjunctions would very likely develop, and also in adverbial clauses. Moreover, relative clauses would follow their antecedents, leading to the development of a signal indicating relativization.

If we assume that Proto-Indo-European was OV, its syntactic development would be clarified in this way. Rather than a primitive language or a primitive people, we would posit for Proto-Indo-European a linguistic structure attested for approximately half the languages known today. Changing from this structure – through some cause not pertinent to the present study – late Proto-Indo-European would have developed signals to indicate dependent clauses.

Since syntactic change takes place through a long period of time, we might with good fortune have early texts which maintain other OV characteristics as well as clause structures. The early dialects indeed have evidence of these, such as postpositions, comparative constructions with the standard preceding the adjective, genitives and adjectives preceding nouns, and so on. Evidence for such OV patterning and its change has been noted elsewhere (Lehmann 1974 and references). The patterns involved in the changes of clause structure must also be described, as papers presented at this colloquium indicate.

5. THE CHANGE OF SUBSTANTIVE CLAUSES FROM OV TO VO PATTERNS.

Substantive clauses, as illustrated by (6), may be expressed in early Hitti-

te with no device comparable to English complementizers, such as *that* or *whether*. Yet other syntactic patterns are found as well. Substantive clauses used as objects, often referred to as complements, are commonly classified in accordance with the governing or matrix verb. Well-recognized in classical grammars, these are treated in various classes, such as linguistic-performative verbs or *verba dicendi* and *putandi*, verbs of perception or *verba sentiendi*, and causatives or *verba causativa*. Older handbooks unfortunately list the patterns used with these classes by morphological rather than syntactic criteria; in using such handbooks one must then assemble the treatments of complementation from sections on infinitives and participles, on particles, and on clause types. We must now prepare handbooks which will deal with these patterns in accordance with their syntactic structure.

Here we have space for only a general statement, with a few examples. Sentence (6) exemplifies Hittite expressions for the first group of matrix verbs, *verba dicendi*. *Verba sentiendi* may have verbal nouns as complements; these are formed by means of *-uuar* and *-atar* suffixes (Friedrich 1960:142-4).

- (18) LÚMES KUR^{URU} Mizra - ma maḥḥan ŠA KUR
 people land Egypt ptc when ptc land
 URU^{Amka} GUL - aḥḥuuar ištamaššanzi
 Amka attacking they-hear

Literally: But when the people of the land of Egypt hear the attacking of the country of Amka.

‘But when the Egyptians hear that Amka has been attacked.’
Verba causativa take as complements forms in *-anna* and *-uuanzi*, labeled infinitives in Hittite grammars. They may in turn govern a noun in the accusative.

- (19) LÚ SANGA akuuanna uekzi
 priest drinking desires

Literally: The priest desires drinking

‘The priest desires to drink’.

- (20) apāš - ma - mu ḥarkanna šanḥta
 he but me destroying sought

Literally: He however sought for destroying me.

‘He however sought to destroy me’.

Verbal nouns may also be used in subject clauses. These show a

loose relationship between the verbal noun and its objects. (Friedrich 1960:143-4).

- (21) *LU*MUNABTUM EGIR-*pa* pijanna UL ara
fugitive back giving not is-right

Literally: A fugitive is not right for giving back.

In a similar English construction the entire clause would be the subject: ‘Giving back a fugitive is not right’ or ‘To give back a fugitive is not right’.

Hittite substantive clauses then may be expressed by preposed non-finite verbal elements, whether they function as object or subject, or simply by preposed clauses.

Similar patterns are found in the OV language, Turkish, which possesses a number of verbal nouns. Those indicating the present are formed by suffixing *-ış*, *-mek* and *-me* to the stem, e.g. *gelme* ‘coming’; those indicating past action by suffixing *-dik*, future by *-ecek*. Verbal nouns having these affixes are used with verbs of perception, and with performative and causative verbs, such as *öğrenmek* ‘hear’, *sevinmek* ‘rejoice’, *istemek* ‘want’. In this use they are treated like other nouns, with appropriate inflections; as in (24), they may also take objects.

- (22) *Gelme-n-e* *sevindim*
coming-your-at rejoice-PAST-I

‘I was happy at your coming (= that you came).’

- (23) *Ahmet’in çalıştığını* *öğrendim.* [compare 18]
Ahmet’s worked-its-ACC hear-PAST-I

‘I heard of Ahmet’s having worked (= that Ahmet has been working).’

- (24) *Mehmet bu kitabı okumayı istemiyor*
Mehmet this book-ACC read-INF-ACC want-not-PRES-3d sg
‘Mehmet does not want to read this book.’

These examples illustrate typical complement constructions of OV-languages. The Hittite substantive clauses based on verbal nouns accordingly can be accounted for through its highly consistent OV-structure.

Other early Indo-European dialects also include this pattern, such as Vedic. It contains a number of so-called infinitives, which are more accurately identified as “old cases of verbal nouns”; these are very frequent in the *Rigveda* (Macdonnell 1916:190-5, 333-8). Having various case

forms, e.g. the dative in (25), the accusative in (26), they are commonly used as complements, where they in turn may take objects.

- (25) RV 8.71.15 *agnīm dvēṣo yótavāi no grṇāmasi*
 Agni hostility to-ward-off us we-implore
 ‘We implore Agni to ward off hostility from us’
- (26) RV 4.55.7 *nahí. . . . dhāsīm árhāmasi pramíyaṃ.*
 not nourishment we-can diminishing
 ‘We cannot diminish the nourishment’

These verbal nouns are for the most part lost in post-Vedic Sanskrit, as early as the *Brāhmaṇas*. In later Sanskrit, complements are generally made up of finite verbs, introduced by conjunctions. This development is in keeping with the change toward VO structure in post-Vedic Sanskrit. The early period however maintains patterns comparable to those in Hittite.

Evidence for verbal nouns in substantive clauses, and for the change to postposed complement clauses might be cited from the various older dialects. Copious material has been assembled in the standard handbooks, such as Speyer’s for Classical Sanskrit (1886:291-3; 300-9; 347-87), Schwyzler’s for Greek (1950:372-8; 385-96; 639-45), Bennett’s for early Latin (1910:366-428). Even a brief sketch of the complementation patterns in each of the early dialects would extend this treatment far too long. I simply cite evidence for the dialectal origin of complementation patterns, and note how constructions in the early dialects still reflect the complementation patterns of Proto-Indo-European, though modified.

Late or dialectal origin must be assumed from differing constructions in the several dialects. In the western languages a typical pattern is the accusative with the infinitive, widely used in contemporary languages as in the following example; for essays on the development of complementation in early Germanic dialects see JLAS I.3-4, 1976.

- (27) We expect her to come.

This construction, so prominent in Classical Greek and Latin, is not found in Sanskrit; instead the participle is used (Speyer 1886:292).

- (28) *vetsi na mām upasthitam*
 you-know not me standing-near
 ‘You do not know that I am near.’

This use of the “accusative with participle” in Sanskrit as opposed to

the accusative with infinitive in other early dialects alone provides ample evidence that complementation patterns were developed independently in the individual dialects.

The late origin is also clear from constructions which may be interpreted as transitional. Such is the pattern, found in Sanskrit as well as Greek, where the complement is a noun amplified by a descriptive clause (Speyer 1886:359; Schwyzler 1950:645).

(29) *oída tòn ándra hóti díkaiós esti*

I-know the man that just he-is

‘I know that the man is just.’

The construction can be ascribed to an earlier relative pattern, in explicative use as the grammars note. At a still earlier stage the object would have preceded the matrix verb, and the relative clause the object. Subsequently the complement was treated as the object, with inclusion of the noun concerned, as in the English translation above.

Further such transitional constructions are found in the use of infinitives in early Greek. In a careful study Kurzová has pointed out how the infinitive at this stage of Greek filled functions which subsequently were expressed through subordinate clauses and “verbal substantives” (1968). In this way constructions are found which admit of multiple interpretation. We may account for this excessive use of the infinitive by assuming that it was a postposed nominal form, introduced to provide a VO complement pattern. Meillet too pointed out how nominal forms were used to express subordination (1937:373-4). As noted above, some of these have been maintained, when modified in the accusative with infinitive construction; others have been replaced with complement clauses.

In general then the early stages of the older dialects give evidence for verbal nouns as well as “paratactic clauses” as preposed complements; their precise use is being investigated (cf. Justus 1979 a, b). When postposed in accordance with VO order, the verbal nouns are gradually reduced in frequency, to be replaced by finite verb forms, as Delbrück pointed out (1897:440-75). The nominal forms of later dialects, that is, the infinitives, differ from the early verbal nouns in having frozen forms, in being incorporated into the verbal system, and in being used less flexibly.

The postposed complement clauses also underwent changes as the

dialects adopted VO structure. Even in Hittite some substantive clauses were postposed, introduced by *kuit* (Friedrich 1960:163). These essentially were unmodified in other respects. In dialects with more continuous documentation special verb forms came to be characteristic of some of the complement constructions, such as optatives and subjunctives. This use will only be accounted for briefly. It developed from sequences like: 'he said he wishes (optative) to come', which in time were interpreted: 'he said he would come' with a shift in the meaning of the optative. Similar shifts took place with regard to the subjunctive. Thereupon the elaborate system found in dialects like Classical Greek and Latin developed. The handbooks describe these syntactic changes, as we note here briefly of Schwyzler's for Greek (1950:306-7, 311-3, 322, 325, 331-8). Homeric Greek does not yet show a stable pattern for the use of subjunctive and optative in complements; these still alternate with the indicative (1950:333). By the fifth century there are regular patterns. These are introduced by well-developed conjunctions, which in turn as Schwyzler suggests have become dominant (1950:334). Thereupon the optative begins to diminish in frequency, and by the time of New Testament Greek it has been in effect lost (1950:337-8). The expression of complementation has accordingly undergone considerable change in Greek, as it has in other dialects. Description of these changes must be left to handbooks for each dialect.

6. **RELATIVE CLAUSES AND THEIR CHANGE.** Relative clauses according to Meillet are the only subordinate clauses which we have factual evidence to assume as Proto-Indo-European (1937:377). He based this assumption on the reconstruction of **yo-* as Proto-Indo-European relative particle, evidence that was severely weakened by the relative pattern in the Anatolian languages, where, as in Latin, forms of **k^wi-* - **k^wo-* are used. We must therefore interpret the use of **yo-* as another Indo-European isogloss demonstrating close ties between Greek and Indo-Iranian, but not as a feature of the parent language.

Moreover, we have evidence on the time of development of **yo-* as relative marker. This evidence is found in the genitive singular of *-o-* stems. Formed in different ways among sub-groups, it is made with *-*syo* only in those dialects using **yo-* as relative marker. Recent syntactic studies have once again called to our attention the close relationship

between relative constructions and genitives. We may then conclude that **yō-* was introduced in both uses at the same time (Lehmann 1980). The time we can determine from the origin of the thematic inflection, whose oblique cases at least belong to the period of the early dialects. Accordingly we place the time of origin of postposed relative clauses in the early dialect period, not at the time of the parent language itself.

This conclusion requires us to hypothesize the pattern for relative modifiers in Proto-Indo-European. There is slight evidence in Hittite for preposed clauses without a relative particle, though scholars interpret it differently (Jeffers 1976:986). There is no question whatsoever, on the other hand, concerning the predominance of preposed relative clauses, in Indo-Iranian, Latin and also Greek as well as Anatolian. Justus has illuminated this pattern, as noted further below. Such preposed clauses are to be expected in an OV language. Reconstruction of a presumably earlier stage, without a relative marker, must remain uncertain, unless we obtain further evidence from the early dialects.

Before examining the structure of relative clauses in their earliest attested form, we recall other devices which have been proposed for Proto-Indo-European relative constructions, as by Meillet (1937:375-7). One such device is the nominal compound, for which Meillet gives examples like the following from Homer.

(30) *Iliad* 1.231 *dēmobóros* *basileús*
people-devouring king

'un roi qui dévore son peuple – a king who devours his people'

Such compounds are particularly prominent in Greek, but not absent from other dialects, as evidence from the *Rigveda* may indicate.

(31) 1.92.7 *prajāvato* *nṛvato* *āsvabudhyān*
 accompanied-by-children acc.-by-heroes horses-as-basis

<i>uṣo</i>	<i>góagramṇ</i>	<i>úpa</i>	<i>māsi</i>	<i>vājān</i>
dawn	cattle-at-top	to	measure	riches

‘Measure out to us, oh dawn, riches accompanied by children,
by heroes, having horses as foundation and cattle as supreme’.

Here, as the translation indicates, a series of compounds each of which corresponds to a relative construction precedes the head-word. Such compounds may also follow the head-word, as in the next stanza of this hymn.

- (32) 1.92.8. *úṣas tám aśyāṇṇ yaśásam suvīram*
 dawn such I-would obtain respect excellent-in-men
dāsāpravargaṇ rayīm áśvabudhyam
 slaves-as-beginning wealth horses-as-basis
 ‘Oh dawn, I would like to obtain such wealth, which brings respect, which is outstanding in men, which has slaves as beginning and horses as basis.’

The compounds may also be accompanied by forms of *yo-, probably as focus marker.

- (33) RV 10.26.5 *ṛṣiḥ sá yó mánurhito*
 Rishi the who disposed-to-men
víprasya yāvayatsakhāḥ
 of-poet warding-off-(enemies)-friend
 ‘the Rishi who is friendly to men,
 (who is) a protecting friend of the poet’

Compounds, comparable to participles accompanied by nominal elements as Jacobi had proposed (1897), then provided one device for relative constructions in the early dialects, and presumably in Proto-Indo-European.

Another device, as Meillet pointed out, consisted of participles, which according to him had immense importance in the earliest texts, as is clear from the Homeric poems. Two examples from the first lines of the *Iliad* may be noted.

- (34) 1.58 *toṣi d’ anistámenos metéphē pódas ōkùs Akhilleús*
 them ptc standing-up spoke-to feet swift Achilles
 ‘Swift-footed Achilles, who then stood up, spoke to them.’
 (35) 1.56 *kédeto gàr Danaôn, hóti rha thnēiskontas horâto*
 she-pitied for Danaans because ptc dying she-saw
 ‘for she pitied the Danaans, because she saw them (who were) dying’

Such participial constructions may well have served as descriptive relative constructions in the early dialects and Proto-Indo-European, as also in the following Hittite example (Otten-Soucek 1969:24).

- (36) *mān MUSEN hāranan hušuuandan appanzi n-an údanzi*
 if eagle living they-capture ptc it they-bring hither
 ‘If they capture an eagle which is alive, they bring it hither.’

Yet these participles and compounds provide only residual evidence for relative clause structure in the parent language. We are however better informed of the change of preposed clauses to VO patterning, which has been clarified in articles of Justus (1976,1979; see also Lehmann 1974: 61-8)

There is no need to repeat, or to supplement the careful argumentation of Justus supported by well-chosen illustrations. We may simply recall that she identifies *ku* - as an anaphoric element indicating a focus. As Hittite developed away from OV-structure, which requires no indicator for relativization, such focus-indicators came to be more prominent, and eventually served to indicate relative constructions, while developing into relative pronouns. Such indicators for relative constructions are particularly necessary in VO-languages.

Justus cites adequate examples outlining the shift from focus-indicators to relative markers in Hittite (1976:233-8). The introduction of *kuis*, which she notes in a late version of a legal passage whose earlier version has no relative indicator, provides an elegant demonstration of the syntactic development, answering also Jeffers's objection to this interpretation (Justus 1976:237-8; Jeffers 1976:986). We may therefore accept this development as assured, even though the evidence for Indo-European relative clauses with finite verbs as sole marker is unfortunately inconclusive.

The occurrence of the relativized noun in both the relative clause and the matrix seems, however, to be a retention of OV patterning; the following is an example from Hittite (Götze-Pedersen 1934:6).

- (36) GUD-pūḫugarin-ma kuedani UD-ti únuēr nu-za dUTU-ŠI
 ox-scape ptc which-on day-on they-decorated ptc ptc sungod (=I)
 apēdani UD-ti warapta
 that-on day-on he-bathed [the sun-god bathed = I bathed]

'I bathed on the day on which (day) they adorned the scape-ox.'
 Such patterns of repetition are not found in English, nor indeed in VO languages. Their presence in Hittite and other early Indo-European dialects illustrated below may be accounted for by comparing the OV language Japanese.

In Japanese such noun phrases may be represented in the relative as well as the matrix clause, not solely by repetition of the noun itself but also by means of anaphorics, as example (b) illustrates; the example is

from Kuno (1973:237).

(37) a. [watakusi ga sono okyaku-san no namae o wasurete-simatta]

I that guest 's name have forgotten
 okyaku-san
 guest

b. [watakusi ga sono hito no namae o wasurete-simatta]

that person
 kare
 he
 so
 that

okyaku-san

‘a guest whose name I have forgotten’

The Japanese parallel may help clarify the genesis of the relative pattern in Hittite and other Indo-European languages, in this way answering Jeffers’s query “why....elements like **k^we-* and **yo-* came to be reinterpreted as relative pronouns with such consistency in the dialects, if Proto-Indo-European lacked any form of relative marking” (1976:984). Comparable to the Japanese anaphoric elements *sono*, *kare*, and *so*, Indo-European anaphorics, whether **k^we-*, **yo-* or **so-* were used in the relative clause to identify the noun used as focus in the sentence. As the focussed noun was omitted in the embedded clause, the anaphoric came to be a relative signal, often a relative pronoun.

Parallels may be cited from Vedic Sanskrit and other early dialects; see also Delbrück 1871:54.

(38) RV 1.35.11 *yé te pánthāḥ.....tébhir no adyá pathíbhiḥ*
 which your paths on-those us today on-paths
 ‘whatever your paths, on those paths for us today.’

The noun may thereupon be omitted in the relative clause.

(39) RV 3.48.2 *yáj jáyathās, tād dhar.....pīyūṣam apibo*
 which you-were that day milk you-drunk
 ‘On the day on which you were born, you drank the milk’

Finally the anaphoric element may be omitted from the matrix sentence, though as here (*id*) a particle may serve to indicate the focussed element.

- (40) RV 5.3.7 *yó* *na* *āgo* *abhy* *éno* *bhārāty*
 who us evil on sin bears
 ādhi *aghām* *aghāsanse* *dadhāta*
 to-ptc evil evil-thinker place
 ‘Who would like to bring evil and sin on us,
 unload evil on the evil-thinker indeed.’

Early Latin includes similar examples where the focussed noun occurs in the relative as well as the matrix clause; this pattern persists in the conservative legal language, as in (14) and in (41) (Szantyr 1965:563-4).

- (41) Lex ag. 4 *quei* *ager* *publicus*....*fuit, extra eum agrum*....’
 ‘which field was public, outside that field....’

As Paolo Ramat has pointed out to me in communication “the repetition of the noun in the extraposed relative clause is a strategy for preliminary focussed information about the noun; the principal clause will then predicate something new about it.” Subsequently the relative clause is postposed, and there is no necessity for repeating the focussed noun.

The development of the later pattern, as in the Latin literary language, is treated amply in the handbooks on Latin grammar. The older patterns of Latin, however, provide evidence for an OV relative construction as it was changing to the VO pattern, as did also Vedic Sanskrit and Hittite examples.

7. **ADVERBIAL CLAUSES AND THEIR CHANGE.** The elements used in the later dialects to indicate adverbial clauses, like those of the other subordinate clauses, develop from particles which were markers for focus. Delbrück has discussed fully the adverbial particles for the dialects known at his time. While numerous examples could be given here, the situation is so well-documented that a few will suffice, especially since further analyses were made by later Indo-Europeanists (Delbrück 1900:295-338; 406-47; Lehmann 1974:167-74). These analyses illustrate in detail how the mechanisms used in VO-languages to indicate adverbial clauses were gradually developed in the various dialects.

Before particles were used in this way, adverbial clauses were expressed through juxtaposition of clauses, as Hittite informs us with its unmarked purpose and result clauses (Friedrich 1960:193). Participles also served this function, as Meillet pointed out and as the handbooks illustrate (Meillet 1937:373-5; Chantraine 1963:319-29). Temporal, causal and concessive relationships are expressed by participles in Homer,

‘Come hither, friends, (so that) we can perform worship’

As illustrated in the previous example (39), *yád* could as readily be interpreted as a subordinating particle, not merely as relative marker.

In the course of time further particles came to be introduced, to mark more specifically the various types of subordinate clauses, such as *yathā* in the following example.

(45) RV 10.85.26 *grhān gacha grhāpatnī yāthāsaḥ*
 house go house-mistress through-that you-may-be

‘Go into the house, so that you may be house-mistress!’

These particles then became the primary indicators of subordinate relationship, as noted above. The various conjunctions, such as Sanskrit *yādi*, Greek *ei*, Latin *sī*, Gothic *jabai* for the conditional clause, and similarly varied forms for the other conjunctions, indicate that they were adapted in the dialect period.

It is remarkable that Hittite underwent a similar development, with different lexical elements: *kuit* signal causal clauses, and also like *kuit-man* and *kuwapi* temporal: *mān* and *maḥḥan* signal conditional and concessive clauses (Friedrich 1960:163-7). Since examples in Friedrich and elsewhere illustrate the situation within Hittite and its changes, one citation may serve here. (Oettinger 1976:6):

(46) *kāš-ya kuit TI-anza ēšta nu šarā nepēš ūemiškit*
 he ptc what healthy was ptc above heaven he-could-see

‘When he was hale (unblinded), he could see the heavens above.’

Like *yaj* in the Vedic example above (39), *kuit* indicates subordination which might be interpreted variously: ‘as, because, when’. Hittite too has adapted focussing particles to signal subordination, presumably in parallel development with the other dialects.

The shift from a loose relationship between subordinate and matrix clause to a more specific relationship is characteristic of a shift from OV to VO structure. In OV languages, initial unspecified subordinate clauses may be interpreted when the matrix clause is completed, as might be illustrated with countless Japanese and Turkish examples. Sentence (39) above may serve instead as illustration. The initial subordinate clause might be interpreted as a relative, elaborating on the day in question: ‘the day on which you were born.’ It could be taken as temporal, corresponding to: ‘when you were born.’ Still other interpretations are possi-

ble, such as causal. Such latitude of interpretation is avoided in VO languages. The tighter relationship required in these languages might well have developed from focussed expressions reinterpreted as relative or adverbial constructions.

8. **CONCLUSION.** The general outlines of the development of compound and complex sentences in Proto-Indo-European and the early dialects are then clear. At the earliest ascertainable period clauses were combined in apparent paratactic relationship, as might be expected of an OV language. Subordinate clauses were placed before principal clauses. In this arrangement the verb maintained its high pitch accent, giving to the subordinate clause an intonation pattern comparable to that of the initial clause of a complex sentence in English. Evidence from Vedic Sanskrit supports this conclusion. The early dialects: Anatolian, Greek, Latin and even Germanic also support it though providing only indirect evidence on the intonation. The arrangement of the clauses is however clear.

The earliest indicators apart from intonation patterns are found in relative clauses. These are still preposed, but the relativized noun is singled out by a particle, to mark its status as focussed. Earlier scholars pointed out the use of such anaphoric elements, though not precisely. Gonda, for example, described **yo-* as “originally...an introductory, announcing; isolating; explaining, qualifying, defining, distinguishing pronominal word... [not] a full-grown relative pronoun” (1954:9, 18). And Speyer, citing the Vedic example below and indicating his interpretation in the translation stated that “a relative clause with nominative predicate is often not much but a merely explicative expression” (1896:84).

(47) *ya āryas taṃ praśna*
 who noble him ask

Literally: The one who is noble, ask him.

‘Ask a man of honor.’

It remained to Justus to clarify the use of the anaphoric particles, and to identify their role in the changing syntax.

The time of introduction of **yo-* as relative marker, and presumably **k^wi-* - **k^wo-*, can be dated by comparison with the genitive singular ending of *-o-* stems as the late Indo-European period when dialects were clustered in sub-groups which were no longer in close enough contact to adopt innovations throughout the entire set of dialects. In the early

period complex sentences could occur with the relativized noun included in the principal as well as the subordinate clause. As illustrated above, such repetition is attested for OV languages if not for VO (37). The massive changes which thereupon occur indicate a shift to VO-patterning. Subordinate clauses are then placed after the principal clauses. The focussing marker, whether from **yo-*, **k^wi-* / *k^wo-* or **so-*, then becomes a relative marker.

Even before the postposing of subordinate clauses, some clauses including one of these markers involved relationships that were adverbial rather than adjectival. Again we can cite from the standard handbooks (Speyer 1896:87).

(48) *yát sāvám juhóti réta evá tát siñcati*
 what evening offers semen ptc that pours-out

Literally: What offers every evening, that [is equivalent to] pouring out semen.

‘To offer every evening is like pouring out semen.’

The first clause here provides the circumstance for the main clause; it might be translated as a temporal or conditional clause. The following is taken by Speyer as a causal clause, a relationship resulting at least in part from the form *téna*.

(49) *yán nāvedīṣam tēnāhimsiṣam*
 what nnot-I-knew for-that-I-injured

‘Because I did not know (you), I injured (you).’

Calling this use of *yad* ‘uralt’, Speyer like other scholars considers it the oldest of the adverbial conjunctions to arise. He bases this conclusion in part on its broad use in Vedic, where it can still introduce temporal and conditional clauses. We interpret the broad use as focussing on an adverbial relationship without specifying that relationship further. This latitude of preposed subordinate clauses is characteristic of OV languages, as for the *te*-forms of Japanese and the participles of Turkish; it can be maintained in them, in part because elements like *téna* in the second clause indicate the relationship between the two clauses.

In VO-languages, on the other hand, the subordinate clause itself contains the elements necessary for its interpretation, inasmuch as the principal clause often precedes and has no indication of the kind of subordination to follow. Hence more specific markers like conjunctions

arise. Further, additional markers may be used, such as specific verb forms, as noted by Hermann (1895). These may be most essential in substantive object clauses, referred to as indirect statements and questions. An OV language marks off quotations with final particles, like Japanese *to*, or *iti* maintained in Sanskrit. But such final markers are obviously impossible in VO-languages, because the initial segment of the quotation is not clear from them. Such devices did develop in Classical Greek and Latin, with regulated use of the subjunctive and optative, shifts in person, and development of specific conjunctions. The general outline of such devices in their use was presented above; precise descriptions must be left to investigators of individual dialects, or even selected individual authors in these dialects. In this way we will be able to determine the emergence of the patterns found in each dialect, and also likely paths of development when languages shift from OV to VO structure. Such syntactic studies will give us a deeper understanding of the Indo-European languages in their development, and of syntactic change.

CHART I.

Hermann's list of characteristics of subordinate clauses.

1. special clause-connecting words
2. shift of person
3. shift of mood
4. shift of tense
5. clause-accent of the clause
6. tempo
7. duration of pause between the clauses
8. clause arrangement
9. position of words
10. clause-accent of the verb
11. method of compounding of the verb (kompositionsweise)
12. words which are not clause-connectives that occur only in subordinate clauses.

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ORIGIN OF INDO-EUROPEAN PARATAXIS

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1. **PARATAXIS AND HYPOTAXIS.** The relation between parataxis and hypotaxis has not been precisely defined to this day in spite of long discussions on the subject, which on the other hand allowed us to discover certain formal differences between them. The most universal seems to be the principle that in subordinate clause neither the imperative nor interjections can be used whereas parataxis is characterized by the possibility of transposition of constituents, and the conjunctions which occur here can join words. Thus, as paratactic we can assume copulative, alternative, disjunctive, and adversative connections only, and consequently, various kinds of causal, resultative, consecutive etc, sentences which are sometimes involved here, have to be excluded.

When examining the languages of different families one can come to the conclusion that the opposition 'parataxis/hypotaxis' has a universal character. Considering this, it is impossible to state empirically which of these two constructions is older or which has arisen from which. The most widespread theory¹ which says that hypotaxis has arisen from parataxis is based on the fact that it is less frequent in colloquial language and in children's speech, while in the historical development of different languages it expands at the cost of parataxis. In some languages, on the contrary, we can observe the expansion of parataxis at the cost of hypotaxis, e.g. in Modern Greek (Mirambel 1963), Late Latin (Hofmann,

Szantyr 1965: 527-528), in contemporary French (Damska - Prokop 1960: 57), Polish (Wierzbicka, 1962), etc.²

2. **FORM AND ORDER OF CONSTITUENTS IN PARATAXIS.** The sentence is the basic constituent of paratactic construction. In extreme cases it may occur as a single word or as a developed period in co-ordinate relation with another sentence. Paratactic conjunctions also join parts of the sentence. The terms parataxis and hypotaxis may occasionally be applied to morphological connections, i.e. composite. Consequently, we could define as paratactic all elements of equal rank at all levels of the language. It seems desirable, however, to limit this term to strictly syntactic (i.e. word and sentence) connections whereas the analogical connections at other levels could be defined as co-ordinate, because, in spite of some isomorphism³, there are essential formal differences between them. At the level of morphological connections the so-called connective affix⁴ is an equivalent of conjunction. It does not fulfil any semantic value but serves only to connect significant morphemes; at the phonetic level a similar role is played by the so-called glide-sounds (epenthesis)⁵. Although some conjunctions occur at the level of stylistic organization of the text, in connections of greater syntactic units, yet they lose their specific semantic function, serving but to introduce or refer to the preceding texts.

Constituents of paratactic construction can generally be transposed without changing the meaning of utterance, yet their order in given texts is not indifferent though perhaps it is less determined than in hypotaxis and in word connections of unequal rank. As it depends on stylistic factors, it is difficult to state any precise rules. The emotional factors should also be taken under consideration; we can, however, quote here (especially in word connections) certain phonetic, prosodic and semantic tendencies. In phonetic we can distinguish vowel alternation: *high* in the preceding member - *low* in the following one, as well as labial initial of the following member⁶. In prosody we may quote the 'law of increasing members' discovered for 'dvandva' by Pāṇini (2.2.34) and rediscovered for syntactic units by O. Behaghel (1909: 110-142)⁷. In semantics we should mention a predilection to place at the beginning the more important, nearer and logically basic notions (Krause 1922: 74-129, Behaghel 1923: 3.367-368), which can be reduced to the two main prin-

ciples: importance and sequence. When both factors contradict each other we have the stylistic figure 'hýsteron - próteron'.

As regards the linear contact of joined constituents, the developed system of syntactic congruence in Indo-European makes possible a relatively free order of constituents in utterance, thanks to which syntactically connected words need not be in immediate contact with each other. For such connection in classical poetics we find the term 'hypérbaton' whose special variant was 'schema Alkmanikon', i.e. a co-ordinate combination of words which are not placed beside one another.

3. **THE PROBLEM OF ASYNDETON.** In the historically attested Indo-European languages asyndeton is a facultative stylistic variant of the conjunctive construction. As regards the Proto-Indo-European period, the fact that there existed at least two paratactical conjunctions: **k^he* and **^huē*, allows us to suppose that co-ordinate word asyndeton was of no great importance in the period which directly preceded the disintegration of the proto-language. However, its presence with copulative function is indicated by 'dvandva' composita and certain old poetical connections. The alternative function may have been realized by interrogative intonation as in the case in Old Indian 'plūti'. The disjunctive connection was preasumably achieved by negation, while adversative meaning may have been expressed by difference of matter. In spite of certain opinion (Watkins 1964) there is no sufficient basis for the reconstruction of elements joining greater syntactic units in Proto-Indo-European. This seems to indicate that asyndetic connections were more frequent between sentences than between words.

4. **PROTO-INDO-EUROPEAN TYPE OF CONJUNCTION AND ITS EVOLUTION.**

On the ground of all early attested Indo-European languages we can suppose that in the final period of the proto-Indo-European community the copulative function was fulfilled by **k^he* which has been preserved as an independent conjunction in Indo-Iranian *ča*, Lycian *ke*, Old Phrygian *ke*, Greek *te*, Messapic *ti*, *si*, Latin *que*, Venetic *kve*, in the inscription from Ornavasso *pe*, from Picenum *-p*, Gaulish *-c*, Celto-Iberian *C(u)e*, Old Irish *-ch-*, Gothic, Proto-Nordic *-(u)h*, as well as combined with other elements: Tocharian 'A' *ś-kam*, (?) Hittite *ni-kku*, Lydian *ni-k*, Armenian *o-č*, Albanian *mo-s*, *a-s*, Osco-Umbrian *nei-p*, Welsh *a-c*, *na-c*, Cornish Breton *(h)a-g*, *na-g*, Old Germanic *ja-h*, *ne-h*, (?) Slavonic *ješ-čē*. The etymo-

logy of this conjunction is not clear⁸. The copulative function has probably arisen from enhancing-strengthening meaning which is attested in **k_ue* after the indefinite pronoun in Hittite, Indo-Iranian, Armenian, Greek, Italic and Gothic.

In the epoch of dialectal disintegration the copulative function of **eti* attested in (?) Phrygian, Italic, (?) Gaulish and Gothic, as well as of **od* attested in Balto-Slavonic and Indo-Iranian, may have arisen.

As Proto-Indo-European alternative conjunction we may consider **uē*, preserved in Tocharian 'B' *wa-t*, (?) Tocharian 'A' *pa-t*, Old Indo-Iranian *vā*, Greek *ē-é*, *ē*, Latin *ve*, Celto-Iberian *ue*; after negation: see e.g. Hieroglyphic Hittite *napa-wa*, Lycian *ne-u*, Old Irish *nó*, *nú*, Welsh *ne-u* Old Breton *no-u*. The etymology of **uē* is also not clear enough. It is generally compared with the Old Indian *iva* 'like, as it were', Greek *ēúte* 'as, like as', Latin *ceu* 'as, just as'. In comparison with **k_ue* it can be observed that fewer direct continuants of **uē* have been preserved and that postpositive usage was removed earlier. This is caused by the fact that this conjunction is less frequent, and thus its susceptibility to changes is greater. In disjunctive function the repeated sentence negation may be used in most Indo-European languages, whereas in Vedic and in a few Modern Indo-Aryan languages the autonomous disjunctive conjunction does not occur at all. It seems that the original Proto-Indo-European state has been preserved here (Delbrück 1893-1900:2.513). However, judging from geographical repartition we can infer that the tendency to the connection **ne-k_ue* dates back to the epoch of the proto-languages, since it is attested in classical Sanskrit, Anatolian, Italo-Celtic, Germanic and with renewed negation in the Greek *oú-te*, Albanian *a-s* and Armenian *o-č*. In Indo-Iranian, Anatolian and Italo-Celtic the continuants of the parallel **ne-uē* occur. With prohibitive shade **mē-k_ue* is attested in Indo-Iranian and Greek; Albanian *mo-s* and (?) Tocharian *mā-k* 'non' are its formal equivalents. The autonomous disjunctive conjunction arose when to iterated negation with each constituent the copulative (or alternative) conjunction or intensifying particle was added to mark the connection in the further position (**ne ... ne-k_ue*) and then in the initial one to emphasize parallelism (**ne-k_ue ... ne-k_ue*). Both schemes are attested in the texts from the oldest times⁹.

To the Indo-European dialectal innovations belong: Old Saxon *nek*

= Baltic *negi* related to Old Indian *nahi* ‘certainly not’ etc. Also Avestan *naēda*, *māda*=Greek *oudé*, *mēdē* may be inherited (Wackernagel 1926 - 1928:2.309).

The fact that there is no ground for the reconstruction of Proto-Indo-European adversative conjunction does not prove that the adversative function should not exist in the syntactic system of the proto-language. It seems that this function was partly fulfilled secondarily by the copulative **k_ue*, as can be shown by the examples in the texts of early attested Indo-European languages (Gonda 1954: 195-200). The lack of a special adversative conjunction in the proto-language may be explained by the fact that the postpositive type of conjunction which occurs there, was originally polysyndetic and belonged to the ‘word type’, (see below) whereas the adversative function is limited to monosyndetic, mainly sentence constructions.

The autonomous adversative conjunction appeared, as it seems, in the epoch of dialectal disintegration of the proto-languages. Here belong: **at(i)* occurring in this function in Avesta, Greek, Latin and Gothic; **mā/me* in Anatolian, Greek and Messapic; **auti* in Greek and Italic; the generally copulative **ōd* in Iranian and Balto-Slavonic.

The continuants of **k_ue* and **u_e*, if they occur independently, are generally postposed after the second (and the following) constituent, and in Indo-Iranian, Anatolian, Greek and Germanic they may also be placed with all constituents (polysyndeton). The last mentioned usage seems to be original. It is more frequent in poetry (Speijer 1886: 331, Denniston 1934: 503), which is stylistically more archaic than prose, and was lost earlier than the first usage. The postpositive character predestined both conjunctions to word connection, bringing them closer to derivative-inflectional morphological formants such as suffix and ending and the conjunction was placed after each word to which it referred. It followed the stem, having no stress of its own¹⁰, and in case of simplification of polysyndeton it was omitted (like suffixes and endings in agglutinative languages) together with the preceding constituent. Thus, we can name this kind of conjunction a ‘word type’. A similar conjunction can be found in other families, e.g. Caucasian, Altaic, Tibeto-Chinese and in some American Indian languages. In Indo-European languages beside **k_ue* and **u_e* other postpositive conjunctions occur, notably in Tocharian, Old Indian

and Hittite; however, the polysyndeton is here frequent and therefore those conjunctions do not belong to word type 'sensu stricto'.

Because of the need to join greater syntactic units postpositive polysyndeton could not be preserved. The starting point here was the simplification of polysyndeton and the fixing of the position of conjunction in word groups by Wackernagel's law. The emergence of new anteposed conjunctions of the sentence type belongs to the epoch of the Indo-European dialects. These innovations affect one or (more rarely) a few groups of languages related to one another. In the historical period conjunctions of the sentence type occur already in all - excepting Mycenaean Greek - languages gradually eliminating the continuants of **k^huē* and **u^hē*. In Old Indo-Iranian *ča* and *vā* are still quite frequent; they became obsolete towards the end of the Middle Indo-Iranian period, whereas to this day *ča* has been preserved in a few Indo-Aryan languages and in Yazgulami. In Homer *te* is twice less frequent than *kaí* and it disappeared in the Koiné period. In archaic Latin *que* is customary; however, in Plautus *et* is twice as frequent; in colloquial Latin *que* and *ve* become obsolete at the beginning of the imperial epoch, being more persistent in the literary language. In the Celtic areas the continuant of **k^huē*, attested in Gaulish and Celto-Iberian, occurs in Old Irish but only in combinations with certain deictic-accessory elements. In Germanic this conjunction showed a certain vitality only in Gothic.

The fact that the continuants of **k^huē* and **u^hē* connected with order elements are preserved in anteposed positions proves their removal to be the result of their postpositive usage¹¹. Thus, for instance, Indo-Iranian *vā* in the connection *adha-vā* (*atha-vā*), which gradually became widespread in Middle Indo-Iranian, is known in certain Indian and most Modern Iranian languages. Similarly in Greek *é*, Rumanian *sau*, Welsh *neu* the continuants of the Indo-European **u^hē* have survived to this day. The continuants of **k^huē*, too, show a greater durability in anteposed connections: in Old Germanic *ja-h* is a normal copulative conjunction; **ad-k^huē* has preserved to this day in the languages of the Brythonic group and in some Romanic dialects. The connection **ne-k^huē*, which goes back to the Proto-Indo-European epoch, is attested in most languages. Also combinations of both types of conjunction, such as Indo-Iranian *uta ... ča*, Greek *te ... kaí*, Latin *et ... que*, Gothic *jah ... (u)h*, etc.

are possible.

5. **SOME TYPOLOGICAL CONCLUSIONS.** As it seems, the parataxis has a universal character, and so we may regard the Indo-European parataxis as representative for all languages that have a similar structure and the same degree of development. Among different typological classifications the distinction between 'nominal' and 'verbal' languages (cf. Capell 1965), which can be seen in the structure of the predicate and complex sentence, is worthy of notice. In the 'nominal' languages the so-called pure nominal proposition and participial constructions prevail, whereas in the 'verbal' ones the personal verb forms in the function of predicate and hypotaxis are more frequent. As regards parataxis in the 'nominal' languages, the word type conjunction prevails, while in the 'verbal' ones the sentence type conjunction decidedly predominates. As to the diachronic aspect, the process of replacement of word type conjunction by a sentence type - which is the most characteristic syntactic development of the Indo-European parataxis - may be understood as symptomatic for the general evolution of syntax. It seems to consist in the change from word to sentence constructions, which, for instance, can be found in: (1) gradual elimination of pure nominal proposition (Moreschini Quattordio 1966), (2) replacement of the ergative (by origin nominal) construction by a predicative one, (3) replacement of participial, gerundial and infinitive connections by a complex sentence¹².

FOOTNOTES

- (1) Bühler 1934: 398-418, differently Paul 1920: § 100-102.
- (2) The conception that hypotaxis has arisen from parataxis is theoretically imprecise, both notions being mutually conditioned: if it were not for hypotaxis, parataxis could not exist. Both types of connection seem to have arisen independently, from loose syntactic sets which formally may resemble asyndetic parataxis.
- (3) This isomorphism can be expressed by: (1) the appearance or the lack of special joining elements, (2) the modification of structure of joined units (special variants, prosodic features etc.). On the notion of isomorphism cf. Kuryłowicz 1949.
- (4) The notion of connective morpheme (Verbindungsmorphem) which serves only to join other morphemes was introduced by Trubetzkoy 1934: 14-15. Cf. also Netteberg 1961.
- (5) On syntagmatic phonology and morphology see Bally 1950: § 163.
- (6) Cf. Wackernagel 1938: 166-170, where further literature can be found. The connection between back vowels and labial consonants (grave) in opposition to front vowels and other consonants (acute) was revealed in spectral analysis.
- (7) As a parallel from morphology we may quote the tendency to lengthening final vowels in Greek and Latin, cf. Safarewicz 1934: 10-13.
- (8) It is usually derived from the interrogative pronoun **kko-*; however, the explanation of its semantic function is difficult, cf. Meillet 1898: 272-273, Gonda 1954: 177-181.
- (9) Except for Old Icelandic where the combination *nē ... nē* is not attested, cf. however Gothic *nih ... nih*.
- (10) Cf. the Greek atonic *te*, versus other stressed conjunctive enclitics, e.g. *dé*.
- (11) According to Bally 1950: § 350, the elimination of the postpositive usage of conjunctions is the result of a tendency to 'progressive' word order. Cf. also Dik 1968: 41-48.
- (12) The paratactic constructions and conjunctions in the various Indo-European languages have been more exhaustively presented in Bednarczuk 1971.

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DER INDOGERMANISCHE **kwi-/kwo-* RELATIVSATZ
IM TYPOLOGISCHEN VERGLEICH

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1. ZUR ANWENDUNG DER TYPOLOGIE AUF DIE INDOGERMANISCHE SYNTAX. Um die Syntax des Indogermanischen zu erschliessen, stehen uns hauptsächlich zwei Wege offen: Wir können entweder die historisch-vergleichende Methode auf die Syntaxen der indogermanischen Sprachen anwenden und so die Syntax der historisch zugrundeliegenden Sprache rekonstruieren. Oder wir können die typologisch-vergleichende Methode auf die Grammatik des Indogermanischen und typologisch verwandter Sprachen anwenden und von bekannten grammatischen Eigenschaften des Indogermanischen auf sonst nicht bekannte syntaktische Eigenschaften schliessen. Die zweite Methode setzt voraus, dass gewisse Bereiche einer Grammatik bereits bekannt sind; d.h., eine Anwendung der typologisch-vergleichenden Methode auf das Indogermanische setzt notwendig Ergebnisse der historisch-vergleichenden Methode voraus, nicht aber umgekehrt.

Die Schlussverfahren, die bei der Aufdeckung syntaktischer Eigenschaften aufgrund der typologisch-vergleichenden Methode angewandt werden, beruhen grundsätzlich auf dem Syllogismus, haben also im einfachsten Falle die Form von *modus ponens* ("Wenn eine Sprache x hat, hat sie auch y. Das Indogermanische hat x. Also hat es auch y".) oder *modus tollens* ("Wenn eine Sprache x hat, hat sie auch y. Das Indogermanische hat y aber nicht. Also hat es auch x nicht."). Ein solcher

Schluss ist so gut wie seine Prämissen. Die jeweils zweite Prämisse ist, wie gesagt, durch Ergebnisse der historischen Rekonstruktion abgesichert, deren Gültigkeit hier nicht zur Debatte steht. Die jeweils erste Prämisse ist eine implikative Generalisierung über alle Sprachen, die durch Ergebnisse des historischen Vergleichs gesichert ist. Der Schluss ist in dieser kategorischen Form nur zulässig, wenn die Generalisierung ausnahmslos ist; andernfalls hat er nur eine gewisse Wahrscheinlichkeit für sich. Im Bereich der Syntax sind nicht sehr viele solche Generalisierungen bekannt, zu denen nicht auch Ausnahmen bekannt wären.¹

Ein weiteres formales Hindernis mindert den Nutzen solcher Generalisierungen bei der Erschliessung sonst nicht bekannter Eigenschaften. Ein hieb- und stichfestes implikatives Verhältnis setzt eine einseitige Abhängigkeit voraus; Ausnahmen zu den typologischen Generalisierungen finden sich charakteristischerweise dort am ehesten, wo eine solche nicht besteht. Einseitige Abhängigkeiten bestehen immer dann, wenn eine komplexe Konstruktion auf einer einfacheren basiert.² Regelmässig steht die komplexe Konstruktion im *implicans* und die einfachere im *implicatum* einer implikativen Generalisierung. Wenn wir nun eine Sprache nur teilweise kennen, werden wir normalerweise eher die einfacheren als die komplexeren Konstruktionen kennen. Das bedeutet aber, dass der Einsatz unserer Implikation in einem *modus-ponens*-Syllogismus uns nichts Neues über die Sprache lehrt. Z.B. könnte man sich mit der Kenntnis, dass das Indogermanische einen Dual hatte, und mit der Farge, ob es auch einem Trial hatte, an die Typologie wenden, und wäre enttäuscht zu erfahren, dass aufgrund einer implikativen Generalisierung das Indogermanische auch einen Dual gehabt haben muss, falls es einen Trial hatte. Unter solchen Voraussetzungen ist allermeist bloss der *modus tollens* für die Erschliessung sonst nicht bekannter Eigenschaften einsetzbar, da man hier die Kenntnis weniger komplexer Konstruktionen nutzen kann. Dafür erfährt man hierbei aber auch regelmässig nur, was das Indogermanische nicht gehabt hat, so dass auch dieses Verfahren von begrenztem Interesse für die Indogermanistik ist, es kann vor allem dazu dienen, Hypothesen über Eigenschaften des Indogermanischen zu widerlegen bzw. auszuschliessen.

Während also für eine Anwendung formaler typologischer Methoden zur Erschliessung von Eigenschaften des Indogermanischen weit-

gehend sowohl die Voraussetzungen als auch das Interesse fehlen, impliziert dies natürlich in keiner Weise, dass der typologische Vergleich ohne Erkenntnisgewinn für die Indogermanistik ist. Das Interesse ist in erster Linie ein heuristisches, und so ist es auch von Anbeginn der Indogermanistik immer wieder verstanden worden. In diesem Sinne ist es nützlich, das Indogermanische mit Sprachen zu vergleichen, die demselben Sprachtyp angehören oder mindestens in einem bestimmten Untersuchungsbereich typologisch mit ihm verwandt sind. Hier hat nun die Entwicklung der Sprachtypologie, und zwar gerade der mit der Indogermanistik gekoppelten Sprachtypologie, in den letzten Jahren einen m.E. nicht durchweg günstigen Verlauf genommen. Es herrscht eine verbreitete Tendenz, die Sprachtypologie auf syntaktische Typologie und diese auf Grundwortstellungstypologie (*basic order typology*) zu reduzieren.³ Die Morphologie bleibt dabei weitgehend unberücksichtigt, und um eine einfache Formulierung der Wortstellungsgesetze zu erreichen, bleibt auch Wortstellungsfreiheit im allgemeinen ausser Betracht. Es liegt auf der Hand, dass diese Sicht, die bereits von einem allgemein-typologischen Standpunkt betrachtet verkürzt erscheint, erst recht verzerrte Ergebnisse hervorbringen muss, wenn sie auf das Indogermanische gewendet wird.⁴ Wenn wir irgendetwas über die indogermanische Syntax zu wissen glauben, dann sicherlich, dass Morphologie und, im Zusammenhang damit, Wortstellungsfreiheit in ihr eine prominente Rolle spielten. Wenn also das Indogermanische mit dem Türkischen und Japanischen verglichen wird⁵, nur weil in ihm höchstwahrscheinlich, ebenso wie in diesen, die Hauptkonstituentenstellung SOV dominant war, so reicht das nicht nur nicht aus, sondern ist im Gegenteil methodisch ziemlich unfruchtbar, weil dies beides agglutinierende Sprachen mit ziemlich strenger Wortstellung sind.⁶

2. **DER ANGESCHLOSSENE RELATIVSATZ.** Ein restriktiver Relativsatz ist ein Nebensatz, der auf der Basis eines nominalen Begriffs, welcher sein Nukleus ist, einen spezifischeren Begriff bildet.⁷ Der Nebensatz eröffnet entweder eine Leerstelle, in die der Nukleus semantisch eintritt, wenn der Nebensatz mit ihm durch Attribution verbunden wird; oder der betreffende nominale Begriff besetzt selbst eine syntaktische Position im Nebensatz, wobei die Begriffsbildung nicht durch Attribution im engeren Sinne, sondern durch ihre Konverse, die Nukleusbildung zustandekommt.

Ueberkreuz zu dieser Einteilung läuft eine zweite nach dem Kriterium, ob der Relativsatz eingebettet ist, also als eine Konstituente mit bestimmter syntaktischer Funktion im Matrixsatz fungiert, oder ob er bloss dem Hauptsatz angeschlossen ist. Im letzteren Falle steht er regelmässig am Rand des Hauptsatzes, wobei Voran- oder Nachstellung, innerhalb der syntaktischen Möglichkeiten der Sprache, von der funktionellen Satzperspektive abhängen, während im ersteren Falle die Position des Relativsatzes, zusammen mit seinem Nukleus, in erster Linie von der syntaktischen Funktion der Relativkonstruktion im Matrixsatz bestimmt wird.

Der eingebettete Relativsatz steht, wenn er eine Leerstelle eröffnet, entweder prä- oder postnominal, oder aber er enthält seinen Nukleus und ist dann zirkumnominal. Der angeschlossene Relativsatz ist dem Hauptsatz nachgestellt, wenn er eine Leerstelle enthält, und vorangestellt, wenn er den Nukleus enthält. Dies sind die Haupttypen; andere Konstruktionen kommen als Varianten vor.

Von den typologischen Zusammenhängen, in denen diese Relativsatztypen stehen, ist bisher vor allem einer bekannt: der pränominalen Relativsatz kommt, mit Ausnahme des Chinesischen, nur in SOV-Sprachen vor, jedenfalls aber nur in Sprachen, die gewisse mit der Hauptkonstituentenstellung SOV harmonische Eigenschaften wie Postpositionen oder pränominalen Adjektive haben.⁸ Unter der Voraussetzung, dass das Indogermanische die Hauptkonstituentenstellung SOV hatte, hat man geschlossen, dass es pränominalen Relativsätze gehabt haben müsse.⁹ Aber selbst wenn ausnahmslos gälte, dass wenn eine Sprache einen pränominalen Relativsatz hat, sie auch die Stellung SOV hat, führt nur eine *fallatio consequentis* zu dem Schluss, dass wenn sie die Stellung SOV hat, sie auch einen pränominalen Relativsatz hat. Tatsächlich ist das letztere weit von der Wirklichkeit entfernt. Der pränominalen Relativsatz kommt, ausser im Chinesischen, fast nur in sogenannten rigiden SOV-Sprachen vor. Ebenso häufig ist in SOV-Sprachen der sonst noch häufigere postnominale Relativsatz. Daneben kommen in SOV-Sprachen der zirkumnominalen und der vorangestellte Relativsatz vor.¹⁰

Wir haben also nicht nur keine typologische Basis für den Schluss, dass das Indogermanische pränominalen Relativsätze gehabt habe. Mehr noch: da das Indogermanische mit Sicherheit keine rigide SOV-Sprache war, ist es sogar ziemlich unwahrscheinlich, dass es solche Relativsätze

hatte. Ferner kann man auf typologischer Basis mit einiger Sicherheit ausschliessen, dass der indogermanische Relativsatz primär nachgestellt war. Der Typ, dessen bekanntester Vertreter der in diversen griechischen Dialekten einschliesslich Homer vorkommende *hó*-Relativsatz ist, kommt in SOV-Sprachen nicht vor; und dies ist auch erklärlich, da in einer Sprache, in der das Hauptverb in unmarkierten Falle den Satz beschliesst, ein Nebensatz, der regelmässig nach dem Hauptverb kommt, ausgeschlossen sein sollte. Fraglich ist, ob man für das Indogermanische auf typologischer Basis einen zirkumnominalen Relativsatz ausschliessen kann; die Verhältnisse der einschlägigen Sprachen sind noch zu wenig erforscht.¹¹ Vom historisch-vergleichenden Standpunkt spricht jedenfalls nichts dafür, dass das Indogermanische einen zirkumnominalen Relativsatz hatte. Wenn man all diese Relativsatztypen ausschliesst, kommen für das Indogermanische nur noch ein postnominaler und ein vorangestellter Relativsatz in Betracht.

Der vorangestellte Relativsatz kommt in mehreren Sprachen als sekundäre Konstruktion vor; so etwa in den dravidischen Sprachen, im Hurrischen, Mordwinischen, Chinesischen und nicht zuletzt im Deutschen. In dem meisten, jedoch keineswegs in allen Fällen handelt es sich hier um Relativsätze ohne Bezugsnomen vom Typ "Wer einmal lügt, dem glaubt man nicht." Der vorangestellte Relativsatz kommt in einigen Sprachen als primäre Konstruktion vor. Hier sind in erster Linie Mande-Sprache wie Bambara und Mandinka zu nennen. In der Yuki-Sprache Wappo (Kalifornien) und dem nordaustralischen Mabuiag ist der vorangestellte Relativsatz die primäre Konstruktion, ist jedoch sekundär nachstellbar oder zirkumnominal bzw. postnominal. Im zentralaustralischen Walbiri ist der Relativsatz angeschlossen, entweder voran- oder nachgestellt. In den neuindoarischen Sprachen, etwa dem Hindi, ist der vorangestellte Relativsatz, vor dem nachgestellten und weiteren Varianten, die primäre Konstruktion. Alle hier genannten Sprachen mit Ausnahme des Chinesischen und Deutschen sind SOV-Sprachen. Die letzteren beiden, wo der vorangestellte Relativsatz, wie gesagt, nur eine sekundäre Variante ist, sind SVO-Sprachen.¹² Es ist erklärlich, dass ein primär vorangestellter Nebensatz nur in Sprachen vorkommt, die das Hauptverb nicht primär am Anfang haben.

Der vorangestellte Relativsatz hat im allgemeinen die Struktur:

[... Nukleus ...] [(...) (Anaphorikum) ...]

RS

HS

wobei RS=Relativsatz und HS=Hauptsatz. D.h., der Relativsatz enthält den Nukleus in der ihm syntaktisch zukommenden Position und geht dem Hauptsatz in toto voran. Dieser enthält häufig ein Anaphorikum, das den durch den Relativsatz gebildeten Begriff repräsentiert und meist, jedoch nicht notwendig am Anfang steht. Es kann fehlen, wenn es eine wenig komplexe syntaktische Funktion, also die des Subjekts, seltener die des Objekts, hat. Typische vorangestellte Relativsätze sind die folgenden aus dem Bambara und Walbiri:¹³

- (1) *n ye tyè mìn ye, ò be fìni fère*
 BAM ich PERF Mann REL seh DEM IMPF Stoff:DEF verkauf

“Der Mann, den ich gesehen habe, (der) verkauft den Stoff.”

- (2) *yankiri - ò kutja - lpa ŋapa ŋa-ŋu ŋula ña pantu-ŋu ŋatjulu-ù.*
 WAL Emu-ERG SR-PRT Wasser trink-PRT DEM SBJ.1 erleg-PRT

“Das Emu, das Wasser trank, (das) erlegte ich.”

In dem Bambara-Beispiel sehen wir ein Relativpronomen *mìn*, das dem Nukleus unmittelbar folgt. Es hängt morphologisch mit dem Fragepronomen zusammen und flektiert nach dem Numerus. Es ersetzt jegliche Determination des Nukleus; insbesondere kann dieser nicht definit sein. Der Hauptsatz enthält ein Anaphorikum, das den durch den Relativsatz gebildeten Begriff vertritt und dessen Subjektsstelle im Hauptsatz vertritt. Der Relativsatz selbst ist nicht Subjekt, da er gar keinen nominalen Status hat. Im Walbiri gibt es kein Relativpronomen. Der Relativsatz wird stattdessen durch eine Konjunktion *kutja* untergeordnet, die auch in nicht-relativischen Nebensätzen auftritt. Das anaphorische *ŋula* im Hauptsatz kann fehlen; die Konstruktion ist im übrigen der von (1) parallel.

Der Relativsatz kann auch nachgestellt werden. Im Bambara ist dies eine markierte Variante; im Walbiri ist die Nachstellung so üblich wie die Voranstellung:

- (3) *tyè ye mùrù sàñ, n ye mìn ye.*
 BAM Mann:DEF PERF Messer: DEF kauf ich PERF REL seh.

“Der Mann kaufte das Messer, den ich sah.”

- (4) *ŋatjulu - ù ña yankiri pantu-ŋu kutja-lpa ŋapa ŋa-ŋu.*
 WAL ich-ERG SBJ.1 Emu erleg-PRT SR-PRT Wasser trink-PRT

“Ich erlegte das Emu, dass Wasser trank.”

Sowohl in Bambara als auch im Walbiri wandert der Nukleus bei dieser Konstruktion vom Relativ- in den Hauptsatz. Er bleibt also im Vorder-
satz, wodurch normale anaphorische Verhältnisse gewahrt werden. Das
Relativpronomen des Bambara verbleibt dagegen im Relativsatz und fun-
giert nunmehr anaphorisch.

Charakteristisch für den vorangestellten Relativsatz mit Relativpro-
nomen ist die Häufung mehrerer Relativsätze am Satzanfang, der in
Sprachen mit eingebettetem Relativsatz die Einbettung einer Relativkon-
struktion in eine andere entspricht. (5) ist ein Beispiel:¹⁴

(5) *n ye so min ye, tyè min be ò dyɔ*
BAM: Ich PERF Haus REL seh Mann REL IMPF DEM erricht
n be ò dɔn.
ich IMPF DEM kenn

“Den Mann, der das Haus, das ich sah, baut, (den) kenne ich.”
Hier wird der im ersten Relativsatz gebildete Begriff im zweiten wieder-
aufgenommen, wo ein weiterer Begriff gebildet wird, der schliesslich in
den Hauptsatz eintritt.

Die Beispiele mögen gezeigt haben, dass ein vorangestellter Relativ-
satz etwas völlig anderes als ein pränominaler ist. Ein pränominaler Rela-
tivsatz enthält grundsätzlich weder den Nukleus noch ein Relativpronomen
und meist nicht einmal ein die Leerstelle bezeichnendes und den
Nukleus vertretendes Personalpronomen. Er verhält sich syntaktisch zu
seinem Nukleus wie ein einfaches Attribut. Ein vorangestellter Relativ-
satz dagegen enthält grundsätzlich seinen Nukleus und meist auch ein
Relativpronomen und verhält sich syntaktisch nicht wie ein Attribut, da
er gar nicht eingebettet ist. Es gibt keine unmittelbaren diachronen Be-
ziehungen zwischen diesen beiden Typen.¹⁵

3. DER INDOGERMANISCHE *KWI-/KWO- RELATIVSATZ. Durch das Aus-
schliessungsverfahren in § 2. waren wir zu der Ansicht gelangt, dass das
Indogermanische einen vorangestellten und/oder einen postnominalen
Relativsatz gehabt haben kann.¹⁶ Der vorangestellte Relativsatz des In-
dogermanischen ist der *kwi/kwo- Relativsatz, wie er vor allem aus dem
Anatolischen und Italischen rekonstruierbar ist. Die ältesten dort beleg-
ten Relativsätze tragen deutlich die in § 2. besprochenen Merkmale. (6)
und (7) sind typische Beispiele:

(6) *suwāru kwe GAL^{HLA} akkuskizzi, ta apē-pat ekuzi* (KBo XVII HET 74 IV: 33f=StBoT 12: 35) -

“Die Becher, die er voll zu trinken pflegt, eben die trinkt er.”

(7) *ab arbore abs terra pulli qui nascentur, eos in terram deprimi-*
LAT *to.* (Cat. agr. 51) -

“Die Schösslinge, die von dem Baum aus dem Boden wachsen werden, die stosse man in die Erde.”

Die wesentliche strukturelle Aehnlichkeit von (6) und (7) mit (2) und besonders (1) ist unverkennbar. Analog zu (3) und (4) ist nun auch der indogermanische **kwi-/kwo* - Relativsatz nachstellbar, und zwar im Althethitischen noch sehr selten, im Altlateinischen jedoch ohne weiteres: (8) *natta apūn GEŠTIN-an piēr, LUGAL-us kwin austa.* (KBo III 34 Vs. HET II 5f// KBo II 36 Vs. 13’f) -

“Jenen Wein haben sie nicht gegeben, den du, König, gesehen hast.”¹⁷

(9) *quem dolum doloso contra conservo parem, qui illam hic vidit*
LAT *osculantem* (Pl. Mi. 198f) -

“Welche List kann ich dem listigen Mitsklaven entgegensetzen, der sie hier hat küssen sehen.”

Ebenso wie oben in (3) und (4) verbleibt auch hier der Nukleus im Vordersatz, und das Relativpronomen im Nachsatz fungiert anaphorisch. Auch die charakteristische Häufung von Relativsätzen am Satzanfang findet sich wieder:

(10) ^D*U-as kwedani UD-ti hatuga tethiskit ... TÚGNÍG.LÁMMEŠ kwe*
HET *apēdani UD-ti wassan harkun, ... nu ... apātta dāir.* (Murs. Sprachl. II 23ff) -

“Die Kleider, die ich an dem Tag angezogen hatte, an dem der Wettergott furchtbar donnerte, auch das nahmen sie [für sich in Anspruch].”

(11) *Quei intra eos fineis agrum posedet Genuas aut Viturius, quei*
LAT *eorum posedeat K. Sextil. L. Caicilio Q. Muucio cos., eos ita possidere colereque liceat.* (CIL I² 584, 28f) -

“Jeder Genuese oder Viturier, der innerhalb dieser Grenzen Land besitzt und es schon am 1. August des Konsulatsjahres von L. Cäcilius und Q. Mucius besass, soll es so behalten und bebauen dürfen.”

Mit ihrer Konstruktion ‘erster Relativsatz - zweiter Relativsatz mit

anaphorischer Aufnahme des ersten - Hauptsatz mit anaphorischer Aufnahme des zweiten Relativsatzes' sind (10) und (11) strukturell und funktionell völlig parallel zu (5). An solchen Beispielen wird besonders deutlich, dass der vorangestellte Relativsatz in erster Linie dazu dient, ein Thema zu etablieren, auf das dann im Hauptsatz Bezug genommen wird.¹⁸ Diese primäre Funktion erklärt auch die charakteristische Konstruktion als korrelatives Diptychon: Der Nukleus eines restriktiven Relativsatz ist notwendig semantisch indefinit, das ihn determinierende (Relativ-) Pronomen muss also indefinit sein.¹⁹ So erklärt sich der Gebrauch eines dem Frage- und damit dem Indefinitpronomen nahestehenden Relativpronomens im Bambara und Indogermanischen. Die sonst nicht notwendige Markierung der Indefinitheit eines Nominals löst eine Nukleusbildung und damit die Bildung eines spezifischeren auf der Basis diese Nukleus aus, und sie bereitet gleichzeitig darauf vor, dass dieser Begriff in einem Folgesatz wiederaufgenommen werden wird,²⁰ etabliert also, in Wechselwirkung mit der Anapher selbst, das korrelative Diptychon. Dieses entsteht mithin aus einer Sequenz zweier selbständiger Sätze, in deren erstem ein indefinites Nominalsyntagma eingeführt wird, das im zweiten anaphorisch aufgenommen wird, und wo der erste Satz, im Zusammenhang mit der vorherrschenden Hauptkonstituentenstellung SOV, dem zweiten als Thema untergeordnet wird.

Die Verwendung eines der Form nach indefiniten Relativums ist also semantisch erklärlich in einem primär vorangestellten Relativsatz. Im Laufe der Entwicklung des Relativsatzes wird das Pronomen grammatikalisiert, und seine ursprünglichen semantischen Eigenschaften spielen keine Rolle mehr. Dies muss vorausgesetzt werden für die nachgestellte Konstruktion, in der das Relativum anaphorisch fungiert. Die Zunahme der nachgestellten Konstruktionen lässt sich durch die hethitische Sprachgeschichte und später noch in der lateinischen verfolgen. Im Indogermanischen war die Nachstellung des **kwi-/kwo-* Relativsatzes wahrscheinlich noch nicht möglich.

Es gibt wahrscheinlich keine strengen typologischen Rahmenbedingungen für den vorangestellten Relativsatz über die schon erwähnte SOV-Stellung hinaus. Abgesehen von ihr, sind die oben zum Vergleich herangezogenen Sprachen Bambara und Walbiri gründlich voneinander Verschieden. Bambara hat eine sehr arme Morphologie und strenge Wortstellung,

Walbiri eine sehr reiche Morphologie und freie Wortstellung. Während der indogermanische **kwi-/kwo-* Relativsatz dem des Bambara ähnlicher ist als dem des Walbiri, ist die Gesamtstruktur des Indogermanischen zweifellos der des Walbiri ähnlicher. Diese beiden Sprachen wurden also, wie schon gesagt, aus heuristischen Gründen herangezogen, und nicht um Eigenschaften des Indogermanischen zu deduzieren.

4. ZUM INDOGEMANISCHEN **yo-* RELATIVSATZ. Das Ausschliessungsverfahren in § 2. ergab für das Indogermanische die Möglichkeit eines postnominalen Relativsatzes. Dass eine Sprache mehrere Relativsatztypen nebeneinander haben kann, wurde schon erwähnt und ist völlig üblich. Da alle bisherigen Rekonstruktionen immer auf mindestens zwei voreinzelsprachliche Relativa gekommen sind, nämlich **kwi-/kwo-* und **yo-*, liegt der Gedanke nahe, dass **yo-* seinen Ursprung in einem postnominalen Relativsatz hat. **yo-* dürfte mit dem Personalpronomen **i-* zusammenhängen, also anaphorischen und/oder demonstrativen Ursprungs sein.²¹ Dies ist, typologisch betrachtet, in der Tat der allerhäufigste Fall für Elemente, die einen postnominalen Relativsatz einleiten. Die Genese eines solchen Relativsatzes kann auf zwei Wegen vonstatten gehen, die einander nicht ausschliessen: entweder wird ein durch ein solches Demonstrativum angekoppeltes Attribut zum Relativsatz ausgebaut;²² oder ein durch ein Anaphorikum auf einen Vordersatz bezogener Satz wird zum Relativsatz grammatikalisiert.²³

Während vom typologischen Standpunkt einiges für eine solche Genese des **yo-* Relativsatzes spricht, gibt es vom historisch-vergleichenden Standpunkt zwei Probleme: Erstens, im Vedischen spricht nichts dafür, dass der *ya-* Relativsatz ursprünglich ausschliesslich postnominal war; tatsächlich kommen fast alle denkbaren Stellungen von Anfang an nebeneinander vor. Zweitens, wenn es im Indogermanischen sowohl **kwi-/kwo-* als auch **yo-* Relativsätze gab, ist es schwer erklärlich, warum die Sprachen mit **kwi-/kwo-* Relativsatz kaum Spuren eines relativischen **yo-* und die Sprachen mit **yo-* Relativsatz kaum Spuren eines relativischen **kwi-/kwo-* aufweisen. Man müsste annehmen, dass die ursprüngliche Doppelheit in allen Sprachen zugunsten einer der beiden Konstruktionen aufgegeben wurde und dass diese auf die semant syntaktischen Bereiche, die ursprünglich die andere Konstruktion innegehabt hatte, ausgedehnt wurde. Dies würde mindestens erklären, wieso der al-

tindische und griechische Relativsatz trotz des anaphorischen Ursprungs von *yo- seit Beginn der Ueberlieferung vorangestellt, also wie ein *kwi-/kwo- Relativsatz konstruiert werden kann.²⁴

ANMERKUNGEN

- (1) Hier sind die in diesem Zusammenhang immer wieder angeführten sog. Universalien in Greenberg 1963 zu nennen. Für viele von ihnen räumt der Autor bereits selbst Ausnahmen ein; zu weiteren sind seither Ausnahmen gefunden worden.
- (2) Dies besagen Jakobsons (1941) Prinzipien der einseitigen Fundierung.
- (3) Am klarsten ausgeprägt in den Veröffentlichungen von W. Lehmann, z.B. 1974 und in diesem Band, aber auch in Antinucci 1977.
- (4) S. die Kritik in Watkins 1976 und Lightfoot 1979: 155ff.
- (5) So in den zitierten Veröffentlichungen W. Lehmann.
- (6) Etwas anderes ist es, wenn das in solche Vergleiche einbezogene Indogermanische nicht das mit den Methoden des historischen Vergleichs rekonstruierbare ist, sondern ein Vor-Indogermanisch. Die Plausibilität spricht dafür, dass dies in der Tat eine agglutinierende SOV-Sprache war. Aber da über dieses Stadium wissenschaftlich gesicherte Erkenntnisse kaum möglich sind, sollte es nicht im Zentrum der Bemühungen stehen.
- (7) Ausführliches zur Theorie und Typologie des Relativsatzes in Lehmann 1979 (b).
- (8) S. Greenberg 1963: 90f.
- (9) W. Lehmann in den zitierten Publikationen; Antinucci et al. 1979: 173f.
- (10) Die Verhältnisse in den SOV-Sprachen meiner Stichprobe sind wie folgt: Pränominaler Relativsatz: Türkisch, Mongolisch und andere Turksprachen, Lahu, Tibetisch, Birmanisch und andere tibeto-birmanische Sprachen, Amharisch und andere äthiopische Sprachen, Baskisch, Abchasisch, Japanisch, Koreanisch, Quechua, Ijo. Postnominaler Relativsatz: Djirbal und andere australische Sprachen, Hurrisch, Sumerisch, Akkadisch, Persisch und andere iranische Sprachen, Galla, Ungarisch, Dakota, Yaqui und andere uto-aztekische Sprachen. Zirkumnominale Relativsätze: Navajo, Diegueño, Mojave und andere Yuma-Sprachen. Zu den vorangestellten Relativsätzen s.u.
- (11) Die rekonstruierbaren Relativpronomina wären jedenfalls in einem zirkumnominalen Relativsatz nicht unterzubringen.
- (12) Beide haben wohl nicht zufällig auch SOV-Stellungen sowie weitere, mit SOV harmonische Merkmale.
- (13) Die Beispiele und Angaben über das Bambara stammen aus Bird 1968, diejeni-

gen über das Walbiri aus Hale 1976. In den Morphemübersetzungen werden folgende Abkürzungen verwendet: DEF=definit, DEM=Demonstrativum, ERG=Ergativ, IMPF=imperfektiv, PERF=perfektiv, PRT=Präteritum, REL=Relativum, SBJ = Subjekt, SR=Subordinator, 1=erste Person.

(14) Parallele Beispiele aus dem Mandinka finden sich in Bokamba/Dramé 1978: 25

(15) Es ist nicht möglich, den (indogermanischen) vorangestellten Relativsatz als eine Variante einer pränominalen Konstruktion zu erklären wie es in Raman 1973 und W. Lehmann, in diesem Band, geschieht. Der fundamentale Unterschied zwischen den beiden Typen wird besonders deutlich, wenn sie in einer Sprache nebeneinander vorkommen, wie im Tamil (s. Beythan 1943) oder im Hindi (s. Kachru 1978, 3.3.f und 6.).

(16) Ausführliche Literaturhinweise zu den im folgenden vorgebrachten Hypothesen über den indogermanischen Relativsatz wolle man aus Lehmann 1979 (a) und 1979 (b), Teil VI. entnehmen.

(17) Von E. Neu brieflich mitgeteiltes Beispiel.

(18) “Thema” wird hier im Sinne von Chafes (1976, bes. 50f) *topic* verwendet. Zur themabildenden Funktion des Relativsatzes s. Lehmann 1979 (b), Kap V.5.1.

(19) NB: es geht hier um Determinationseigenschaften des Nukleus; die Determination des durch den Relativsatz gebildeten Begriffs bleibt dadurch unberührt.

(20) Dies dürfte auch J ustus (1978: 112f) meinen. Ihre Termini scheinen jedoch unangemessen: **kwi-/kwo-* markiert keinen Fokus, wenn man den in Chafe 1976, § 2. explizierten und auch sonst verbreiteten Fokusbegriff zugrundelegt; und als ein Indefinitum kann es auch, nach geläufigem Sprachgebrauch, nicht kataphorisch fungieren.

(21) So schon Windisch 1869.

(22) So stellt es Benveniste 1957 dar.

(23) So stellen es sich Windisch und seine Nachfolger vor.

(24) Dieser Gedankengang klärte sich in einem Gespräch mit H. Rix.

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LES RELATIVES NOMINALES INDOEUROPEENNES*

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0. Dans cet article on se propose d'analyser un type de modifieur de tête nominale bien attesté au cours des phases les plus anciennes des dialectes indoeuropéennes: il s'agit des structures qu'on appelle souvent - et à juste titre comme on essaiera de le montrer - 'phrases relatives nominales'. L'analyse théoriquement la plus intéressante de ces constructions est celle qu'en donne E. Benveniste (1966: 208-222) en comparant les relatives indoeuropéennes aux structures apparentées de certaines langues non-européennes, par exemple l'arabe classique. En faisant cela il cherche à expliquer des faits indoeuropéens au moyen d'une généralisation interlinguistique, notamment l'existence d'un modèle typologiquement très répandu de phrase relative. Cependant nous allons montrer que cette analyse est probablement trompeuse, parce qu'elle est fondée sur une faute méthodologique concernant la notion de grammaire générale: il s'agit de l'essai classique du structuralisme traditionnel d'atteindre l'*adéquation explicative* (au sens de Chomsky 1964, par exemple) sans avoir précédemment atteint l'*adéquation descriptive*. Autrement dit, on a trop souvent essayé de découvrir les tendances universelles de la syntaxe en comparant l'aspect superficiel d'un certain ensemble de langues avant d'avoir dégagé la caractérisation précise de la grammaire de chacune de ces langues. En plus la comparaison a été faite sur des langues très différentes l'une de l'autre sans chercher, auparavant, à ramener les différences

entre celles strictement reliées au point de vue typologique ou génétique à un nombre réduit de contrastes minimaux.

1. Voyons maintenant quelles sont les données indoeuropéennes pertinentes et quel type de problèmes elles posent.¹ Dans les poèmes homériques, dans les hymnes védiques et dans l'Avesta, on trouve très souvent une sorte de modifieur d'une tête nominale composé d'un syntagme prédicatif (normalement un syntagme adjectival) précédé par un morphème du thème **yo-*, c'est-à-dire par ce qui allait devenir dans l'histoire des langues en question le pronom relatif:

- (1) a *viśve maruto ye sahāsaḥ* (RV 8, 34, 24)
 tous-les Maruts- qui puissants
 b *azd̥m yō ahurō mazdā* (Y 19,6)
 moi- qui- Ahura- Mazda
 c *Teukros hōs áristos Akhaiōn* (Il. 13, 313)
 Teucre qui-le meilleur-des Achéos

Au point de vue synchronique le fait le plus important concerne le pronom: celui-ci, en effet, demeure toujours au nominatif indépendamment du cas de la tête nominale:

- (2) a *somam bhuvanasya yas patiḥ* (RV 5, 51, 12)
 Soma (acc)- du monde-qui (nom)-le maître
 b *tam daēnam yā hātām vahištā* (Y 44, 10)
 cette-foi (acc) qui (nom)- pour les existants- la meilleure
 c *Zēna hōs tis te theōn áristis* (Il. 23, 43)
 Zeus (acc)-qui (nom)-indef. prt.- des dieux-le plus important.

Ce type de modifieurs se retrouve, plus marginalement, comme Benveniste le souligne, dans d'autres dialectes où le relatif représente le thème **kwi-/kwo-*, notamment en hittite et même en latin archaïque, dont nous allons donner des exemples:

- (3) a *hi Samothraces dii qui Castor et Pollux* (Varr. L.L.5, 58)
 ces S. dieux-qui-Castor-et-Pollux
 b *omnes scient quae facta* (Plaut. Amph. 779)
 tous-connaîtront ceux qui-les-faits

Il serait naturel de traiter ces phrases comme phrases nominales, c'est - à - dire comme prédications sans copule (comme les a décrites Benveniste même 1966: 151-167); cependant des considérations diachroniques vont compliquer le problème: les structures citées deviennent tout à fait im-

possibles dans les phases classiques des mêmes langues ou, pour mieux dire, elles ne sont plus attestées dans le corpus et sont nettement rejetées par le sentiment linguistique d'un grammairien moderne de ces langues. Pour mieux établir les données on propose maintenant des exempla ficta pour le grec attique et le latin classique:

- (4) a **Sōkrátēs hòs phronimōtatos tòn Hellēnōn*
 Socrate qui le-plus-sage des Grecs
 b **omnia quae rara cupio*
 toutes les chose-qui-rares-je désire

La question est donc de préciser quel changement de structure a eu lieu dans le développement diachronique des modifieurs nominaux des langues littéraires anciennes.

2. Pour rendre compte des faits qu'on vient de mentionner et, éventuellement, pour les expliquer deux choix sont possibles a priori: on peut dire, tout simplement, que la formation de la phrase relative n'a pas subi de changements au cours de l'histoire des langues indoeuropéennes et que le contraste entre (1) - (2) - (3) et (4) relève d'une contrainte idiosyncratique, qui n'existait pas dans les phases archaïques mais qui a été introduite plus tard, indépendamment, dans tous les dialectes cités bien que déjà séparés et éloignés l'un de l'autre. Il est clair pourtant qu'une description de ce type n'aurait aucun pouvoir explicatif. De l'autre côté on pourrait imaginer que la contrainte qui exclut (4) a toujours fait partie de la grammaire de toutes les langues indoeuropéennes (et qu'il s'agit peut-être d'une tendance linguistique très générale) et qu'elle ne bloquait pas (1) - (2) - (3) parce que la structure de ces dernières phrases était différente de celle des relatives 'classiques': il faudrait donc A) établir la différence entre la structure des modifieurs d'une tête nominale dans les deux périodes² et B) formuler de façon assez précise la contrainte en question. Benveniste, qui adopte cette deuxième procédure, théoriquement beaucoup plus intéressante, propose une solution très radicale au problème A): il essaie de montrer que (1) - (2) - (3) ne sont pas des phrases relatives nominales car ce ne sont pas du tout des phrases. Nous allons voir, maintenant, les détails de son analyse.

3. On peut commencer par les mots de Benveniste même qui présente l'essence de son propos: 'Ce ne sont pas des "phrases nominales", mais des syntagmes où le pronom, introduisant une détermination nominale, a

nant qu'il soit déterminant d'un terme nominal ou d'une phrase complète.' (221). Si l'on poursuit, alors, la formalisation des hypothèses du savant français on est amené à poser une condition pour exclure l'accord casuel entre la tête et le modifieur:

- (8) Condition: le noeud Mod est une barrière à l'égard de l'accord casuel

Ensuite il faut envisager une règle qui assigne un cas non marqué, le nominatif, au constituant Mod dans tous les environnements:

- (9) R3 Mod $\rightarrow$ + nom

Il va de soi que, comme dans la plupart des langues, la phrase est elle aussi une barrière vis-à-vis de l'accord casuel: cela est nécessaire même dans l'analyse de Benveniste pour empêcher au cas nominatif assigné au constituant Mod et descendu jusqu'au déterminant de descendre à l'intérieur d'une phrase relative et d'y marquer tous les syntagmes nominaux, y compris l'objet direct ou oblique. On formulera, donc, la condition générale.

- (10) Le noeud P est une barrière absolue empêchant l'accord casuel.

Nous avons vu jusqu'à ce moment quelles sont les règles et les conditions qu'il faut imaginer pour décrire correctement, au moins au point de vue observationnel, les données si l'on maintient l'hypothèse de Benveniste sur la fonction *articulaire* de *yo - (kwi-/kwo-) et sur le parallélisme entre (5)a. et b. On va se demander, maintenant, quelle est pour Benveniste la structure des relatives indoeuropéennes dans la période plus récente ou classique. A ce qu'il dit, il considère ces structures semblables à celles correspondantes des langues européennes modernes: en effet en tête de phrase il y a une position (qu'on appellera le noeud Comp) où est déplacé un pronom qui maintient en général (c'est-à-dire sauf les cas d'attraction) le cas correspondant à son rôle grammatical dans la relative. Or quel type de contrainte, enfin, empêche l'effacement de la copule, possible par ailleurs, dans ces structures? Il s'agirait de répondre à la question B) du paragraphe précédent, en expliquant le contraste du latin classique, par exemple, entre (4)b. *supra* et des phrases bien connues comme les suivantes:

- (11) a *Omnia praeclara rara*

toutes les choses-vraiment excellentes-rares

b *Triste lupus stabulis*

chose funeste-le loup-pour les étables

Ici même Benveniste ne formule aucune contrainte et il semble vouloir tout à fait oublier le problème. Il ne fait que remarquer, à propos du latin, qu'une longue tradition de grammaire normative a toujours exclu très rigide-ment la possibilité de phrases relatives nominales comme celles de (4): 'Le pronom relatif *qui* gouvernant une phrase verbale est en latin chose si banale qu'on le prend pour le modèle de toute phrase relative. Par contre *qui* coordonné à une phrase nominale semblera une anomalie.' (1966: 219). La solution implicite dans la pensée de Benveniste ne peut être une contrainte contre l'effacement du verb dans toutes les subordonnées car on trouve des complétives nominales, en grec par exemple, comme on le verra ensuite. En tout cas, le silence sur cette question représente l'élément le plus obscur et le plus faible de toute sa théorie.

4. Dans la section 3. nous avons essayé d'explicitier le plus possible les hypothèses d'Emile Benveniste et d'en tirer toutes les conséquences pour la grammaire des plus anciens dialectes indoeuropéens. Ici nous montrerons que le fragment de grammaire bâti sur l'analyse de Benveniste ne se révèle pas seulement complexe, comme nous l'avons déjà remarqué, mais qu'il paraît franchement inadéquat lorsqu'il s'agit de traiter d'autres simples faits syntaxiques. Nous allons exposer en ordre nos arguments: a) à la différence de (10), la condition (8) ne semble pas être très répandue dans les langues naturelles; plus encore, elle n'est pas normalement respectée dans les langues indoeuropéennes: c'est un fait bien connu qu'en grec on trouve des exemples comme

(12) *Agamémnonos Atréïdao* (Il. 2, 9)

Agamemnon-descendant d'Atreus

† gen + gen

mais jamais comme

(13) **Agamémnonos Atréïdes*

Agamemnon-descendant d'Atreus

† gen † nom

Faire une distinction entre ce type de modifieur nominal et les autres serait une solution ad hoc: d'ailleurs ces faits sont expliqués si l'on élimine la condition (8) et l'on considère la phrase (2)c. comme un exemple de relative nominale, où le noeud P conformément à la condition (10) bloque l'accord de cas;

b) avec la grammaire qui suit des hypothèse de Benveniste on s'attendrait à trouver à côté - ou même à la place - de (14), qui a évidemment une structure de type 'classique', une phrase comme (15)

(14) *Héctori... tòn te stygéousi kai álloi* (Il. 7,112)

Hector - lequel - prt - craignent - même - les autres

+ dat + acc

(15) **Héctori hòs stygéousi (tòn) kai álloi*

Hector- le - craignent-(lui) - même-les autres

+ dat + nom + acc

avec l'effacement de l'objet relativisé ou bien avec un pronom de reprise. Mais cette phrase n'est jamais attestée et est complètement rejetée par notre intuition linguistique du grec (et des autres langues en question). Cela nous semble être l'argument le plus fort contre toute l'analyse proposée;

c) les phrases (5)b et (7) illustrent toutes deux l'emploi de la particule modale *ke*, la forme homérique correspondante à l'attique *án*. Il s'agit de particules qui ont généralement une portée sur le prédicat et indiquent une éventualité ou une possibilité. Or, ces formes se trouvent toujours avec un syntagme verbal, même un participe ou un infinitif, mais jamais avec un syntagme nominal ou adjectival. Comment justifier, alors, sa présence dans (7), si *hós k'epideuēs* n'est pas une phrase et *epideuēs* ne fait pas partie d'un syntagme verbal à copule 'zéro'?

d) La sémantique normale d'une phrase nominale indoeuropéenne, comme l'a affirmé le plus clairement Benveniste, exprime une vérité générale, souvent en contexte formulaire. Si l'on envisage, maintenant, les structures nominales qui font l'objet de notre étude, on remarquera la frappante identité de signification et d'environnement stylistique avec le modèle classique de la phrase nominale.⁴

Il nous semble finalement assez clair que les exemples (1), (2), (3) ne peuvent être analysés comme le propose Benveniste, mais qu'ils contiennent plutôt de véritables phrases nominales. Il reste maintenant à explorer d'autres solutions pour le problème de la disparition de la structure nominale des relatives des phases plus récentes.

5. Les différences hypothétiques de structure entre les relatives anciennes et celles plus modernes seront naturellement en relation avec le type de contrainte qu'on pourra imaginer pour exclure des phrases com-

me (4). Une contrainte à première vue possible vient d'être suggérée à la fin du paragraphe 3.: on va la examiner et la rejeter comme insoutenable. Selon l'hypothèse en question la construction nominale pourrait se retrouver uniquement dans les phrases principales. On devrait conclure par ce principe que dans les phases anciennes les relatives n'étaient pas de véritables subordonnées, mais seulement des principales parenthétiques; ou bien qu'une telle condition sur la distribution des phrases nominales n'était valable que dans les phases récentes. Cependant il y a des arguments contre l'une et l'autre de ces hypothèses: avant tout la phrase relative est le seul type de subordonnée généralement reconnu et attribué à la période commune par toute la tradition de la grammaire comparée. En second lieu c'est encore Benveniste qui nous offre, dans son étude sur la phrase nominale, des exemples grecs de phrase subordonnée sans copule tirés soit d'Homère soit de la prose 'classique' d'Hérodote:

- (16) a *allà píthesthe kai hýmmes epei peíthesthai ámeinon* (Il.1,274)
 mais-obéissez même vous parce que - obéir - mieux
 b *dēloí kai oútos hōs hē mounarkhiē krátiston* (Hdt. 3,82)
 montre-même-lui-que-la- monarchie-le mieux

Il serait préférable, alors, de rechercher si d'autres solutions sont possibles: on partira de la considération du fait que des phrases du type suivant ne semblent pas être exclues dans les phases classiques comme celles de (4):

- (17) a *tís áristos theôn;*
 b *quis optimus deorum?*
 qui-le meilleur-des dieux?

Si cela est correct,⁵ on pourra découvrir quelque indice sur la nature de la contrainte en question en analysant la différence entre les structures interrogatives et relatives. A ce propos, si la configuration syntaxique des deux types semble être la même dans les langues indoeuropéennes développées⁶ (et, souvent, la forme phonologique des pronoms aussi), il y a toujours une différence de nature interprétative: le pronom relatif est un élément anaphorique dans le sens qu'il doit obligatoirement être interprété comme coréférentiel du nom tête, l'interrogatif ne l'est pas et par conséquent il ne doit pas être interprété par référence à un autre élément de la phrase. On pourrait maintenant faire suivre le contraste entre (4) et (17) d'une contrainte formulée de la façon suivante:

Cette contrainte, qui relève d'une notion de semantic heaviness, pourrait bien être 'stylistique': cela suffirait à expliquer l'absence de phrases comme (4) dans les témoignages écrits et littéraires qui nous restent. Voyons alors si la condition proposée nous aide à attribuer une structure plausible aux relatives nominales anciennes. Etant donné que, par hypothèse, la contrainte s'applique de façon homogène à toutes les phases des langues indoeuropéennes anciennes - il s'agit de l'hypothèse la plus simple et la plus intéressante - les phrases (1), (2), (3) pourraient échapper à (18) si leur sujet était non-anaphorique. Est-ce qu'il est possible de soutenir que les relatifs n'étaient pas encore anaphoriques au début de l'histoire des dialectes indoeuropéens et probablement dans la 'langue commune'? Pour les relatifs du thème *yo- cette analyse ne devrait pas poser de problèmes et pourrait même être forcée par certaines données: en fait ce thème renvoie, probablement, par son étymologie, à des pronoms démonstratifs tels que le latin (et le gothique) *is* et il a été employé parfois comme démonstratif même en grec (pour *hos* comme non-anaphorique cfr. Monteil 1963: 44-45).⁷ Les démonstratifs sont des éléments à référence libre que seul le contexte du discours ou des conditions pragmatiques peuvent faire se référer à une autre syntagme nominal dans la séquence. Un pronom de ce type devrait donc être doué de l'autonomie sémantique requise par (18): voilà que les phrases (1), (2) se révèlent interprétables de la même façon que les suivantes, qui contiennent un véritable thème démonstratif, *so/to-:

Zeus-*prt*-le courage-aux hommes-augmente ...: il-en fait-le plus fort-de tous

même-*p_{rt}*- Achille-celui ci-craint-affronter-*qui-_{p_{rt}}*-que toi-
il

beaucoup-plus fort

Dans la deuxième phrase il est vraiment difficile de choisir entre la traduction de *ho-* dont le paradigme était normalement et significativement

employé comme relatif même avec une phrase verbal, dans la langue homérique.⁸ par un relatif ou par un démonstratif, mais au point de vue de la syntaxe ancienne il n'y a pas de distinction: dans les deux cas l'antécédent (*Achille*, comme il est clair seulement par le contexte) serait assigné au pronom par une règle du discours et non pas de la grammaire de phrase, exactement comme dans (19)a et comme pour les démonstratifs dans les langues modernes. Dans ce cadre interprétatif il est remarquable qu'on puisse maintenir que les phrases (1), (2) sont des subordonnées sans marque segmentale précise: ce cas ne serait pas différent de celui des déclaratives sans *dass* de l'allemand moderne, qui ont la structure de propositions principales mais sont employées comme des subordonnées.⁹ Le dernier problème est le statut des éléments relatifs du thème **kwi-/kwo-* et, donc, l'analyse des phrases (3). Mais ce type de pronoms aussi devrait se révéler doué de valeur sémantique autonome dans l'hypothèse formulée par C. Lehmann (ce volume-ci) selon laquelle une structure comme

(20) *Qui patriam defendit, is fortis vocatur*

qui-sa patrie défend-celui ci-courageux-est appelé

recevait originellement l'interprétation

(21) *Quelqu'un défend sa patrie, celui-ci est courageux*

où *qui* est réduit à sa signification primitive d'indéfini. Si la possibilité de cette interprétation a été conservée au moins comme archaïsme jusqu'à date historique le relatif a pu avoir marginalement un sens non-anaphorique et par conséquent n'a pas été soumis à la condition (18).¹⁰

NOTES

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(1) Des données beaucoup plus complètes et un compte-rendu de la littérature se trouvent dans Moreschini (1966: en particulier 39-52).

(2) Afin d'obtenir une analyse vraiment explicative il faut, naturellement, que l'hypothèse d'un changement diachronique dans la structure des modificateurs soit indépendamment justifiée.

(3) Benveniste interprète, alors, (5) a et b comme

a le Pélide-le-beaucoup-plus fort

b il faut-celui-ensevelir-le-meurt

laquelle est en effet la structure des relatives dans certains des langues non-indoeuropéennes qu'il envisage.

(4) Pour le même argument, attentivement formulé pour le védique, cfr. Moreschini (1966: 46).

(5) Ces jugements sont fondés sur la compétence 'intuitive' ou 'normative' des langues classiques, mais je crois qu'on pourrait même en retrouver des exemples.

(6) Par exemple, les relatifs et les interrogatifs obéissent aux mêmes contraintes sur les extractions (cfr. Ross: 1967).

(7) La voie pour une solution pareille semble être suggérée, implicitement, par Mme Moreschini à la fin de son travail (1966: 52).

(8) Le même double emploi de ce thème (démonstratif-article et relatif) en germanique aussi suggère que l'isoglosse soit très ancienne.

(9) cfr. Thiersch (1978: 133-143).

(10) Notre conclusion générale pourrait être renforcée par l'observation, qui m'a été confirmée par M. Roman Jakobson, que dans une langue moderne qui emploie la phrase nominale, comme le russe, la relativisation d'un sujet de prédicat sans copule est très difficile et beaucoup plus marginale que l'interrogation (directe ou indirecte) sur le même élément.

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SYMBOLES CATEGORIELS

P	=	Phrase
SN	=	Syntagme nominal
SA	=	Syntagme adjectival
SPréd	=	Syntagme prédicatif
Mod	=	Modifieur
Dét	=	Déterminant
Comp	=	Complémenteur
prt	=	particule
indéf	=	indéfini

TYPOLOGICAL SYMMETRIES AND ASYMMETRIES IN HITTITE AND IE COMPLEMENTATION*

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INTRODUCTION

The question has been raised as to what entities are to be compared in historical and comparative syntax (Watkins 1976:312). If we believe that research into syntactic universals has any validity, the answer must be: variant typological and language specific expressions of universal categories and functions. Universals of language, by definition, are not subject to change, only variant expressions of them are. The first task of the comparative-historical syntactician then is to identify particular language expressions with universal categories and functions.

But one important consideration makes the Indo-Europeanist hesitate: is it possible that the oldest data could represent a stage of language different in kind from human language today, a stage in which some of our universal categories and functions were not yet developed? In reality the protolanguage of ca. 3000 B.C. or even 5000 B.C. is not early enough in the history of our species to have been different in kind from human language today. It is unlikely then that texts of only 4000 years ago would still reflect a stage of language in any sense more primitive, or language even reminiscent of a stage without a mechanism for constructing cohesive texts (cf. Justus 1979). Whether or not text cohesion was based on the more familiar mechanisms of hypotaxis in modern IE languages,

the descriptive term 'parataxis' cannot realistically suggest that sequences of clauses were loosely connected to the degree that they reflect an earlier lack of cohesion (Justus 1976; 1978). It follows therefore that we can expect already in the protolanguage systematic devices for forming structures larger than the simple sentence. While grammatical theory has traditionally assumed, and more recently explicitly stated, certain categories, processes, and relationships which are fundamental to syntax in language (Chomsky 1965 *passim*; Lehmann 1978), studies in word order typology have begun to uncover sets of variants that are harmonic with each other for reasons we can only explain as the result of the underlying structure of language (Greenberg 1966; Vennemann 1974; Lehmann 1973 *passim*). My first assumption thus is that reconstructed structures in pre-IE or PIE must be constrained by universal and typological facts known to apply to actual human languages.¹

Although the linguist concerned with the theory of grammar is perhaps most interested in universal generalization, the Indo-Europeanist has long focused on the particulars of IE languages, often as if they in fact expressed universal principles of language, but without consideration of the degree to which such universals might be better understood from a less biased IE point of view. With new syntactic understandings and methods, etymology in PIE should also reflect new goals (cf. Lehmann 1977). Now one might expect from syntactic reconstruction that it identify at least three aspects of a construction: (a) the universal, which is not particular to IE; (b) the typologically widespread, therefore also not confined to IE; and (c) the part which is indeed so particular that it occurs in the IE languages because they share an earlier common period of development.

To discover which facts belong to the IE particular, then, one must have a clear understanding of how the particulars relate to universal and typological aspects. As working hypothesis, the reconstruction of PIE as an OV language (Lehmann 1974 *passim*) provides a framework to account for both systematic and particular aspects of language, and how they interrelate. Revisions will, of course, be necessary as generalizations and predictions based on our still imperfect knowledge of language lead to new insights that prove the old ones right or wrong. Chronological stages of PIE have to be distinguished, too, to account for the difference

between the earlier consistent OV stage and the attested inconsistencies in all of the daughter languages. But the method need not be abandoned just because the first prediction fails to prove the totality of the hypothesis.

To identify particular IE characteristics of complementation, I first sketch pertinent universal relations as systematic, then examine typical OV and VO expressions of the universal structure, complementation. These opposite types in fact express a universal category whose variant functions depend on particular noun-verb relations and predicate type, each expressed by a syntactic marker, the complementizer. But surface differences in constituent order correlate with OV or VO word order harmonics. One particular attested set of IE complement structures, that represented by constructions with Hittite *šak(k)* - *šek(k)* - ‘know’ and *ištamaš* - ‘hear’ exhibits systematic asymmetries which clarify the role of IE accusative infinitive complementation. As an heuristic device, I use reconstruction to show how, as complementizer, the IE accusative infinitive type is neither universal nor typologically predictable, but the historical reflex of a particular mechanism in an OV parent language. The point is to examine implications that the typological hypothesis has for identifying IE particulars and their relation to universal and typological phenomena. If the particular identified here as IE turns out to be typologically predictable instead, the argument will not have been in vain, but will have led to a discovery beyond what this study is prepared to claim.²

THE UNIVERSAL SYSTEM

The term complement clause covers as unitary category in language such expressions of substantive function as ‘that’ clauses, gerund, or infinitival constructions (cf. Bresnan 1970):

- (1a) I heard *THAT the king had lost his speech*.
- (1b) *THAT the king had lost his speech* was obvious.
- (1c) It was obvious *THAT the king had lost his speech*.
- (2a) I heard *John/him playing the piano*.
- (2b) *John’s/his playing the piano* was a treat for everyone.
- (3) I know *John/him to be a good piano player*.

Of these six, (1a, 2a, 3) express verb phrase complementation, the rest noun phrase complementation (Rosenbaum 1967), with (1c) constitu-

ting an extraposed variant of the basic constituent order Subject, Verb, Object (SVO: Kuno 1974: 128ff.). Complementizers as diverse as *that* (1a), accusative plus *-ing* (2a), accusative plus infinitive (3), or genitive plus *-ing* (2b) express a single syntactic category, complementation. Various functions of the category determine the choice of complementizer.

Fillmore (1968:22ff.) pointed to uses of syntactic subject for different grammatical functions of the verb to achieve variant pragmatic effects (cf. Agent, Object, Instrument: '*John* broke the window with a hammer', '*The window* broke', and '*The hammer* broke the vase'). In incorporating these facts into a descriptive model for PIE, Lehmann (1974:11f.) suggests eight semantic relations, using terms like Target (= Object), Receptor (=Dative), Manner, Source, Time, Place. Such a view of noun-verb relations implies that, as nominal counterparts, traditional adverb clauses, noun and verb phrase complementation constitute instead parallel noun-verb functions. Complementation then includes e.g. Target, Manner, and Time/Place³ relations:

(4a) Target complementation: 'I know *THAT the king lost his speech.*'

(4b) Manner complementation: 'I know *HOW the king lost his speech.*'

(4c) Time/Place: 'I know *WHERE/WHEN the king lost his speech.*'³

Complementizer differences expressed by 'that', 'how', and 'where/when' encode the differences between various semantic noun-verb case relations.

Universally in syntax complementation contrasts with relativization and indirect expressions as recursive structures:

(5a) Relative: 'I know *the king WHO lost his speech.*'

(5b) Indirect: 'I know *WHAT the king lost.*'

In (5a) the dependent clause as attribute to 'the king' contrasts with the Target object 'that' clause (4a), which itself functions as object. Both contrast with indirect (5b) '*what* the king lost'.

But complementizer choice depends as well on factors which result in subcategorization in the verb, semantic factors associated with the verb independent of noun-verb relations. Kiparskys (1970) and Karttunen (1971) among others focused on factors of verbal qualification such as assertion and presupposition. In English, for example, 'hear' does not imply that its Target complement states a fact. In 'I heard *that* the king lost his speech', the 'hearing' does not imply that in fact the king

did lose his speech. To imply that, one must say ‘I heard *about* the king’s losing his speech’ or ‘I *know that* the king lost his speech’. While ‘know’ belongs to a class which implies that the complement states a fact, ‘hear’ does not. The subform of Target relation which ‘that’ expresses is further determined in English by verb class. The equivalent subtype with ‘hear’ requires ‘about’ with genitive plus *-ing*.

In language generally paradigmatic sets of functions identified with single categories often develop uniquely interwoven forms to express subcategorical relations in individual language systems. For example, neither Spanish infinitive nor English ‘that’ clause translates every Latin infinitive or subjunctive best (cf. Lakoff 1968:218-235). Even more obvious, English gerunds inadequately render every Old Irish verbal noun, as that language uses, instead of the usual range of IE complementizer types (see below), primarily verbal nouns. The problem then is first one of identifying language specific paradigmatic differences with the universal category and function, then marking crosslinguistic associations of form on the basis of common function. This approach contrasts with traditional IE approaches to etymology which make associations based first on common form.

TYPOLOGICALLY PREDICTABLE VARIATION

Identification of a universal category complementation and the marker of its syntactic function, the complementizer, allows us to circumscribe limits for change and comparison. But metalinguistic tools available to the comparativist for understanding specific language systems are not therewith exhausted. Typological studies provide a framework for measuring only apparently random differences (Greenberg 1966 *passim*; Lehmann 1973; 1974; 1978 *passim*). For example, typologically opposite arrangements of complement clause and complementizer, e.g. ‘I *sun shining is* THAT know’ (OV) versus ‘Know I THAT is shining the sun’ (VO), tend to correlate with OV and VO ordering among the other constituents in many languages. Between these two extremes lie SVO languages: ‘I know THAT the sun is shining’ (Kuno 1974:123ff.; 127).

One might recall here some other harmonics of the OV and VO ends of the word order continuum. Of binary possibilities for constituent order, postpositions correlate with verb final order, prepositions with

verb initial of nonfinal order. In the comparison of inequality the verb final adjective follows the standard, while the verb initial adjective precedes (cf. Andersen in this volume and Justus 1979 ms.), and verbal qualifiers stand on the side of the verb opposite the noun it governs.

Lehmann (1973) showed that these opposite orders are perceptually optimal arrangements in language, which is constrained by its utterance in time and space to linear sequencing. Kuno (1974) has more specifically pointed to perceptual difficulties inherent in center embedding and juxtaposition of conjunctions as motivation for typologically opposite clausal order and complementizer placement. Slobin (1977) has further enunciated principles of regularity and efficiency, including consistency in word order type, as factors in determining the ease with which children master linguistic constructions. The present study emphasizes implications of Kuno's work on subordinate order for the identification of the typologically predictable as opposed to the properly IE character of solutions to perceptually difficult OV object complementation. IE raising, I suggest, finds its motivation in the perceptually difficult center-embedded Target complement, and thus argues for an OV parent language. But this raising plus nonfinite verbal is peculiarly IE.

Cursorry examination of languages both modern and ancient, IE and non-IE, quickly illustrates systematic variation in placement of complementizers consistent with other harmonic orders in language. At one end of the ordering continuum, verb final order correlates with complement clauses preceding the main verb and complementizers postposed after the complement clause, before the final verb. At the other end, verb initial order correlates with complement clauses following the main verb and complementizers preposed before the complement clause. Compare here the OV Japanese complementizer paradigm with main verbs 'hear' and 'know' (Kuno 1978:121-6):

- (6a) Boku wa Taroo ga kekkonsita *koto* o sitte iru
 I ptc Taroo ptc married fact-that knowing am
 'I know that Taroo got married.'
- (6b) Boku wa Taroo ga kekkonsita *to* kiita
 I ptc Taroo ptc married that heard
 'I heard that Taroo got married'
- (6c) Boku wa Taroo ga piano o hiku *no* o kiita

I ptc Taroo ptc. piano ptc. play that heard
 ‘I heard Taroo play the piano.’

(6d) Boku wa Taroo ga kekkonsita koto o kiita
 ‘I heard about the fact that Taroo got married’.

The basic main clause order is SOV, where O is the complement clause marked by the variant postposed complementizers *koto*, *to*, and *no*. Particles *wa* and *ga* express subject and topic relations, while *o* is an object marker. In (6a) main clause order SOV is interrupted by replacement of O with Target complement *Taroo ga kekkonsita* ‘Taroo (got) married’ plus complementizer *koto* ‘fact-that’. Compare simple noun object in (6c): *Taroo ga* (S) *piano o* (O) *hiku* (V).

Kuno (1973; 1974; 1976; 1978) has described the language specific system of OV Japanese complement structure in terms of its universal, typological, and language specific character. As in other languages, Japanese complementation expresses noun-verb relations and subcategory in the verb by use of different complementizers. Kuno (1973:216ff.) argues that differences are based on semantic factors such as presupposition of fact. Complementizer *koto* with main verb ‘know’ signals a factive object complement, while *to* and *no* with ‘hear’ are nonfactive. With ‘hear’ *koto* (6d), however, renders the presupposition factive.⁴ Consistent with OV ordering, complement clauses regularly precede the main verb, and complementizers *koto*, *to*, and *no* all follow the complement clause.

Kuno (1976) further shows how Japanese raising processes differ from SVO English. Instead of subject raising to subject position, Japanese has verb raising which results in compound verbs (1976:21):⁵

(7) koori ga toki-dasu
 ice ptc melt-begin ‘Ice begins to melt.’

Raising to object position has certain similarities, but also marked differences (Kuno 1976:24ff.), compared with English:

(8) Yamada wa Tanaka o baka da to omotte ita
 Yamada ptc Tanaka ptc fool be ptc thought
 ‘Yamada thought Tanaka to be a fool.’

Here the subject of the complement has object particle *o* instead of *ga*, but postposed *to* with *da* is not to be confused with English nonfinite verbals, among other differences (Kuno 1976:40-3).

Contrasting with OV order are examples from Ancient Egyptian and the more recent Easter Island. We know from records over a long period of time that Egyptian was VO (Gardiner 1957:34ff.; 65f.). Typical uses with *rh* 'know' illustrate what one expects in a VO language (Gardiner 1957:140ff.):

- (9a) *rh* · *kwī* *ntt* *ḥtp* · *f* *ḥr* · *s*
 know-perf-I ptc at-rest- he face (of) it be pleased about it'
 Verb S Target Complement
- (9b) *rh* · *n* · *f* *ḥrp* · *i* *n* · *f* *st*
 known-to-him undertake-I for-him it administer it for him.'
 V S Target Complement

In VSO Egyptian (9a-b) illustrate the typologically predictable order VSO where O is (complementizer plus) Target complement. Concerning the (optional) use of complementizer *ntt* cf. Gardiner 1957:141.

Chapin (1978) has evaluated the typological character of Easter Island, a VSO Polynesian language, in the context of language universals. He includes subordinate examples (1978:155, 27; 162, 55):

- (9c) *ka ui koe ki te kape hoki ekō haga ia*
 imper. see you dat. the Captain whether not want he
 mo *avai o matou ki a ia hai moni*
 inf. give nom. we dat pro. he some money
 'Ask the Captain *whether* he doesn't want us *to* give him money.'
- (9d) *he hakarogo au he tu'u atu te vānaga ho'ou mai Papeete*
 past hear I past arrive away the talk new from Papeete
 mo oho etahi miro ki Rapanui
 inf. go one boat to Rapanui 'I heard the news had come from
 Papeete *that* a boat was going to Easter Island.'

The meaningful elements in (9c) are Qualifier Verb Subject Dative and Object (QVSDO) in the initial main clause where verbal qualifying particle *ka* (imperative) precedes the main verb *ui* 'see', and dative marker *ki* precedes the dative noun *te Kape* 'the Captain'. The indefinite complement clause, object of *ui* 'see' is preceded by complementizer *hoki* 'whether' and verbal qualifying *ekō* 'not', while the further subordinate complement as Target to *haga* 'want' has complementizer *mo* preceding it. All clauses are VSO in order and complementizer precede. In (9d) the

complement clause as Target after the main verb *hakarogo* ‘hear’, however, like Egyptian (9b), has omitted the complementizer which one expects to precede the complement clause: cf. English ‘I heard (that) the boat came.’⁶

THE IE COMPLEMENTIZER

While certain facts belong to the universal character of language, and others are with more than chance probability typologically predictable, a certain residue remains as property of particular languages or language families. From this residue the Indo-Europeanist identifies the particulars of IE. Since IE asymmetries in complementizer placement and morpheme type obscure universal and typological generalizations, detailed analysis of these asymmetries is pivotal to an understanding of such inconsistent language types and language change. Older IE scholars understood this, we know, from the many studies of pronoun, conjunctions, and the infinitive, for example. What they failed to do was to clearly set diverse formal morpheme types in proper systematic relation to one another, to universal, and to typological facts of language. The analysis of PIE as evolutionarily different in kind undoubtedly constituted a deterrent to such investigation of earlier morphemic expression.

Complementizers in the older IE languages, not totally different in kind from modern IE patterns, vary both in order of placement and in morpheme type. The IE complementizer has three basic positions. It can stand initial before the complement clause:

(10a) Ich weiss, **DASS** der König sein Sprachvermögen verloren hat.

‘I know **THAT** the king has lost his power of speech.’

(10b) iam ego ex hoc, ut *factum est*, scibo. Plaut. Men. V ii 57

‘I will know from this **THAT** it is done.’

(10c) **HO TI** mèn hymeîs..pepónthate hypò tôn emôn katēgórōn ouk oída

‘I do not know **WHAT** you have learned from my accusers.’ Pl.Ap.17a

It can be postposed as a suffix (cf. Latin, Greek, and Old Irish):

(11a) **ITA** exdeicendum censuere nequis eorum *Bacanal habuisse* velet.

‘**THUS** they determined *it SHOULD BE decreed*, (that) no one of them should want **TO** hold a *Baccanal*.’ S.C. Bacch.

(11b) oída d’eni stadiēi dēioi mélpēsthai Arēi Il, H 241

‘And I know how **TO** celebrate with *Ares* in single combat.’

(11c) nī foilsitis dEICSIN (< do-écci) a gnúsa. (Wb. 15^a20)

not bear-fut.-3pl. seeing (<'see') his face-gen.

'They will not be able to look at his face.' (Strachan 1949:119)

Or, it might be somewhere internal in the complement clause:

(12a) 'I heard *John playing the piano*.' 'I want *him to play*.'

(12b) *haice UTEI in conventionid exdeicatis...senatuosque sententiam*

UTEI scientes esetis eorum sententia ita fuit.

S.C. Bacch.

'It was their opinion **THUS THAT** you should know the opinion of the senators, and **THAT** you should publish this in a meeting.'

cf. also the position of Hittite *maḥḥan* and *kuit* below (17-19).

In addition, endophoric deictics form redundant correlative markers in both dependent and main clause; cf. Latin, Sanskrit, Old Irish:

(13a) *nescio etiam id quod scio.* Plaut. Bacch, IV vi 21

I do not know even *that which* I know cf., too, *ita* 'thus' (11a, 12b).

(13b) *kás tād veda yád ádbhutam.* (Delbrück 1888:569 RV1, 170,1)

'Who knows *that which* is wonderful.'

(13c) *aní as berinn cosse is ed as baér beus* (ML 91^b10)

'*That* I was saying till now, *it* I will still say.' (Strachan 1949:116

nr. 159); cf. also *kuit*-at (21) below.

Variant morpheme types form complementizer mechanisms which one might classify variously as (a) particle (6, 9 above),⁷ (b) verbal suffix (e.g. 11, 14, 24, and 28), (c) endophoric pronominal (13, 16), and (d) frozen conjunction (10, 12b, and 17-19), with apparent etymological relations to endophoric pronominals. One further class can best be described as a process: the accusative infinitive construction or the 'raising' of the complement subject to object position in the main clause ('I know *him to be* a good player'; cf. also 28 below).

In older IE, order between main and complement clause is often OV in type, but IE endophoric pronouns as complementizers and the accusative infinitive construction in particular are not easily categorized. Both represent widespread asymmetric particulars which coexist beside more consistent forms of the complementizer such as conjunction (VO when initial: e.g. 10) and verbal suffixes (OV in placement at the end of the complement clause). The question then is, where did endophorics and the accusative infinitive type come from? They are not universal, as all languages do not use them.⁸ Are they typologically predictable for all inconsistent types, or are they part of the language history of IE alone?

I suggest that the accusative infinitive type, or raising of complement subject to object position in the main clause, has its origin as an OV mechanism for grammatically isolating perceptually difficult center embedded clauses. The use of a nonfinite verbal with raising, however, is peculiarly IE.

HITTITE COMPLEMENTIZER VARIANTS

With the language specific paradigm of complementizers in Hittite, the oldest attested IE language, we come to the original data in this study. As in other older IE languages, Hittite complementizers take the form of (a) suffix on the complement verb, (b) frozen form used as conjunction, (c) endophoric pronominal, or (d) raising plus dependent predicate. Old Hittite examples with *šak(k)* -/ *šek(k)* - ‘know’ may also have the entire Target complement preposed to the main clause with no complementizer word, while the later language uses either anaphoric precomplementizer *-at* (16,25), or frozen *kuit* (17,18). Complementizers for Manner and Time/Place complementation are *maḥḥan* and *kuwapi*, while correlative *kuit...-at* marks indirect Target. Examples with *ištamaš* ‘hear’, in addition, have as Target complementizers the verbal noun (24) and IE type raising (27). Variant forms are thus not randomly distributed. Complementizer choice depends on noun-verb relations governing the complement and semantic factors differentiating ‘know’ and ‘hear’. The Hittite system, consistent with universal facts of complementation, exemplifies in this IE asymmetries.

The Hittite system of verbal suffixes includes infinitives in *-anna* and *-uwanzi*, verbal nouns in *-atar* and *-uwar*, a supine in *-uwan*, and the participle in *-ant* (Friedrich 1960:112f.; 142-5; 164). Ose’s 1944 study of Hittite verb forms in *-uwanzi* showed that they, like forms in *-anna*, constitute infinitives with verbal direction. Both *-anna* and *-uwanzi*, like their finite counterparts, take accusative objects, but as nonfinite forms, they depend on a following predicate:

- (14a) nu EN SISKUR.SISKUR warpuwanzi paizzi nu= za warapzi
 ptc. lord offerings wash-inf. go-3sg. ptc. - ptc. wash-3sg.
 maḥḥan=ma=za=kan warpuwanzi ašnuanzi: KUB XXIX 8 II 12ff
 when-ptc.- ptc. ptc. wash-inf. finish-3pl. (Ose 1944:24).
 ‘The priest goes to wash, washes, and when they finish washing, ...’
 (14b) uruḥattusaṇ=za= kan zammurauwanzi kuiški tiškizzi

H. -acc. - ptc.-ptc. offend - inf. someone-nom. set (to) -3sg.
 ‘(If) someone begins to offend the Hatti land, ...’⁵: KUB XIII 4 III 27
 (Ose 1944:18).

In (14a), with order Subject, Infinitive, Verb, the complement expressed by infinitive *warpuwanzi* precedes the Verb. While ‘wash’ (14a) is intransitive, ‘offend’ (14b) takes accusative object *Ḫattušan*. Somewhat variant, too, is the infinitive complement preposed to Subject and Verb *kuiški tiškizzi* ‘someone begins’. Order OSV is usual, however, if the subject is *kuiški* (Raman 1973:41ff.).

Kammenhuber’s (1954 - 56) important series on the Hittite infinitive showed that the choice between *-uwanzi* and *-anna* depends on subcategory in the complement verb (e.g. ablauting verbs take *-anna*: Friedrich 1960:142f.), not the main verb, as Ose had first hypothesized. Hittite verbal nouns in *-uwar* differ syntactically from the infinitives in ways comparable to English genitive plus *-ing* (2b). While *-anna* and *-uwanzi* take noun objects in the accusative, verbal nouns take a formal genitive as object (cf. Old Irish where the verbal noun is the usual complementizer mechanism: Lehmann and Lehmann 1975: 3ff.; 13f.). Such verbal suffixes, as postposed complementizers, are OV in type.

Not OV in type are frozen conjunctions *takku* ‘if’, *kuwapi* ‘where/when’, or *man* and *mahhan* with temporal, conditional, and comparing functions. Sternemann’s study separating chronological uses of the semantics also deals with syntactic order (1968:377ff.). Although both preposed and postposed clauses occur with these conjunctions, the unmarked order is subordinate plus main clause (405ff.), the conjunction itself may be initial, internal, or final in the subordinate clause (380-93), and a specific meaning can be assigned to conjunctions often only in context (378).

Asyndetic preposing, as Friedrich (1960:157f.) surmises, is chronologically early, not grammatically predictable, however:

(15) kinun=wa= z nuwa sal.mešŠU.GI [uš (punuškiz)] zı UL šaggahhi
 now- ptc. ptc. still old-women [acc. asks] - 3sg. not-know-1sg.

‘I do not (want to) know that she is still consulting the old women.’⁹

The order here, complement clause-main clause, is harmonic with OV arrangement, but there is no complementizer word. In declarative contexts Later Hittite examples attest some form of complementizer. With

‘know’ complementizer choice depends first on noun-verb relations. Complements in Target relation, where the context is declarative and not Old Hittite, use either anaphoric *-at* (16) or frozen conjunction *kuit* (17, 18), while Manner complements use *maḥḥan* noninitially (19):

- (16) namma= wa GAM BAL-an NINDA KAŠ UZU=ya KU-kanzi
further-ptc prev. offering-acc. bread beer meat-and eat-3pl.
n= *at* karu šek [(kueni)](HTR 108 Vs. II 5; Mat. heth. Thes 91-2)
ptc. - it early know-1pl.

‘We have known for a long time that they (the palace officials) eat the bread, meat, and beer offering (meant for the gods).’

- (17) [kinun? or GIM?] =ya IDI *kuit*=za KUR uruMizri
[now? as?] -ptc. know-1sg. that-ptc. land Egypt
KUR uruHatti=ya 1^{EN} KUR^{TIM} kišari (KUB XXI 38 Rs. 13f.; Mat.
heth. Thes. 74)

land Hatti- and one land become-3s.

‘Now too I am aware that the land of Egypt and the land of Hatti are becoming one land.’

- (18) [I] ŠTU É.GAL^{LIM} =ya= war= at= kan *kuit* para udaš
from palace ptc. ptc. it-ptc. that prev. brought-3sg.
nu= war= aš= mu= kan UL ŠĀta (StBoT 4, 34 II 11f.;
Mat. heth. Thes 75)

ptc. ptc. it¹⁰ me ptc not heart¹⁰

‘I did not know that (anybody) took it out of the palace.’

- (19) [h] antezzius= ma= at LUGAL^{meš} *maḥḥan* arḥa pittalair
first - acc. - pl. - ptc.- it kings how prev. neglected - 3pl.
n= at dUTU uruTULna GAŠAN= JA [š] akti (KUB XXI
27 I 16-8; Mat. heth. Thes 80)

ptc. it Sungoddess Arinna lady- my know-2sg.

‘But you, Sungoddess of Arinna, my lady, know how earlier kings neglected it (the sanctuary of your son).’

In (16) anaphoric *-at* refers to the entire preceding complement whose relation is that of Target to ‘know’. Elsewhere (Justus 1979a) I have suggested that such use of a simple anaphoric enclitic pronoun be termed precomplementizer to indicate its innovating and asymmetric character as OV complementizer. It is, on the one hand, harmonic with other OV arrangements in that the complement precedes the Verb, and *-at* follows

the complement. But *-at* stands in a separate clause and belongs to the asymmetric set of endophorics exemplified by IE correlative types (cf. 13 above).

While frozen in form, *kuit* (17, 18) exhibits clear morphological and syntactic similarities with productive cataphoric uses (Justus 1978):
 (20) *memiyan*= ma *kuin*= pát daiškit n= an *UL* šaggahḫi
 matter-acc. ptc. what-acc.ptc. taking-3sg.ptc. it not know-1sg.

‘But I do not know what was concerning him.’ KUB XXIII 91, 16;
 Mat. heth. Thes 64.

(21) nu kaš= *kuit* memai n= at zik šakti
 ptc. this-one- nom-what speak-3sg. ptc. it you-nom. know-2sg.

‘You will know what this one says.’ KUB XXXV 148 Rs. III 12;
 Mat. heth. Thes 70.

In (20) cataphoric ‘relative’ *memiyan* ... *kuin* ‘what matter’ stands as ‘antecedent’ to anaphoric enclitic *-an* (both common gender). Parallel to the relative construction and formally similar is the indirect correlative *kuit* ... *-at* ‘what ... it’ (21). Morphologically related is temporal/spatial *kuwapi*:³

(22) *kuwapi*= wa paiši ammuk=ma= wa= tta le šaggahḫi
 where- ptc. go-2sg. I- ptc.- ptc.- you let-not know-1sg.

‘Let me not know where you are going.’ (Dupp II 44f.; Mat. heth. Thes 77).

(23) *nepišan* = mu=kan *kuwapi* daganzipann= a š[e] r weter
 heaven-acc. - me- ptc. when earth-acc. and upon built - 3pl.
 nu *UL* *kuitki* šaggahḫun (KUB XXXIII 106 III 40-1 Mat.
 heth. Thes 78)

ptc. not anything knew-1sg.

‘I knew nothing (about it) when they built heaven and earth on me’

While forms *kuit* and *kuwapi* have clear cataphoric relations, like *maḥḥan* they are frozen forms in clauses which can also follow. They contrast in type with both anaphoric *-at* and suffixed verbals.

Hittite ‘know’ takes no verbal suffix, be it infinitive or verbal noun, as complementizer. But nonfactive *ištamaš-* ‘hear’ includes among its complementizers for Target relation verbal noun-*uwar*:

(24) LÚmeš KUR uruMizra= ma maḥḥan ŠA KUR uruAmka
 men land Egypt- ptc. when of land Amka

GUL-ah_huwar ištamaššanzi KBo V 6 III 5f.; CTH 40 IV
destruction hear - 3pl.

‘But when the men of Egypt hear that A. has been defeated, ...’
Hittite verbal noun *GUL-ah_huwar*, as that of Old Irish, has a genitive object. Precomplementizer *-at*, with ‘hear’ as with ‘know’, expresses a factive presupposition in contrast to nonfactive *-uwar*:

- (25) ŠA dIŠTAR para ḫandandatar memaḫḫi n= at
of Istar prev. favor-n/a-n. speak-1sg. ptc.- it
DUMU.NAM.ULUL_Uaš ištamašdu Hatt 2 § 1; KUB I 1 I 5f.
son-mankind-nom. hear-3sg.-imper.

‘Let the son of mankind hear about my speaking the favor of Istar’.
Here enclitic *-at*, as in (16), refers to a preceding Target complement. Like the verbal noun, indirect *kuit ... -at* contrasts with *-at*:

- (26) nu= tta kuit memiškimi n= at i[š(tamaški)]
ptc. - you what speaking-1sg. ptc. - it hear-2sg.-imper.

‘Hear what I am saying to you ...’ Gurney AAA 16; B I 13-4.

Compare (26) with *kuit ... -at* in (21) and IE correlatives (13).¹¹

Most interesting, though, is the raising of the subject of the Target clause to object position in the main clause:

- (27) maḫḫan=ma KUR.KURmeš lúKÚR mArnuwandan ŠEŠ=JA
when- ptc. lands enemy A. - acc. brother-my
irman ištamaššir AM 14f.; KBo III 4 I 6f.
sick-acc. heard-3pl.

‘But when the enemy lands heard that my brother Arnuwanda was sick, ...’

The complement in (27), *Arnuwandan ŠEŠ=JA irman* ‘my brother (is) sick’, is center embedded between Subject, *KUR.KURmeš lúKÚR* ‘enemy lands’, and the Verb, *ištamaššir* ‘they heard’. The complement subject, *Arnuwandan*, with which *irman* agrees, is raised to object of *ištamaššir*.

Friedrich (1960:164) has given (27) as an example, beside *ammuk=war=an akkantan IQBI* (lit. ‘me-ptc. -him dead - part. - acc. he said’) ‘he told me (that) he was dead’, of the use of the participle with verbs of saying and feeling to express the equivalent of ‘that’ clauses. One expects as participle in (27) **irmantan* ‘him sick’ to parallel *akkantan* ‘him dead’ for an accusative participle construction, not simply *irman*.¹² Unam-

biguous, however, is the raising of an otherwise nominative *Arnuwandaš* (subject of the complement) to object of *ištamaššir*.

Parallels in IE are well known with accusative plus infinitive:
(28) *hoi gār akoúontes hēgoūntai toūs taūta zētoūntas oudè theòs nomízein*

Subject Verb Target Complement

‘Those hearing think **THAT** *those seeking such things do not believe in the gods.*’ (Pl. Ap. 18c).

Here the infinitive construction *toūs zetoūntas oudè theòs nomízein* ‘those seeking do not believe in the gods’ with accusative subject *toūs zētoūntas* functions as Target complement to *hegoūntai* ‘they think’. Greek (28) differs from Hittite (27) in that the Greek complement ends in an infinitive (*nomízein*) instead of accusative agreement (*irman* or accusative participle *akkantan*). The Greek complement, postposed after Subject + Verb, yields main clause order SVO in contrast with Hittite SOV order. But both mark initial and final constituents of Target complement for its grammatical function, in effect isolating it grammatically with raising and the nonfinite predicate.

Also raised, but somewhat differently, is Old Irish infixed pronominal subject of postposed verbal noun *precept*:

(29) *cách ro t chechladar oc precept* (Wb. 28^d16)
each ptc.-you-hear-fut.-3sg. at preaching

‘Everyone will hear *you preaching.*’ (Strachan 1949:115 nr. 147)

In (29) infixed *-t-* is overtly object of main verb *checchladar* ‘hear’, but semantically subject of verbal noun *precept* ‘preaching’.

ANALYSIS

Kuno (1974:130ff.) argued that extraposition and raising avoid perceptually difficult center embedding. In OV Japanese, verb raising replaces English subject raising to subject position (1976:21ff.), and raising to object position differs in having finite complement verbs (40ff.), among other things. Hittite complementizer paradigms further allow us to understand object raising as one of a set of processes motivated in an OV complement system, and to separate IE use of nonfinite complement predicates with raising from the general OV processes.

In SOV Hittite object position is the perceptually difficult one, yet object embedding, excludes only Agent/Actor embedding, but includes

Target, Manner, Time/Place e.g. (verb complementation and adverb clauses). This predicts that most subordinate clauses will be perceptually difficult and motivate some kind of object isolation to avoid center embedding problems. And such motivation results from word order types, *SOV*, not *VSO* or *SVO* (Kuno 1974). In fact, the unifying factor in the Hittite complement paradigm is avoidance of the position between Subject and Verb. The avoidance usually takes the form of *OV* preposing, which earlier scholars described as parataxis, complement plus *-at* e.g. Whereas *SVO* subject complements are the center embedded ones and result in extraposed structures (Kuno 1974:128ff; cf. *'The fact that the king had lost his speech was obvious'* clarified by *'It was obvious that the king...'*), *SVO* object complements are the vulnerable ones. Hittite (16) thus represents the typological counterpart with extraposing to the left instead of the right. Precomplementizing *-at* finds its parallel in English *It*. Both endophorics stand in the main clause where the difficult complement clause would have been: *SVO* gives English *ItVOS*, but *SOV* Hittite gives *O (n=) atSV*. Other grammatical factors such as preverbal positioning of the indefinite Subject *kuiški* (14b), encoding of a subject from context as suffix on the Verb (*OV_S*), and verb raising¹³ conspire to achieve the same effect: isolation of perceptually difficult center embedding.

While extraposing positionally isolates the difficult object complement, raising to object position achieves this grammatically. Japanese raising has, as isolating boundaries at either end of the complement, raised subject and postposed complementizer particle *to*. In IE the grammatically isolating boundaries are again the accusative first word, but the final boundary is the nonfinite predicate, in Hittite the accusative adjective or participle. Both preposing (16) and raising (27) in Hittite thus have the effect of isolating on a linear continuum an otherwise center embedded complement. While extraposing positionally isolates it, raising does it grammatically.

RECONSTRUCTION

Reconstruction, as an heuristic device, posits a possible original symmetric system as prototype of attested asymmetries. Use of Hittite morphemes in the syntactic etymology here is not to imply the Indo-Hittite hypothesis, but to avoid morphological arguments which are separate. The earlier consistent *OV* set of contrasts is based on the opposition

between universal categories in factive ‘know’ and nonfactive ‘hear’. Postposed particles mark noun class, verb class, discourse deixis, and complementizer function in pre-IE:

- (30) *u-k Mursili-s mem(i) a-s i-an (nu) istamas-m-i
 I Mursilis speech go hear
 ‘I hear (that) the speech of Mursilis went (away).’
 *u-k Mursili-s mem(i) a-s es/har (ta)¹⁴ sak-H-i natta
 ‘I did not know (that) M. could speak (lit. ‘had speech’).’

One can further distinguish PIE contextual variants:

- (31) (a) Mursili_s is topic in the context of this construction:
 *u-k (Mursilis) memian appa ian (ta) (nu) istamasmi (*appa*:prev.)
 *u-k (Mursilis)¹⁵ memian es/har (ta) (nu) saggahhi
 (31) (b) First person *uk* is emphatic, M. is still topic:
 u-k-ma Mursilin¹⁶ memias¹⁷ appa ian (ta) (n) istamasmi (-ma*:ptc.)
 *u-k-ma Mursilis memian es/har (ta) (nu) saggahhi
 (31) (c) First person *uk* is topic in the context:
 *u-k (nu) Mursilin memias appa ian (ta) (n) istamasmi
 *u-k (nu) Mursilis memian es/har (ta) (nu) saggahhi

Widely separated branches Anatolian, Hellenic, and Celtic exemplified by Hittite, Classical Greek, and Old Irish independently innovated particular raising constructions from earlier OV object isolation. Late Hittite reflexes of PIE nonfactive complements would look like:

- (32) nu= šši ammuk-ma memian appa ian(tan) nu ištamašmi
 ptc. to-him I-ptc. speech-acc. prev. go (part)-acc. ptc. hear-1sg.
 ‘I hear that his speech is gone.’

Hittite has reanalyzed postposed particles (e.g., *nu*) as initial and innovated in choosing the participle as nonfinite verbal. Classical Greek instead chooses the infinitive as nonfinite form in primary object isolation:

- (33) egō dè tòn toû Sōkrátous lōgon apheīnai akouō
 I-nom ptc the-acc. the gen. S.-gen. word-acc. prev.-go-inf. hear 1sg.

Secondary object isolation postposed the grammatically isolated object:

- (34) egō dè akouō tòn toû Sōkrátous lōgon apheīnai.

Major Greek innovations include generalization of enclitic position for particles, use of infinitive as verbal, and secondary postposing. Old Irish shares particle usages with Hittite (Dillon 1947:22ff.), secondary object isolation with Greek, but has its own treatment of complement postpos-

ing. The raised pronominal subject of the complement is not moved with it:

(35) *ro-t (*oc*) *precept* chechladar ro-t-chechladar *oc precept*

The single nonfinite verbal noun of Old Irish characterizes this construction like other complement constructions.¹⁸

CONCLUSION

While Kuno showed that OV postposed complementizers contrast typologically with the VO preposed variety, this does not account for asymmetries in complementizer usage typical of older IE languages. I have argued here that raising to object position is in origin one of a number of solutions to an OV perceptual problem in Hittite, and that the Hittite construction is sufficiently parallel to other older IE constructions to conclude that the IE variety of raising to object position is distinct from the OV motivated structure only in having nonfinite predicates instead of postposed particles. I have not attempted to account for asymmetries such as endophoric correlatives, leaving it open as to whether or not they characterize inconsistent language types. It seems somehow unlikely, though, for them to be peculiarly IE. Further studies need to determine the paradigmatic relations of raising in the complementizer systems of particular verbs in each of the IE languages, as well as the degree to which other Hittite verbs elaborate the system described for 'know' and 'hear'.

NOTES

* This study represents part of investigation into IE complementation and is particularly related to the portion being published on *šak(k) / šek(k)* 'know' as Nr. 7, Lfg. 12 of Annelies Kammenhuber's *Materialien zu einem hethitischen The-saurus* (Heidelberg: Winter), which is based on her files and owes much to her philological expertise and comments. Discussion of both theoretical and philological points at this conference has sharpened the focus of several issues in important ways. Abbreviations for textual citations are from Liddell and Scott's *Greek Lexicon*, Lewis and Short's *Latin Dictionary*, and Friedrich and Kammenhuber's *Hethitisches Wörterbuch* except for the archaic Latin inscription, *Senatus Consultum de Bacchanalibus* (S. C. Bacch.).

Grammatical abbreviations are ptc.(particle), inf. (infinitive), prev. (preverb), imper. (imperative), part. (participle), nom. (nominative), acc. (accusative), gen. (genitive), n/a (nominative-accusative), d/l (dative/locative), dat. (dative), pro. (pronoun), n. (neuter), fut. (future), perf. (perfect), sg. (singular), and pl. (plural).

(1) I disagree, of course, with Lightfoot (1979 and here) that we cannot reconstruct a protosyntax because we cannot reconstruct particular underlying grammars in a phonological Andersenian (1973) sense. Use of reconstruction as a model incorporating what we know generally about language in fact is predictive and explicit for the study of diachronic syntax in a way that transformational-generative models are not.

(2) Christian Lehmann's objection, that the OV Quechua shares this IE particular, is quite to the point, but needs to be analyzed within the system of Quechua independent of contact with IE Spanish of the region.

(3) Traugott's (1978) argument that spatial and temporal represent a single original function explains Hittite use of *kuwapi* 'where/when'.

(4) More recently McCawley (e.g. 1978) has emphasized the role that pragmatic factors play in complementizer choice in Japanese.

(5) Cf. Hittite supine (-*uwan* plus *dai-* 'set, place' meaning 'begin to' (Kammenhuber 1955:31-57).

(6) Absence of a complementizer word is probably not VO, but needs study in OV languages (e.g. 15) in relation to verb raising (7).

(7) IE complementizing particles include English 'to' and Gothic *ei*.

(8) Japanese e.g. lacks endophoric 'it' in clefting (Kuno 1978: 137) and nonfinite verbals with raising (Kuno 1976: 40-3).

(9) This example (HAB III 68f. = IV 67f., Mat. heth. Thes. 62) has two parallels

in other Old Hittite texts which are also copies, not original ductus (cf. HG II 48 and HG II 77 = Mat. heth. Thes 53 and 54).

(10) Erich Neu called attention to philologically difficult *-aš* (nom. common gender) which I translate ‘it’. The problem has its parallel in use of *-an* (acc. common gender) to refer to an entire preceding clause (Justus 1979a). Formally locative *ša-ta*, as predicate of a clausal subject, fills a syntactic gap left by active *šak(k)* - / *šek(k)* - which a medio-passive 3s in *-ta* would fill (cf. Mat. heth. Thes Lfg. 12).

(11) Correlative examples are not confined to the use exemplified here.

(12) Neu’s objection, that *irman* is not a participial form, is in fact important for the understanding of the earlier situation, and calls attention to the need to distinguish types of raising in Hittite. As accusative, however, *irman* is clearly not the predicate nominal one expects in an independent clause. The fact that this text is not OH must also not be used to conclude that it cannot exhibit an archaic usage. Studies in variation theory (e.g. Weinreich, Herzog, and Labov 1968) have shown that precisely functionally differentiated innovation beside archaism at a single synchronic state is a mechanism for embedding change.

(13) A series of clauses with the same subject or one understood otherwise from context is not repeated in each clause but coded as agreement in older IE verbal inflection, effecting a result similar to Japanese verb raising.

(14) The *-s* of active-animate nouns is ambiguously nom./gen. (Lehmann 1958), while original postposed particles (e.g. *ta*) contrast with nonfinite forms such as *i-an*. ‘Be’ and ‘have’ express possession.

(15) Repetition of the topic is optional.

(16) IE raising probably marked the first word as accusative, not necessarily the subject, if the subject is preceded as here by the OV genitive modifier ‘of *M*. (the) speech’. Order Subject, Appositive (27: *Arnuwandan šeš=JA* ‘A., my brother’) is just another OV possibility.

(17) At this stage noun class suffixes *-s*, *-m* (*-n*), *-H* have become case endings (Lehmann 1958), *-s* now being ambiguous (cf. note 14).

(18) Lehmann and Lehmann 1975: 13f.; 70f.; Thurneysen’s Old Irish Grammar: 444ff.

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**ZUR REKONSTRUKTION VON INFINITIVKONSTRUKTIONEN
IM INDOGERMANISCHEN**
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0. Es hat in den letzten Jahren nicht an Arbeiten zur Syntax des Indogermanischen gefehlt, die das Beschreibungsformat der Transformationsgrammatik benutzen und sich auf Erkenntnisse der Universalienforschung stützen konnten. Ich bin allerdings der Meinung, dass in einigen Fällen erst deskriptive Vorfragen hätten geklärt werden müssen, bevor rekonstruiert wurde. Obwohl z.B. die grammatische Kongruenz ein altbekanntes Phänomen ist, dem man wegen seines Vorkommens in allen indogermanischen Sprachen gerne ein hohes Alter und einen gemeinindogermanischen Status zusprechen möchte, ist mir keine befriedigende Gesamtdarstellung für die alten Sprachen bekannt. Um solche Vorfragen geht es mir in diesem Vortrag. Ich möchte auf einige Infinitivkonstruktionen eingehen die man nur erklären kann, wenn man z.B. weiss, was Kongruenz ist. Die grammatische Kongruenz ist eine Regel von hoher Konstanz, deren Anwendungsbereich in den einzelnen Sprachen sich im Laufe ihrer Geschichte geändert hat. Für eine gesicherte Rekonstruktion scheint es mir wichtig zu sein, genau zu erkennen, wie Konstanz und Veränderung im sprachlichen System verteilt sind.

1. Die Infinitivkonstruktionen der alten Sprachen sind in den letzten Jahren relativ gut erforscht worden; abgesehen von mehr morphologisch orientierten Arbeiten wie denen von Jeffers (1975) oder Rix (1976) ist

nach der umfassenden Arbeit von Sgall (1958) vor allem diejenige von Haudry (1977)¹ zu nennen. In der zuletzt genannten Untersuchung spielt die Erscheinung des doppelten Dativs eine grosse Rolle. Diesem wichtigen Punkt der Infinitivsyntax möchte ich mich zunächst zuwenden und mich dabei auf eine Besprechung der altindischen Verhältnisse konzentrieren.

Ich gebe zunächst Beispiele für die zwei hauptsächlichen Typen des doppelten Dativs.²

- (1) 10, 116, 1 *píbā ... vr̥trāya hāntave śaviṣṭha*
 ‘trinke, du Kraftvollster, um dem Vr̥tra zu erschlagen’.

Hier ist Vr̥tra logisches Objekt von *hāntu-*; in anderen Fällen handelt es sich bei einem der Dative um das logische Subjekt:

- (2) 1, 28, 6 *indrāya pātave sunú sómam*
 ‘schlag den Soma für Indra zum Trinken aus’.

Hier ist Indra logisches Subjekt von *pātu-*. Wie kommt dieser doppelte Dativ zustande? Ist er etwa durch eine besondere Regel für Infinitive zu erklären? Oder bedarf es vielleicht nur der richtigen Einordnung in einen bekannten Rahmen?

Es ist Haudrys (1977) Verdienst, die beiden hauptsächlichen älteren Erklärungsversuche zu widerlegen. Der eine besagt, es handle sich um ursprünglich selbständige Dative, die je für sich als finale und ähnliche Ergänzungen zu verstehen sind, wobei der zweite Dativ eine Art “Expansion” des ersten wäre. Danach wäre (1) zu übersetzen als ‘für den Vr̥tra, für das Erschlagen’. Eine solche Uebersetzung verdeckt aber, wie so viele Scheinerklärungen, die notwendige syntaktische Beziehung zwischen den beiden Dativen; sie ist semantisch völlig unbefriedigend, weil keiner der beiden Dative unabhängig vom anderen sinnvoll ist.³

Eine Erklärung des doppelten Dativs muss die Subjekts- bzw. Objektsbeziehung berücksichtigen. Dies tut nun die andere Erklärung, indem sie von einem rein nominalen Syntagma ausgeht wie:

- (3) (a) *vr̥trām* (Objektsakkusativ)/*vr̥trāya* (Objektiver Genitiv)
hāntave

bzw. (b) *indrāya* (subjektiver Genitiv) *hāntave*

und die Dative statt der Genitive (bzw. des Akkusativs) durch “Attraktion” erklärt. Da aber in diesem Fall keine Antezedensbeziehung vorliegt wie bei den bekannteren Fällen von Attraktion, ist diese Erklärung nicht

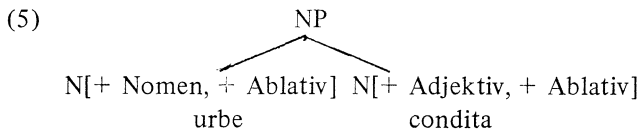
eigentlich falsch, sondern unverständlich. - Haudry (1977) selbst greift zu einem sehr komplizierten Ausgangspunkt, welcher der von ihm mit Recht bekämpften "Expansionstheorie" sehr nahe kommt:

- (4) (a) *āhaye hāntave* 'pour le dragon devant tuer/être tué, bzw.:
 (b) *īndra- pātave* 'Indra destiné à boire'.
īndrāya pātave 'pour Indra destiné à boire', 'pour qu'Indra boive'.

Entgegen Haudrys Meinung hat diese Lösung Nachteile der b e i d e n älteren Erklärungen: Sie setzt ein attributives Verhältnis an die Stelle eines prädikativen,⁴ und sie muss sich des Begriffs der "Attraktion" bedienen (wie Haudry selbst zugibt). Dabei hat bereits Sgall (1958: 200) den Sachverhalt richtig wiedergegeben, wenn auch auf die Fälle mit logischem Objekt beschränkt: "Der Patiens des Infinitivs wird manchmal durch einen mit dem Infinitiv kongruierenden Kasus ausgedrückt, wobei die Kongruenz allerdings nur den Kasus betrifft, nicht etwa Zahl und Geschlecht, denn diese nominalen Kategorien kennt der Infinitiv nicht" - wie jedes normale Nomen, das keine Motion aufweist.

2. Bei dieser Beschreibung bleiben zwei Fragen offen: 1) Was soll man sich hier unter Kongruenz vorstellen, welches sind ihre Bedingungen? Sind die Bedingungen für Kongruenz hier andere als sonst? 2) Wie kommt es, dass in diesem Fall die Bedingungen für Kongruenz überhaupt gegeben sind?

ad 1): Unter Kongruenz versteht man allgemein die Uebereinstimmung von Konstituenten hinsichtlich bestimmter, sog. grammatischer Merkmale. Kongruenz im Kasus (und in Genus und Numerus, die hier - wie gesagt - nicht in Frage kommen) ist zunächst einmal ein typisches Verfahren, durch das in den indogermanischen Sprachen Attribution gekennzeichnet ist. Es gibt keinen Grund, die Bedingung für die Kongruenz z.B. in *urbe condita* in einer komplizierteren Struktur zu suchen als in:



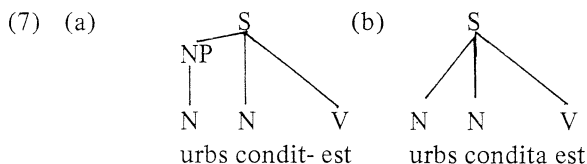
Die Bedingung für Kongruenz lässt sich also so formulieren: Zwei Nomina, die unmittelbar vom gleichen Knoten dominiert werden, kongruieren in Kasus, Genus und Numerus miteinander. Wie ich an anderer Stelle

(Boeder 1973a) ausgeführt habe, muss die Kongruenz von Subjekt und Prädikat auf der gleichen Bedingung beruhen, denn sonst wäre es schwer erklärlich, warum nicht nur das Prädikat mit dem Subjekt, sondern auch das Subjekt mit dem Prädikat kongruiert, z.B. in:

(6) *ea spes est mihi* 'ich hoffe darauf'

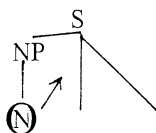
wo *ea* das Subjekt, *spes* das Prädikat ist.⁵

Ich nehme also an, dass sich die Struktur von *urbs condita est* in einer Hinsicht nicht von der Struktur von *urbe condita* in (7b) unterscheidet:

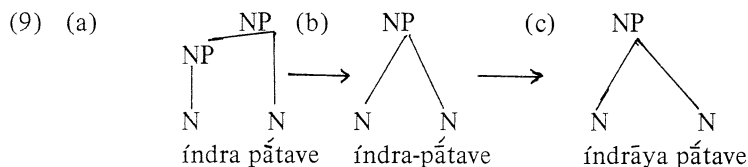


(Auf die Kongruenz in der Person, also die Bildung eines finiten Verbs, gehe ich hier nicht ein; cf. Boeder 1973a). (7) (b) kann aber unmöglich die Tiefenstruktur dieses Satzes sein. Vielmehr ist das Subjekt zunächst als volle Nominalphrase vorzustellen wie in (7) (a). Der Uebergang von (7) (a) nach (7) (b) lässt sich durch eine Regel darstellen, die für eine Fülle von Erscheinungen der Syntax altindogermanischer Sprachen entscheidend ist:

(8) Das Subjekt eines Satzes wird angehoben; d.h. die vom Knoten NP des Subjekts dominierten Konstituenten werden an den nächsthöheren Knoten S gehängt.⁶ Schematisch:



Ganz analog zu dieser strukturellen Veränderung von (7) (a) nach (7) (b) kann man sich nun auch einen Uebergang von (9) (a) nach (9) (b) vorstellen:



In (9) (c) kongruiert das Nomen *índra-* nach der allgemeinen Regel mit dem Nomen *pátave*

Aber wenn diese Annahme richtig sein soll, muss erst die andere oben gestellte Frage in einer bestimmten Weise beantwortet werden.

ad (2): Es ist zwar richtig, dass die Struktur (9) (b) die strukturellen Voraussetzungen für die Anwendung der Kongruenzregel bietet. Aber wie kommen diese strukturellen Voraussetzungen selbst zustande? Es ist zwar richtig, dass *índra-* e i n e A r t Subjekt ist, nämlich das sog. logische Subjekt von *pātave*, aber eine Nominalphrase ist schliesslich kein Satz und man erwartet in der Struktur (9) (a) eigentlich nichts anderes als einen Genitiv, also:

(10) ?*índrasya pātave*

und *índrasya* wäre eben nur ein sog. subjektiver Genitiv, aber kein grammatisches Subjekt.

Nun ist aber aus anderen Gründen klar, dass das Verbalnomen sich in der uns vorliegenden Form des Altindischen wie ein normales Verb verhalten konnte, also wie ein Infinitiv im klassischen Sinne. Ein Beweis sind die Fälle, in denen er ein Akkusativobjekt regiert, z.B. in:

(11) 1, 50, 1 *úd u tyám jātavedasaṃ devāṃ vahanti ketāvah / dṛśé víśvāya sūryam* 'dort fahren den Gott Jātavedas seine Strahlen herauf, auf dass die ganze Welt den Sonnengott schaue'

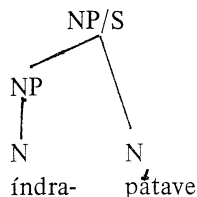
(weitere Beispiele bei Sgall 1958: 195).

Der verbale Charakter kommt ferner darin zum Ausdruck, dass der Agens im Instrumental erscheinen kann wie beim Passiv (cf. Note 8):

(12) 4, 58, 5 *etā arṣanti hṛdyāt samudrāt śatāvrajāḥ ripūṇā na avacákṣe* 'diese (Ströme des Ghṛta) fliessen aus dem Meer im Herzen, durch hundert Gehege geschützt, vom Schelm nicht zu erblicken'.

Mit einem Wort: Nominalphrasen können in dem Sinne satzwertig sein, dass ihr nominales Prädikat, das Verbalnomen, sich wie ein Infinitiv, d.h. wie ein Verb, verhält. Diese Tatsache bringe ich hier informell dadurch zum Ausdruck, dass ich den Knoten NP auch als Satz (Symbol S) kennzeichne:⁷

(9) (a')



In dieser Struktur kann nun *indra-* nach den üblichen Regeln zum grammatischen Subjekt des Satzes gemacht werden und damit erfüllt (9) (a') auch die Bedingung für die Subjektsanhebung.

Allgemein kann man sagen, dass diese Nominalphrase sich nunmehr wie ein Satz verhält: Das direkte Objekt steht im Akkusativ, und dieses direkte Objekt kann zum Subjekt gemacht werden, wenn das Subjekt getilgt ist, und so eine Art Passivkonstruktion bilden.⁸ Ich vertrete also die Auffassung, dass im Typ *vr̥trāya hāntave* das Nomen *vr̥trā-* zum Subjekt von *hāntave* geworden ist und in

(13) 9, 86, 20 *mādhu kṣarad indrasya vāyóh sakhyāya kártave*

‘strömt er Süßes aus, um Freundschaft mit Indra und Vāyu zu schließen’

ist *sakhyā-* ‘Freundschaft’ Subjekt von *kártave* ‘machen’. Eine Passivkonstruktion ist keineswegs an eine morphologische Passivform gebunden; auch das lateinische Gerundivum ist syntaktisch gesehen Prädikat einer Passivkonstruktion, aber keine Passivform des zugehörigen aktiven Gerundiums.

Uebrigens tritt die Kongruenz zwischen Subjekt und Verbalnomen nicht nur beim doppelten Dativ auf. Die eben gegebene Erklärung bewährt sich auch bei den Infinitiven im Genitiv, Ablativ und Akkusativ (wo übrigens Haudrys Erklärung m.E. überhaupt nicht anwendbar ist).

(14) 10, 138, 5 *indrasya vājṛād abibhed abhiśnáthaḥ* ‘sie fürchteten sich, von Indras Keule getroffen zu werden’.

Hier ist *indrasya vājṛa-* logisches und grammatisches Subjekt des Infinitivs, dessen Kasus, der Ablativ, von *abibhed* regiert ist.⁹

3. Wenn meine Erklärung stimmt, lassen sich eine Reihe von Voraussagen machen:

1) Ein Verbalnomen mit kongruierendem Subjekt kann nicht gleichzeitig einen adnominalen Genitiv bei sich haben, eine Struktur wie:

(15) * *indrāya pātave sómasya*

ist unmöglich, denn dann hätte dies Syntagma teils nominale, teils verbale Rektion. Soweit ich sehe, stimmt diese Voraussage.¹⁰

2) Als kongruierendes Nomen kann nur das Subjekt oder das direkte Objekt auftreten. Hier treten Probleme auf, insofern die Angabe des Ziels bei Verben der Bewegung in dieser Position erscheinen kann:

(16) 2, 29, 6 *trādhvaṃ (no devā) kartāḥ avapādaḥ* ‘behütet uns (ihr

Götter) vor dem Fall in die Grube!,
wo *kartá-* ‘Grube’ sich wie ein direktes Objekt von *avapádah* verhält, da es nach meiner Interpretation dessen Subjekt geworden ist. Sgall (1958: 200) weist zweifellos mit Recht auf den Akkusativ des Ziels im entsprechenden Aktivsatz hin:

(17) 9, 73, 9 *kartám ávapaḍāti* ‘(der Unvermögende) soll dabei in die Grube abstürzen’.

Aber ein Akkusativ des Ziels verhält sich in der Passivkonstruktion nicht wie ein direktes Objekt. Ich kann diese Fälle z.Zt. nicht erklären.¹¹

3) Es können keine *z w e i* Nomina, nämlich logisches Subjekt und logisches direktes Objekt gleichzeitig mit dem Verbalnomen kongruieren, denn ein Satz kann in den indogermanischen Sprachen keine zwei Subjekte gleichzeitig haben. Auch hier will ich nicht verschweigen, dass es wenigstens *e i n* Gegenbeispiel gibt (Sgall 1958: 210):¹²

(18) 10, 125, 6 *ahám rudráya dhánur ā tanomi brahmadviṣe śárave hántavā u* ‘Ich spanne für Rudra den Bogen, dass sein Geschoss den Feind der heiligen Rede töte’,

wo *śáru-* ‘Pfeil’ Agens und *brahmadviṣ-* ‘Feind des heiligen Wortes’ Patiens von *hántave* ist, mit dem beide kongruieren. Ob es sich hier um einen Verstoss gegen die Regel handelt oder ob die Regel in irgendeiner Hinsicht falsch ist, lässt sich schwer entscheiden. Jedenfalls muss die Vedaforschung ja auch an anderen Stellen mit Abweichungen von bewährten Regeln rechnen.

4) Solche Formen, bei denen keine Kongruenz im besprochenen Sinne auftritt, können syntaktisch nicht als Verbalnomina aufgefasst werden. Dies gilt z.B. für die sog. Infinitive auf *-dhyai*, deren Auslaut wie ein Dativsuffix aussieht, oder die sog. Infinitive auf *-sani*, *-ani*, *-tari*, deren Auslaute wie Lokativsuffixe aussehen. Es ist sicher kein Zufall, dass zugehörige andere Kasusformen fehlen (Sgall 1958: 157). Man könnte denken, dass gerade dieses Fehlen eines paradigmatischen Zusammenhangs dazu geführt habe, dass z.B. bei den Infinitiven auf *-dhyai* keine doppelten Dative vorkommen (Benveniste 1935: 97). Auch hier gibt es allerdings wieder eine einzige Ausnahme (unter 75 Stellen!):

(19) 6, 1, 1 *tvám sīm vṛṣann akr̥ṇor duṣṭárītu sáho viśvasmai sáhase śáhadhyai* ‘du machtest es zu einer unüberwindlichen Macht, um jede Macht zu besiegen’.¹³

Aber entscheidend ist die Erkenntnis, dass die Syntagmen mit diesen sog.

Infinitivformen (auf *-dhyai* etc.) nie den Ausdruck eines logischen Subjekts enthalten (Sgall 1958:238); sie haben selbst nie ein Subjekt, auch keines in einer Passivkonstruktion. In diesem Punkt verhalten sie sich wie die lateinischen Gerundia, deren getilgte logische Subjekte immer mit Konstituenten des jeweiligen Matrixsatzes identisch sind. Das akkusativische Subjekt in der Konstruktion des Accusativus cum infinitivo ist demgegenüber bekanntlich Bestandteil des Infinitivsatzes selbst. Ein Accusativus cum infinitivo wie

(20) Plautus Am. 1. 1. 197 *advenisse familiarem dicito*

hat Eigenschaften sowohl des Typs *īndrāya pātave* (2) als auch der Infinitive auf *-dhyai* etc. Letzteres insofern, als *advenisse* wie alle lateinischen Infinitive sich nicht wie die Kasusform eines Verbalnomens verhält, was auch immer der morphologische Ursprung der lateinischen Infinitive sein mag. Ersteres insofern, als *familiarem* Subjekt von *advenisse* ist in genau dem gleichen Sinne, wie *īndrāya* in (2) das Subjekt von *pātave* ist. Dass *familiarem* im Akkusativ steht, beruht aber nicht auf Kongruenz, denn *advenisse* ist ja eine kasuslose Verbform. Die Akkusativform beruht lediglich darauf, dass der Akkusativ (und nicht der Nominativ) im Lateinischen syntaktisch die merkmallöse Form ist.

4. Es gibt noch eine weitere Regel für die Bildung von Infinitivsätzen, die sich ebenfalls in praktisch allen indogermanischen Sprachen findet und bisher im Altindischen – soweit ich sehe – ebenso verkannt worden ist wie die doppelten Dative und dgl. Es handelt sich um das Phänomen, dass der Infinitiv im Dativ, das Subjekt aber in dem vom Verb des Matrixsatzes geforderten Kasus steht. Nehmen wir etwa die Modalverben wie in:

(21) 8, 4, 17 *vémi tvā pūṣann ṛñjāse, vémi stótava* ‘Ich wünsche dir, Pūṣan, den Vortritt zu lassen; ich wünsche dich zu preisen’.

Die Wurzel *ṛñ-* ‘wünschen’ wird mit dem Akkusativ konstruiert. Stattdessen steht hier ein dativischer Infinitiv und dessen logisches Objekt im Akkusativ.¹⁴

Ahnlich verhält sich das Verb *veda*:

(22) 8, 18, 5 *té hí putráso áditer vidúr dvéṣāṃsi yótave* ‘Denn diese Söhne der Aditi wissen der Feindschaft zu wehren’.

Direktes Objekt von ‘wissen’ ist hier ‘Feindschaften’ und nicht ‘Abwehr der Feindschaften’.

An einer anderen Stelle erscheint denn auch tatsächlich das Verbalnomen in dem Kasus, der nach der Rektion von *vid-* zu erwarten ist, nämlich im Akkusativ:

- (23) 4, 8, 3 *sá veda devá ānāmaṃ devāṁ ṛtāyaté dāme* ‘Der Gott weiss die Götter herumzuwenden, dem der recht wandelt, ins Haus’.

Einerseits gibt entsprechend der oben besprochenen Regel *īse* mit dem von ihm regierten Genitiv und dem Patiens als Subjekt (d.h. in Kongruenz):

- (24) 7, 4, 6 *īse hī agnir amṛtasya bhūrér īse rāyāḥ suvīryasya dātoḥ*
‘Denn Agni vermag den reichlichen Lebensbalsam, er vermag einen Schatz von guten Söhnen zu geben’.

Andererseits wird an einer anderen Stelle deutlich, dass der Kasus des Subjekts in solchen Fällen vom regierenden Verb abhängt, während der Dativ von dieser Rektion unabhängig ist (cf. Haudry 1977: 125):

- (25) 8, 25, 20 *vāco dīrgháprasadmani īse vājasya gómataḥ īse hī pitvāḥ aviśāsya dāvāne* ‘Ein Wort bei Dirghaprasadman vermag rinderreichen Lohn, es vermag ja giftlose Speise zu geben’.

Hier steht das logische Objekt von ‘geben’, nämlich ‘giftlose Speise’, im Genitiv, der von *īse* regiert wird, während *dāvāne* ‘geben’ selbst im Dativ steht, obwohl es doch von *īse* regiert sein müsste.

Diese merkwürdige Verschiebung der Relationen entspricht dem, was aus der traditionellen und der Transformationsgrammatik unter verschiedenen Namen bekannt ist. In einem Satz wie Terenz Eu. 1035 *scis me, in quibus sim gaudiis* ist das Subjekt des eingebetteten Satzes “vorweggenommen” (Prolepse) und Bestandteil des Matrixsatzes, nämlich dessen direktes Objekt, geworden (cf. Plautus Tri 992 *si t e flocci facio an periis ses prius*; Terenz Ad. 874 *illum ut vivat optant*). Dabei ist zu beachten, dass der Akkusativ damit die Satzgliedfunktion übernimmt, die zunächst der eingebettete Satz hatte. Ebenso kann das vorweggenommene Subjekt auch die Satzgliedfunktion des Matrixsatzsubjektes übernehmen, wenn der untergeordnete Satz ein Subjektssatz war: Cicero Lae. 63 (*quam leves sint quidam amicitiae, saepe in parva pecunia perspicitur* →) *quidam saepe in parva pecunia perspiciuntur quam sint leves* (cf. Cicero Off. 3, 104 (*ne Iuppiter iratus noceret, non fuit metuendum* →) *non fuit Iuppiter metuendus ne iratus noceret*). In den Fällen, wo der untergeordnete Satz ein Infinitivsatz ist, spricht man z.B. von Nominativus cum Infinitivo,

wenn das Subjekt des Infinitivsatzes die Subjektfunktion des Matrixsatzes übernimmt: Plautus Asin. 382 *Demaenetus ubi dicitur habitare*.– Dass es sich bei der Prolepse und bei den Konstruktionen des Typs (25) um die gleiche Erscheinung handelt, wird u.a. dadurch gestützt, dass sie bei den gleichen Verben auftritt (“wissen”, “machen”, “wünschen” und dergleichen). Die Gemeinsamkeit zwischen all diesen Konstruktionen und solchen des Typs (25) besteht darin, dass a) das Subjekt des eingebetteten Satzes dessen Satzgliedfunktion übernimmt und den Kasus aufweist, der vom Matrixsatzverb regiert ist, und den z.B. auch gewisse Proformen des Satzes haben würden; und dass b) der übrige Satz selbst nach einem bekannten Verfahren “extraponiert” wird und so eine “Randstellung” einnimmt.¹⁵ Aus dieser Randstellung erklärt sich die Tatsache, dass der Infinitiv im Dativ steht, d.h. einem Kasus, der nicht durch Rektion bedingt ist und der auch beim finalen Infinitiv auftritt.¹⁶

5. Drei Verwendungsweisen von besonderem Interesse sollen hier angeschlossen werden.

1) Im ŚB 2.1.4.16 heisst es:

(26) *tád áśvam ānetavai brūyāt* ‘he should order [them] to bring a horse then’ (Lehmann 1974: 165).

Wie erklärt sich der Akkusativ *áśvam* bzw. der Dativ *ānetavai*? *brū-* regiert den Akkusativ, aber nicht den Dativ. Ich kann mir die Konstruktion nicht anders erklären, als dass *áśvam*, das logische direkte Objekt von *ānetavai*, zum direkten Objekt von *brūyāt* geworden ist.

2) Nach dem gleichen Muster möchte ich die auch in anderen indogermanischen Sprachen zahlreichen Fälle erklären, wo der “dativische Infinitiv im Prädikat” steht (Sgall 1958: 200-204):

(27) 7, 15, 5 *spārhā yāsya śrīyo dṛśé*

bedeutet nicht: ‘dont les gloires sont désirables à voir’ (Haudry 1977: 223-224); nicht die “Herrlichkeiten” sind wünschenswert, sondern das Anschauen der Herrlichkeiten. Sonst ist ganz unklar, welchen syntaktischen Status *dṛśé* eigentlich hat. Eine Uebersetzung wie: ‘dessen Herrlichkeiten köstlich zu schauen sind’ (Geldner: ‘köstlich zum Schauen’), ist keine Erklärung, denn die deutsche Uebersetzung enthält selbst die zu erklärende Konstruktion; die entsprechende Konstruktion *John is easy to see* wird von *it is easy to see John* abgeleitet, und so ist auch hier *spārhā-* als Prädikat eines zugrundeliegenden Komplementsatzes mit *dṛś-* als Prä-

dikat und *yásya śríyah* als Objekt anzusehen.

3) *Kṛ-* ‘machen’ regiert zweifellos den Akkusativ, und deshalb heisst:

(28) 1, 113, 9 *agnīm samídhe cakártha*

eben nicht: ‘du hast Agni zum Anzünden gemacht’ oder ‘du hast Agni zur Flamme gemacht’ (als Möglichkeit bei Sgall 1958:16 erwähnt sondern: ‘(dass) du das Feuer anzuzünden Anlass gabst’ (Geldner). *agnīm* ist hier zunächst das direkte Objekt von *samídhe-* und *samídhe- agnīm* ist dabei als Ganzes direktes Objekt von *kṛ-*; erst dann wird das direkte Objekt *agnīm* zunächst Subjekt von *samídhe-*¹⁷ und danach direktes Objekt von *kṛ-*.

Bei intransitiven Verben tritt das logische Subjekt des Infinitivs als direktes Objekt von *kṛ-* auf:

(29) 10. 39. 8 *viśpālām étave kṛthah* ‘Ihr bringt die Viśpala zum Laufen’.

6. Welche Schlüsse kann man aus dieser kleinen Detailuntersuchung für die Möglichkeiten der Rekonstruktion ziehen?

Es hat sich herausgestellt, dass ein zunächst scheinbar sehr eigentümliches Phänomen wie der doppelte Dativ durch eine ganz einfache Regel zu erklären ist. Es handelt sich um eine gut bekannte und fundamentale Regel der idg. Sprachen, nämlich die Kongruenz zwischen Subjekt und Prädikat. Ähnlich liess sich auch eine andere Infinitivkonstruktion als ein Phänomen (Prolepse, N.c.i. und dgl.) identifizieren, das bei eingebetteten Sätzen auch in anderen idg. Sprachen auftritt. Wesentlich ist dabei, dass es nicht um spezifische Regeln für Infinitivkonstruktionen handelt. Spezifisch ist nur die Tatsache, dass ein Nomen, nämlich das sog. Verbalnomen, sich syntaktisch wie ein Verb verhalten und ein Nominalsyntaxma satzwertig sein kann.

Erst jetzt kann man sinnvollerweise fragen: Was ist der Gegenstand der Rekonstruktion in diesem Bereich? Die Kongruenzregel z.B. wird aufgrund unabhängiger Evidenz ohnehin für bestimmte Stufen des Indogermanischen rekonstruiert. Die Frage nach der Entstehung der Infinitivkonstruktion muss dagegen lauten: Wie ist der kategoriale Charakter der Verbalnomina bzw. des Syntaxmas zu klären, dessen Kern sie sind? Wann und warum verhält sich ein Verbalnomen wie ein Nomen oder wie ein Verb, bzw. wann und warum verhält sich sein Syntaxma manchmal wie ein Satz? Kann der kategoriale Charakter nach Vergleichung der verschiedenen idg. Sprachen auf bestimmte Stufen der idg. Sprachgeschichte

projiziert werden oder kann er sich in verschiedenen Sprachen unabhängig entwickeln? Bisher hat sich das Interesse der Sprachhistoriker hauptsächlich auf die spezifische Form solcher Konstruktionen wie dem doppelten Infinitiv konzentriert und das ist sicher kein Zufall. Jetzt sieht man, dass gerade diese Form als solche nichts Spezifisches bietet und dass nunmehr eine Frage nach der Kategorie, d.h. nach den Merkmalen bestimmter Lexeme übrigbleibt. Diese richtige Lokalisierung des Problems scheint mir eine wichtige Vorbedingung für die Erklärung jeder Sprachveränderung zu sein.

ANMERKUNGEN

(1) Auf dieses Buch hat mich freundlicherweise Herr Michael Meyer-Brügger (z.Zt. Paris) hingewiesen.

(2) Die Rigvedastellen kennzeichne ich nur durch Zahlen und gebe immer die Uebersetzung von K.F. Geldner.

(3) Haudry (1977: 120) gibt die schwächere Formulierung: “il faut pour qu’une expansion soit envisageable, que le syntagme de base ait une existence indépendante, que le premier datif ne soit pas rendu possible par le second.” Dadurch hängt aber die Entscheidung darüber, ob es sich um zwei selbständige Dative handelt, davon ab, ob der zweite Dativ (das Verbalnomen) weglassbar ist. Nun hat Haudry die interessante Entdeckung gemacht, dass praktisch jeder Dativus (in) commodi auf ein prädikatives Syntagma mit implizitem Prädikat zu beziehen ist:

4, 14, 2 *ūrdhvām ketūṃ savitā devō aśrej-jyōtir viśvasmai bhūvanā-ya kṛtvān* ‘Gott Savitr hat sein Banner aufgerichtet, der ganzen Welt Licht bereitend’

ist zu interpretieren als: ‘damit die ganze Welt Licht s e h e’ (wie in: 1. 50. 1 *ūd u tyāṃ jātavedasaṃ devāṃ vahanti ketāvaḥ/dṛśē viśvāya sūryam*). Das zugehörige Prädikat ist nach Haudry (1977: 127) ein “procès ... implicite avec les verbes”; es ergibt sich aus dem Zusammenhang oder, wie man auch sagen könnte: aus der Situation und dem Vorwissen über die Sachverhalte, die normalerweise mit dem im Satz explizit ausgedrückten Sachverhalt verbunden sind: ‘Licht machen für alle’ bedeutet eben: ‘damit alle das Licht sehen’; 4. 30. 21 *āsvāpayad dabhītaye sahasrā triṃśataṃ ... dāsānām* ‘(Indra) versenkte mit seinem Schlägen für Dabhīti dreisigtausend von den Dāsa’s... in Schlaf’ bedeutet im konkreten Fall: “pour qu’il [sc. Dabhīti] en triomphe”. Wie immer diese Erkenntnis grammatiktheoretisch zu verarbeiten ist, sie bedeutet, dass der “erste Dativ” meist ohne den zweiten stehen kann und dass infolgedessen die meisten doppelten Dative nach der “Expansionstheorie” interpretiert werden können. Deshalb ist Haudrys Formulierung eine Minimalbedingung, man kann sie nicht als Kriterium für den doppelten Dativ benutzen.

(4) ‘destiné à boire’ ist ein Attribut von *indra-*; jedenfalls kann ich die französische Uebersetzung nicht anders deuten.

(5) Diesen Fall, sowie auch die Parallelität zwischen der nominalen Kongruenz im attributiven und im prädikativen Syntagma hat z.B. Lehmann 1974: 72-73 nicht berücksichtigt.

(6) In Wirklichkeit ist die Operation etwas komplizierter. Die Regel kann so formuliert werden:

$[X - N - Y]_{NP} \rightarrow [1\emptyset 3]_{NP}$ 2, wenn NP Subjekt ist.

Ich mache dabei drei Voraussetzungen: 1) N steht für alle nominalen Kategorien (Substantive, Adjektive, Pronomina). 2) Der Knoten NP wird gekürzt, sobald er kein

N dominiert, das Kern einer NP sein kann. 3) A l l e von NP dominierten N werden nacheinander angehoben.

(7) Geeignet wäre eine Beschreibung mit Hilfe von Knoten, die aus Merkmalskomplexen bestehen wie [+ S, + NP]. Auf diese Frage kann ich hier nicht eingehen.

(8) An Passivkonstruktionen sind bekanntlich mehrere Regeln beteiligt. Hier ist nur von der Bewegung des direkten Objekts in Subjektsposition die Rede. Die Bewegung des Subjekts in eine Randstellung, d.h. die Bildung einer Agensphrase (mit *von*, lat. *ab* usw.), die im Altindischen im Instrumental erscheint, ist anscheinend auf die Fälle beschränkt, wo der Infinitiv das Prädikat des ursprünglichen Matrixsatzes bildet. Delbrück (1869: 93) meint: "Bei dem passiven sinn, den diese infinitive im satze bekommen, ist es nicht zu verwundern, wenn bei ihnen ein instr., wie auch sonst beim passiven verbum auftritt": 6, 56, 1 *yá enam ādīdeśati karambhād iti pūṣānam / nā tēna devā ādīše* 'Wer ihn, den Pūṣan, mit dem Wort "Breiesser" gemahnt, von dem lässt sich der Gott nicht (erst) mahnen' (cf. Sgall 1958: 237). Diese Konstruktion mit Agensphrase verhält sich zur Konstruktion mit dem Agens im Dativ (3, 9, 2 *nā tát te agne pramīṣe nivārtanam* 'so sollst du, Agni, die Rückkehr nicht vergessen') wie die jüngere lateinische Gerundivkonstruktion mit *ab* (seit Rhet. Her. 1, 3, 5 *id defendimus, quod ab omnibus defendendum videtur*) zu der mit Dativ (*aliquid mihi faciendum est*) und - wie man hinzufügen kann - wie die Kausativkonstruktion mit dem Instrumental (*kārayati kaṭaṃ devadattena* 'er lässt durch Devadatta eine Matte machen') zu der mit Akkusativ (*kārayati kaṭaṃ devadattam* 'er lässt den Devadatta eine Matte machen') (cf. Schmidt 1966: 123).

(9) Weitere Beispiele, auch aus anderen indogermanischen Sprachen, finden sich bei Sgall 1958: 229-235 und Hahn 1966.

(10) Eine Stelle wie 7, 77, 1 *ābhūd agniḥ samīdhe mānusāṇām* 'Jetzt ist es für die Menschen an der Zeit, den Agni zu entzünden' kann ich vorläufig nicht erklären (cf. Sgall 1958: 163). Neben dem subjektiven Genitiv, der auf eine nominale Geltung des Verbalnomens hinweisen würde, tritt hier ein Nominativ auf, der auf "Prolepse" des Subjekts von *samīdhe* beruhen könnte, wie sie beim Infinitiv vorkommt (cf. Note 16). M.E. muss auch erwogen werden, ob es sich nicht um den Genitiv handelt, der mit *as-/bhū-* 'gehören' bedeutet: "die Menschen haben Agni bekommen, damit (sc. sie das Feuer) anzünden). Ähnlich 1, 79, 11 *asmākam id vṛdhē bhava* 'werde unser [mögen wir dich bekommen], damit (sc. wir) gedeihen' (cf. Sgall 1958: 167).

(11) Eine Alternative wäre nur, die Passivregel für Nominalphrasen anders zu formulieren als für Sätze.

(12) Ein weiteres Beispiel ist unklar (Sgall 1958: 200): 1, 111, 4 *te no hinvantu sātāye dhiyē jiṣe* 'die (Götter) sollen uns aneifern, dass das Gedicht den Preis gewinne'. - Es lässt sich eine andere syntaktische Gliederung mit drei oder zwei Dativsyn-

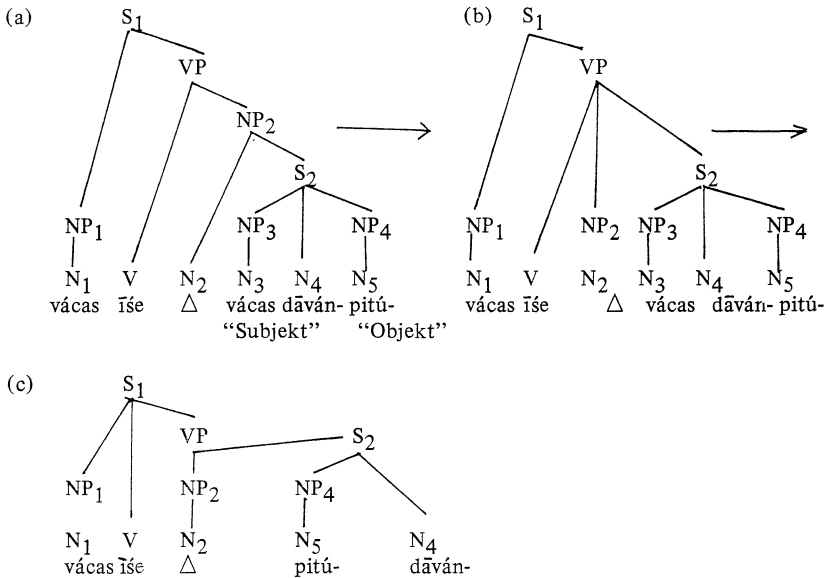
tagmen vorstellen: *sātāye dhiyé/jiṣé* oder *sātāye/dhiyé jiṣé* (?).

(13) Benveniste loc. cit. meint, dass diese Hymne auch aus anderen Gründen spät ist.

(14) 3, 56, 7 *ródasī cid urvī rātnam bhikṣanta savitūḥ savāya* ‘selbst die beiden weiten Rodasi bitten um Savitr’s Kleinod, dass er es zuweise’
übersetzt auch Haudry (1977: 125) trotz seiner Ablehnung der “Expansionstheorie” mit ‘ils demandent un trésor à Savitar, (afin) qu’il le suscite’. Haudry kann zwar für seine Uebersetzung in Anspruch nehmen, dass *rātnam bhikṣanta* ein vollständiges Syntagma ist - ein für ihn wichtiges Kriterium (cf. Note 3). Aber man muss doch sehen, dass bei dieser Auffassung nun der zweite Dativ in der Luft hängt, wie eigentlich schon aus der Künstlichkeit der Geldnerschen Uebersetzung hervorgeht. Die Auffassung als “Expansion” (“Nachtrag”) verwischt die Tatsache, dass *rātnam* notwendigerweise Objekt von *savāya* ist; ein Nachtrag wäre erst dann plausibel, wenn *rātnam* im Vedischen (wie in Geldners Uebersetzung: ‘dass er es zuweise’) durch ein Pronomen im Nachtrag aufgenommen werden könnte, also etwa *tāsya savāya* oder auch *tāsmāi savāya*. Eine solche Stelle dürfte schwer beizubringen sein.

(15) In der Terminologie der relationalen Grammatik heisst dies, dass der Infinitiv ein “chômeur” wird. Im Altgeorgischen, wo die gleiche Erscheinung auftritt (Čxubianišvili 1972; cf. Schmidt 1967) erscheint der Infinitiv im Adverbialis (cf. im einzelnen Boeder 1973 b).

(16) Die oben besprochenen Erscheinungen wurden gelegentlich durch Subjektsanhebung erklärt (Kiparsky 1970). Tatsächlich lässt sich ein Akkusativ bzw. ein Nominativ relativ leicht durch die abgeleitete Satzgliedfunktion der betreffenden NP im Matrixsatz erklären - in einer Sprache wie dem Englischen. In einem Fall wie (25) versagt diese Erklärung, denn das Subjekt des eingebetteten Satzes steht hier in einem s p e z i f i s c h e n, nur vom Matrixsatzverb regierten Kasus. Deshalb lässt sich, soweit ich sehe, die Erklärung durch Anhebung nur retten, wenn man annimmt, dass die Rektionsmerkmale relativ spät in benachbarte Nominalphrasen eingetragen werden, obwohl Wortfolge für syntaktische Prozesse dieser Art in diesen Sprachen kaum eine Rolle spielt. Oder man nimmt strukturbewahrende Regeln an (Emonds 1976), wie z.B. in folgender Ableitung von (25):



Der Uebergang von (a) nach (b) beinhaltet die "Extraposition" von S_2 (den Charakter dieser Regel lasse ich hier offen; cf. Emonds 1976: IV. 2. 2); ferner wird NP_3 getilgt (Subjektsidentität $NP_1 = NP_3$; auf die Regelfolge gehe ich nicht ein) und NP_4 kann "Subjekt" von S_2 werden (s. oben 2); schliesslich ersetzt NP_4 den leeren Knoten NP_2 , wobei angenommen werden muss, dass N_5 die syntaktischen Merkmale von N_2 übernimmt (hier: den Genitiv). - Uebrigens sind Subjektstilgung und "Passivierung" keine notwendigen Voraussetzungen für die Ersetzung des leeren Knotens, auf die es hier ankommt; cf. 1, 127, 11 *māhi śavistha naṣ kṛdhi samcākṣe bhujē asyaī* 'lass uns, Mächtigster, Grosses schauen, um dies zu geniessen', wo das Subjekt von 'Schauen': *naṣ* 'uns' das direkte Objekt von *kṛ* 'machen' wird (s. unten 5.3), und wo *māhi* das direkte Objekt des dativischen Infinitivs *samcākṣe* bleibt.

(17) Tatsächlich gibt Whitney (1889: Abschn. 98 b) mit seiner Uebersetzung diese Zwischenstufe an: "thou hast made the fire to be kindled"; cf. Sgall 1958: 162.

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ON THE RECONSTRUCTION OF THE SYNTAX
OF COMPARISON IN PIE

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1. This paper will be concerned with the reconstruction of one specific syntagma in PIE, namely the comparative construction. But first, by way of introduction, I would like to give the following definition of the syntagma. By comparison I mean comparison of inequality as exemplified by "A is bigger than B." The syntagma in question here is composed of the following elements: (i) the ADJECTIVE *bigger*, (ii) the STANDARD B, and (iii) the PIVOT which is to be regarded as the syntactic means used to connect the ADJECTIVE with the STANDARD in this case it is the particle *than*. An investigation of the comparative constructions in different languages of the world reveals the following means used to accomplish this connection:¹

- (a) JUXTAPOSITION: (A is) big(ger), B (is) not (big)/ small,
- (b) PRE-/POSTPOSITION: (A is) big(ger) from B,
- (c) CASE: (A is) big(ger) B-from, and
- (d) PARTICLE: (A is) bigger than B

It should be pointed out here that only a very few languages have a special comparative form of the adjective (i.e. bigger) at their disposal.² Consequently, those languages which do not have such a form use the positive form of the adjective in the construction types (a) through (c) above. To my knowledge, the particle construction is only found in

those languages which have specific comparatives adjectives.

EXAMPLES FROM INDO-EUROPEAN LANGUAGES

(a) JUXTAPOSITION

- (i) Without comparative adjectives: Hittite

nu-wa-za zig LUGAL.GAL

ammuug-ma-wa-za LUGAL.GAL.TUR-*RU* (KBo VI 29 II 1)

‘And you (are) a great king,

I (am) but a small king.’³

- (ii) With comparative adjectives: Classical Sanskrit

śreyān mṛtyur na nirjayaḥ

‘Better (is) death, not defeat.’⁴

(b) PRE-/POSTPOSITION

- (i) Without comparative adjectives: Hindi

mohan se barā

‘Mohan from big’ = ‘bigger than Mohan’

- (ii) With comparative adjectives: Modern Greek

megályteros apò tò Giánnē

‘bigger from John’ = ‘bigger than John’

(c) CASE

- (i) Without comparative adjectives: Hittite

kapru-set-a-sta-kapruaz GAL-*li* (KUB XLIII 53 I 22)

‘Then his throat (is) big(ger) than (an ordinary) throat (lit. throat-from big).’⁵

- (ii) With comparative adjectives: Latin

melius morte (Plautus: Rud. 675)

‘better death-form’ = ‘better than death’

(d) PARTICLE

- (i) Without comparative adjectives: No examples

- (ii) With comparative adjectives: English

(John is) bigger than Mary

This investigation leads us to the following insight. Most (if not all) languages are able to express a comparison of inequality through some syntactic device with or without a comparative form of the adjective. In our reconstruction of this syntagma we are therefore faced with two distinct questions: First, did PIE possess a comparative form of the adjective or not? Regardless of the answer to this question we must further

ask: What (which) syntactic means was (were) used in PIE to express the construction? In other words, if one assumes that the comparative form of the adjective was not a feature of PIE there was still a way of expressing comparison without it.⁶

2. Let us now turn to the first question, i.e. did PIE have a comparative form of the adjective at its disposal? In order to answer this as well as other questions of PIE grammar one must first collect *and then evaluate* the data from all the dialects. There seem to be two different views at present with respect to our question. One view holds that in PIE only the positive form of the adjective was used in comparisons of inequality. Lehmann, for example, finds examples from Indic in which a positive is used and concludes that this reflects the original construction.⁷

Whereas the data from Indic may show a positive in this function, the *evaluation* of this data for the purpose of reconstruction clearly points to the fact that this usage of a positive is to be regarded as an Indic innovation.⁸

There can be no doubt, then, that in Common-Indo-European there was a comparative suffix **-yes-* for adjectives used in the comparative construction, i.e. an element which exhibits identical **FORM**, **FUNCTION**, and **CONSTRUCTION** in the dialects.⁹ It must be added here that it is an entirely different question as to how and when this suffix developed in earlier stages of the proto-language. In addition to **-yes-* there was also another suffix **(t)ero-* which was used in comparisons (and elsewhere), but which only later acquired the meaning of a comparative suffix in Greek and Aryan.¹⁰

3. The next problem is to determine the syntactic means used in the expression of comparison of inequality. The *communis opinio* regards the particle construction as definitely a feature of PIE, being supported by constructions in most Indo-European dialects including Indic, cf. examples such as:

Classical Sanskrit: *śreyān mṛtyur na nirjayaḥ*.¹¹

A closer look at Indic reveals, however, a somewhat different state of affairs. First of all, the type of expression just mentioned with *na* is, technically speaking, an example of comparison expressed through juxtaposition and not through a particle.¹² And secondly, this type of comparison (with *na*) is not attested in the Rigveda and has not been found

in Old Indic (i.e. pre-Paninian Sanskrit). A true particle construction is found in Pāli¹³ and there are very few examples in the Brāhmaṇas.¹⁴ We should, therefore, leave open the possibility that the particle construction is also an innovation in Indic especially since it is not found in Hittite nor in Mycenaean and the particle differs from language to language where it is found. I will return to this construction later on.

Since all Indo-European dialects have had the ablative (or its cognate) case construction, i.e. identical **FORM** (ablative case) and **FUNCTION** as well as **CONSTRUCTION** (comparison of inequality), we can reconstruct the following construction for PIE:

ADJECTIVE+ *-yes-, **STANDARD** (in the ablative case).

Since the particle construction, on the other hand, has only identical **FUNCTION** and **CONSTRUCTION** but not **FORM** - the particle is different in every dialects where it is found - it is to be regarded as representing parallel development where it does appear.

4. In Indo-European studies our aim is two-fold: first we must attempt to reconstruct the original language, and second, attempt to explain how the different dialects have developed from this reconstructed state. In our investigation of the comparative construction so far we have reconstructed the PIE syntagma for comparison. What is left now is to explain how the different construction types in the dialects could have developed from this state.

I would now like to introduce the notion of **MARKER** in the comparative construction as that element which differentiates a comparison of inequality from a comparison of equality. In the juxtapositional construction the **MARKER** is either the negative element (not (big)) or the antithetical quality (small). For the pre-/postpositional and the case constructions, the **MARKER** is identical with the **PIVOT** i.e. the pre-/postposition and the case ending respectively. Languages which have a specific comparative form of the adjective at their disposal will have an additional **MARKER**, namely the comparative suffix/form of the adjective. We notice, then, that our reconstructed comparative construction has **TWO MARKERS**, namely the ablative case ending and the comparative suffix *-yes-. The question which now arises is: what is the **MARKER** in the particle construction? Let us take a look at a few examples:

(a) Latin: *tam magnus quam tu*

maior quam tu

- (b) German: *Fritz ist so gross wie Johann.*
Fritz ist grösser wie Johann. (Dialectal)

- (c) French: *aussi bon que ...*
*meilleur que ...*¹⁵

and as an additional illustration:

- (d) Finnish: *Hän on yhtä pitkä kuin sinä*
 ‘he is as big as you’
Hän on pitempi kuin sinä
 ‘He is bigger as (=than) you.’

These examples indicate that the particle (i.e. the PIVOT) does not distinguish a comparison of inequality from a comparison of equality, and therefore the particle is not a MARKER. As I have already mentioned, the particle construction is only found in those languages which possess a comparative form of the adjective. The MARKER, then, in the particle construction is the comparative suffix/form of the adjective.

We now see that only the reconstructed ablative case construction contains the MOTIVATION for the development in the dialects. This construction had TWO MARKERS for comparison of inequality, i.e. the comparative suffix **-yes-* and the ablative case ending. The development in the dialects represents either the retention of the original construction or the loss of one the MARKERS. The loss of the comparative suffix as the MARKER of comparison of inequality is attested in Hittite, Tocharian, Armenian, Indic etc. The loss of the ablative case ending as the MARKER of comparison of inequality is attested in those languages which develop the particle construction and retain the comparative suffix. This development can be explained as follows. As the ablative case ending was being given up in these languages (as the MARKER of comparison) a new construction type was developed which was modeled on the juxtapositional construction. Two clauses were then needed in which the ADJECTIVE was expressed in the first clause, the STANDARD in the second and both clauses were in turn connected by a conjunction, i.e. the PIVOT or particle:

(The rug is) *longer than* (*then)¹⁶ *the table* (is wide).¹⁷

5. The following points should now be emphasized:

- (a) The data from the different dialects should be collected and

then evaluated before being used for the purpose of reconstruction. Elements which can be shown to be innovations in the specific dialects should not be used in the reconstruction.

(b) Candidates for the reconstruction should exhibit: (genetically) identical **FORM**, **FUNCTION** and **CONSTRUCTION**.

(c) Constructions which exhibit identical **FUNCTION** and **CONSTRUCTION** but different **FORMS** should be regarded as candidates for parallel development. But even in these cases where there is widespread parallel development our reconstructed state should contain the **MOTIVATION** for this development.

6. I would now like to turn our attention to a type of comparative construction which has been neglected in most studies on Indo-European comparison, i.e. the verbal construction of the form:

(X) *surpasses Y through size.*

What is offered here is only a preliminary investigation of this syntagma in some dialects and a provisional reconstruction.

Jensen, in his discussion of comparison in different languages, makes the remark that this verbal construction is widely used in languages of the *Naturvölker*¹⁸ and consequently gives only examples from these languages. This construction, however, is by no means restricted to these languages nor unknown in the Indo-European languages as the following examples show:

- (a) English: He surpassed all his contemporaries in skill.
- (b) German: *Er übertrifft ihn an Körperkraft.*
- (c) Greek: *sè peri frénas émmenai állōn*
 you sur- in reason -pass others
 ‘(that) you surpass the others in understanding.’
 (Homer Il. 17, 171)
- (d) Old Indic: *ná mâyābhir dhanadām paryābhūvan.*
 no tricks the booty giving they-have surpassed
 ‘They have not surpassed the booty giving (Indra) in
 tricks.’ (RV I, 33, 10)

From the data collected from these and other examples I would like to deduce the following provisional reconstruction:

Preverb-**VERB**, **STANDARD** (acc.), **INSTRUMENTAL**, i.e.:

- (i) a *preverb* with the meaning “over” or “before”,¹⁹
- (ii) a neutral **VERB** - most likely the copula “to be” - which together with the preverb expressed the idea of “surpassing”,²⁰
- (iii) a **STANDARD** in the (unmarked) accusative case, and
- (iv) a **QUALITY** being measured or referred to in the comparison set in the **INSTRUMENTAL** of means case.

Let us now recall the fact that an **INSTRUMENTAL** case should also be used with the case construction²¹ as exemplified by the following example from Homer:

Il. 15, 181: *séo biēi férteros*
 than you through strength better
 ‘better than you through strength.’

If we now compare both constructions as they have been reconstructed, we notice some very interesting parallel structural entities:

Verbal: **VERB** + preverb, **STANDARD** (acc), **INSTRUMENTAL**

Nominal: **ADJECTIVE** + **-yes-*, **STANDARD** (abl), (**INSTRUMENTAL**)

namely:

- (i) the use of the **INSTRUMENTAL** for the **QUALITY** in both constructions,
- (ii) the **VERB** and **ADJECTIVE** parallel each other and each have a **MARKER**, i.e. the preverb/**-yes-*,
- (iii) the **STANDARD** has an additional **MARKER** (i.e. the ablative case ending) only in the nominal construction, in the verbal construction it is unmarked in the accusative case, and
- (iv) functionally the preverb parallels the comparative suffix **-yes-*.

In Indic, Greek and other dialects we find the following development of the verbal construction: the **QUALITY** which was expressed through the **INSTRUMENTAL** case is incorporated into the verb itself:

- (a’) Old Indic: RV VIII, 1, 33
ádha plāyogir áti dāsat anyán
 now son of Playoga over gave others
 ‘Now the son of Playoga gave more than the others.’
- (b’) Greek: Homer Il. 13, 728:

(*ethéleis*) *periídmenai álōn*
 (you want) to know more from others
 '(You want) to know more than others.'

In addition to this type of verbal construction many Indo-European languages use another type of verbal comparative construction of the form:

*I prefer X to Y.*²²

I will not say anything about this type of construction here.

7. To sum up, we have reconstructed only one nominal comparative construction for PIE which contained an **ADJECTIVE** with a comparative suffix **-yes-* and a **STANDARD** in the ablative case which could be expanded through an additional **QUALITY** set in the **INSTRUMENTAL** case.

Since the reconstructed syntagma was doubly marked - **-yes-* and the ablative case ending - it contains the **MOTIVATION** for the later development in the various dialects. In addition to this construction there may have been a verbal construction containing a preverb, a **VERB**, a **STANDARD** in the (unmarked) accusative case and an **INSTRUMENTAL** indicating the **QUALITY** referred to in the comparison.

NOTES

- (1) See Jensen 1934; Panagl 1973: 361; Puhvel 1973; and Andersen in preparation: Chapter Three.
- (2) A special comparative form of the adjective is found in Indo-European, some parts of Finno-Ugric, Semitic (i.e. Arabic), Caucasian and a few other languages see Jensen 1934: 113; Friš 1950: 170; Berg 1958: 202, 215; and Puhvel 1973: 146.
- (3) Cf. the example from Mosquito:
Jan almuk, Sammuel almuk apia.
 John old Sammuel old not
 'John is older than Sammuel.' See Jensen 1934: 117; Ziemer 1884: 10; cf. Friš 1950: 180.
- (4) Cf. RV X, 60, 12: *ayám me hásto bhágavān ayám me bhágavattaraḥ*. Two different hands are involved here - cf. RV X, 137, 7.
- (5) See Puhvel 1973: 146; Melchert 1977: 215; cf. Friedrich 1960: 127. See Ziemer 1884: 137 for an example from Greenlandic with an ablative and a positive adjective.
- (6) I am referring here to remarks like: "The absence of markers of comparison in Hittite and the variation in markers from dialects to dialect and within dialects like Greek indicate that comparison was late ..." Lehmann 1974: 202; cf. *ibid*: 208; Lehmann 1972: 984.
- (7) "Moreover, from other passages in the Rigveda we may conclude that, at an earlier time, comparison was indicated alone by the position of the standard and by the pivot, with no comparative suffix on the adjective ..." Lehmann 1973: 56; see also Lehmann 1972: 985. See also Berg 1958: 217.
- (8) Lehmann's only example from the Rigveda is RV VIII, 76, 12. But in this example the ablative *indrāt* is dependent upon *pāri* - see Delbrück 1884: 460 - which in turn belongs to the verb *mame*. I fail to see an explicit comparison in this case, but it seems that this misinterpretation (i.e. a comparative) goes back as far as Sāyaṇa. My own investigation of comparison in Old Indic - see Andersen: in preparation - reveals no examples for this type without a comparative adjective. The further development of this construction, however, clearly shows that the positive takes over the function of the comparative already in Middle Indic. In Modern Indic the positive prevails. See Berg 1958: 217 and Friš 1953: 103.
- (9) See Szemerényi 1970: 178ff., esp. 183.

- (10) *ibid.*: 183.
- (11) See Benveniste 1948: 115ff., esp. 140ff.; Mørland 1933; Panagl 1973: 362; Puhvel 1973: 146ff.; Berg 1958: 205, 213, 217. See also Szemerényi 1970: 182f.
- (12) The particle in the comparative constructions were originally conjunctions, see Small 1924: 30.
- (13) The particle in Pāli is *yañ ca*, *yañ ce*, see Rhys Davids and Stede 1921-1925: 544.
- (14) The particle in the Brāhmaṇas is *yād (u) ca*, see Delbrück 1888: 196.
- (15) See Jespersen 1924: 246; Jensen 1934: 124; Puhvel 1973: 151; and Berg 1958: 219f.
- (16) The particle here represents a development from the Germanic conjunction **pan (nai)*, see Berg 1958: 219. See also Wessén 1965: 313-314.
- (17) Constructions such as “You are taller than *me*” show that a further development from a particle to a prepositional construction has taken place in English.
- (18) Jensen 1934: 120.
- (19) Cf.; Old Indic *atí* (+ *aś-*, *as-*, *kar-*, *dā-*, *dās-*, etc.), *abhí* (+ *as-*, *bhū-*, *jan-*), *ud* (+ *ric-*), *adhí* (+ *as-*), *pāri* (+ *ar-*), *prā* (+ *as-*, *arh-*, *ric-*) and *atí + prā* (+ *sthā-*); Latin *ante* (-cello, -cedere), *prae* (-stare); German: *über* (-treffen), Greek (Homer): *perí* (bállō, gígnomai, eimí, oída, títhēmi). *hýper* (bállō, echō), *pro* (bállō).
- (20) Cf. **es-* in Indic, Greek, etc., but also ‘to hit’ in Germanic and Greek.
- (21) Cf. Delbrück 1893: 270-271; Brugmann 1904: 430.
- (22) Cf.: German: *Ich ziehe X dem Y vor.*; Latin *prae/ante + ponere/ferre alqm. alci*; Old Indic: *vr̥ṇīti* acc., abl. (cf. RV III, 39, 7; VII, 33, 2), Greek: *pro-/anthetaireísthai tí tinōs*.

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