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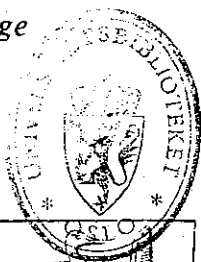
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## The Linguistic Cycle<sup>1</sup>

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*Dass man in glottogonischen Spekulationen sehr leicht irren kann, dessen bin ich mir vollauf bewusst (Hirt 1904-05: 37).*

"Now, it is often said that the history of language shows a sort of gyration or movement in spirals, in which synthesis is followed by analysis, this by a new synthesis (flexion), and this again by analysis, and so forth. . . . But this pretended law of rotation is only arrived at by considering a comparatively small number of phenomena, and not by viewing the successive stages of the same language as wholes and drawing general inferences as to their typically distinctive characters. . . ."

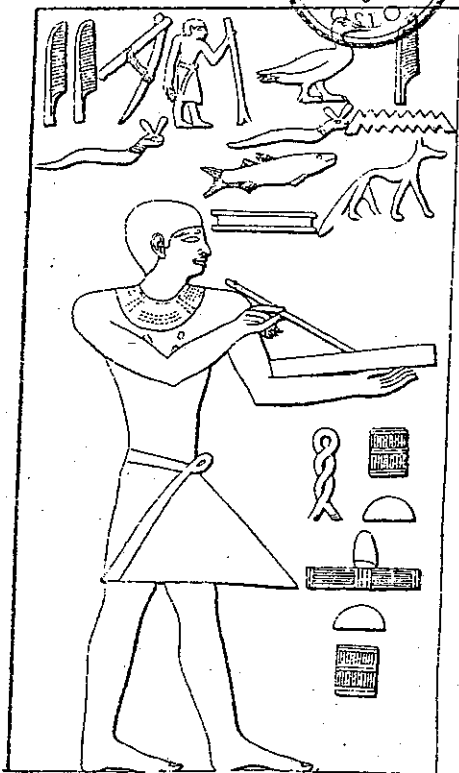
So Jespersen in 1922 (424-25), denying the validity of the theory that we shall here call the 'Linguistic Cycle'. This hypothesis arose from speculation concerning the origin of the inflectional forms of Sanskrit, Greek, Latin, etc. Many scholars came to the conclusion that this morphology was based on earlier syntactic constructions—groups of independent words, and this encouraged the belief that there was an alternating series of

linguistic stages. They observed that present-day Indo-European languages were predominantly syntactic (here abbreviated Sm), while the classical languages were characterized by a complex morphology (sM). The presumed earlier stage, where this morphology was formed, would be predominantly syntactic (Sm). It took little for someone to assume that this cycle Sm to sM to Sm was part of a repeating alternation between Sm and sM. This may be roughly represented as follows:

Purely hypothetical pre-pIE	1	*Sm
"	"	"
"	2	*sM
Proto-Indo-European		*Sm
Classical Languages		sM
Present-day English, etc.		Sm

This series would be without known beginning and would presumably lead to another \*sM stage were present languages to survive. (The representation by Sm, sM is my own and is meant to imply that both syntax and morphology are present at all times in this history.)

This view, Jespersen argued, was unsupported by the evidence of any



Old Egyptian (after Davies)

known linguistic history. Those histories that were known largely demonstrated only one direction—from the heavily morphologic to the predominantly syntactic. That is, known history represented only the last sM and Sm above. All others were based on inference (and are therefore starred). His own interpretation of the data was that the known stages were representative of the general trend and that the preceding stage must have been even more complex morphologically:

“The direction of movement is towards flexionless languages (such as Chinese, or to a certain extent Modern English) with freely combinable elements; the starting-point was flexional languages (such as Latin or Greek); at a still earlier stage we must suppose a language in which a verbal form might indicate not only six things, like *cantavisset*, but a still larger number. . . . Primitive linguistic units must have been much more complicated in point of meaning, as well as much longer in point of sound, than those with which we are most familiar” (1922:425).

This view may be schematically rendered:

With very complex morphology	*sCM
With complex morphology	sM
Predominantly syntactic	Sm

Jespersen overstates the case of his opponents to some degree, as most of them did not go beyond the immediately preceding stage, arguing, one might say, for a sequence \*Sm sM Sm, very comparable to his own as regards the conjectured time depth. It would appear that the sequence itself (Sm sM Sm) was not what bothered Jespersen the most but the over-all concept of linguistic disintegration held by many of its proponents. Writing much later he says in retrospect: “When I began writing on language, the prevalent theory was this: language had begun with inflexible roots, some of these in course of time became subordinate grammatical implements which were first agglutinated to and eventually fused with the more substantial elements. In this way was achieved the development of inflexional languages such as primitive Aryan (Indo-European, exemplified in

Sanskrit, Greek and Latin); here the high-water mark was attained, and since then we witness only decay, degeneracy, and destruction of the beautiful structures of these old languages. To this I objected, trying to show that viewed from the point of view of human energetics so far from being retrogressive the tendency in historical times has on the whole been a progressive one” (1941: 7).

We must, of course, dissociate both of these hypotheses from the decay motif. Indeed, one would think Jespersen's view more subject to such misinterpretation than the other, but there is no necessary connection of either with a pejorative development. He is, of course, correct in describing the general attitude of the historical linguist of the nineteenth century—and even, unfortunately, later. The odor of decay is almost as real as the musty smell of the books themselves; it is all too frequent a metaphor. It was, however, going out of fashion long before 1922. Lounsbury writes in 1904: “Every language, it was supposed. . . had its period of birth, of growth, and of maturity. Then followed the inevitable decay. . . . Undoubtedly traces of this belief still linger among us: but in general it meets no longer with acceptance” (1906: 3-4).

While Jespersen's critique of the ‘spiral’ theory has been allowed to stand, has, in fact, been given little thought, his concept of linguistic change has been sharply challenged. Hakulinen points out (1961: 68) that Finno-Ugric languages have increased their number of cases, contrary to the usual Indo-European pattern. Proto-Finno-Ugric is thought to have had some five cases; of the modern languages, Lapp has eight, Mordvin ten, Cheremis thirteen, Zyryan eight, Votyak fourteen, Vogul six, Ostyak six to eight, Hungarian twenty-one. Even where Indo-European has retained case, it has not increased the number (pIE seven, Lithuanian seven, German four, Russian six). He concludes: “There is therefore no support in Finnish and its related languages for the theory that the development of all languages generally tends towards greater analysis in structure”

(1961: 67).

Ravila speaks directly to the same point, saying, “wir haben unanfechtbare Beispiele für die geradezu grundlegende Rolle der Synthese in der Entwicklung der Sprache” (1941: 122), and “Die Entstehung der grammatischen Bestimmungsverhältnisse ist ebenfalls ein Resultat synthetischer Entwicklung” (123). He also assumes, in discussing the nature of some proto-Uralic disyllabic roots, that these were the result of some presently irrecoverable mergers of separate morphemes; that is, he posits something characteristic of an \*Sm stage. There is therefore clear evidence for a sequence:

predominantly syntactic	Sm
having complex morphology	sM

The Altaic languages are another example of this same direction (Poppe 1965: 177-196), not to mention that there are close relatives such as Polish in which there is considerable observable development of morphology.

Ravila and Hakulinen are in this respect in harmony with the thinking of many Indo-Europeanists from Bopp to the present day. One must not consider all of these to be advocates of the Linguistic Cycle. Not at all. It is rather that their interpretation of the origin of Indo-European and Indo-Hittite inflectional forms is the one that fits this hypothesis.

Although not without predecessors in such speculation (e.g., Tooke; see Wheeler 1906: 21, Robins 1968: 156-57), Bopp was the major figure in promulgating the idea that inflections arose from the compounding of roots and ‘auxiliaries’ (Robins 1968: 173-174); cf. Lehmann 1967: 43; *in extenso* Bopp 1856). Bopp's view of linguistic development has suffered from the early date of his appearance in linguistic history. Since he wrote before the establishment of regular sound correspondences, his equations of morphologic forms in different languages were often wrong, and we find many recent disparaging remarks about his work. This is not only unfair on principle, it has led to less than appreciation of his contributions.

Nevertheless, we find that Bopp has had some illustrious intellectual descendants. The notion that one man's morphology was an earlier man's syntax did not die out. Brugmann, for example, says: "From the principles laid down... we must assume that forms with a case-suffix... are compounds which were once phrases" (1892: 59), and "The forms of the Finite Verb grew out of the connexion of subject and predicate. In the parent language, phrases made up of a word denoting some condition or action and a personal pronoun, used as a sentence in which the latter was subject and the former predicate, coalesced, and became a single word; this is the origin of all the finite verb-forms" (1895: 3-4).

In the next linguistic generation, the Indo-Europeanist Hirt fully appreciated Bopp. Recognizing his inherent faults, he saw through to the ideas behind them. In 1904-05 he published his own theory of inflectional origins along the same line. Two of his points, as summarized by Oertel, are: "that the inflectional system of the Indo-European languages was preceded by an inflectionless period, traces of which are not at all rare in the historical forms. Second, that the distinction of verbal and nominal inflection is not original and that the whole sentence-architecture of the Indo-European, with its characteristic division into subject and verbal predicate, is a secondary growth" (1906: 61). That Hirt did not abandon this basic concept may be seen from his later work (e.g., 1927.1: 5; 1927.3: 1-4, 1934.6: 5, 177-178).<sup>2</sup>

We have had, then, up to our own times, scholars who have accepted and developed Bopp's fundamental thesis, that Indo-European morphology was based on earlier syntactic constructions. At the same time we have had no recent supporters of the Linguistic Cycle as such, barring the present writer.

Jespersen's challenge to produce the cycle within the history of a single language has not been met within Indo-Hittite. It would seem logical to assume that the study of Hittite should throw at least some light on the problem. Considering the age of the documents, this language might be

expected to correspond to the era of conjecture represented by \*Sm and \*sCM:—

The Age of	Bopp	Jespersen
Hittite	*Sm	*sCM
Sanskrit	sM	sM
Hindi	Sm	Sm

Hittite has, as a matter of fact, a comparatively light morphology. "For example, the Hittite verbal system is remarkably simple while the ancient IE languages agree in presenting a very complex system of moods and tenses" (Sturtevant 1933: 30-31). It does not show the actual free form syntactic structure envisaged by Bopp, but it is very much in contradiction to Jespersen's view. Friedrich, with reference to the Hittite use of auxiliaries, says, "Das Hethitisches hat auch mehrere zusammengesetzte Verbalformen, die der Sprache ein seltsam modernes Gepräge geben" (1960: 111; cf. 137).

The two most probable interpretations are: 1) Hittite has lost the complexity of the proto-language; 2) Hittite represents an earlier stage, which did not have this complexity. Both of these have their supporters among recent scholars.<sup>3</sup> It is easy to take a stand on one side or the other. It is less easy to give cogent reasons within Indo-Hittite why one's stand is the more probable.

*Will man die indogermanische Nominalflexion verstehen lernen, so muss man sich zu Sprachen wenden, in denen wir die Entstehung der Flexion sozusagen noch beobachten können (1904-05: 40).*

Hirt, in arguing for the syntactic origin of Indo-European inflections, used Yakut, a Turkic language, to support his case (1904-05). As noted above, both Uralic and Altaic show the development of morphology from syntactic structures. However, although they give us a reverse direction (Sm to sM), they do not furnish a complete cycle. We need to look elsewhere for the "successive stages of the same language" so reasonably requested by Jespersen. A language attested at least as long as the Indo-Hittite family seems called for. Only two languages

have histories that approach or surpass in length that of Indo-Hittite.

One, Chinese, approaches it. Here, because of the writing system, the earliest forms must be largely reconstructed. Despite the uncertainty inherent in such a process, there is good evidence that early Chinese had a greater degree of morphology than does classical Chinese (cf. Jaxontov 1965: 36-37). Some have even argued that it had a case system, but this appears to go beyond reasonable inference (see Kennedy 1940: 196, fn. 7). Classical Chinese is without doubt predominantly syntactic, but Modern Chinese shows a shift to the use of more compounds and other bound forms (Kennedy 1951). We have, then, a history that may be characterized as one from 1) some morphology to 2) little morphology to 3) some morphology. To describe it as comparable to Indo-Hittite would be to go far beyond the evidence as we now see it. Nevertheless, it does illustrate a complete cycle, beginning and ending with a heavier morphologic stage.

There is one linguistic history that is longer than that of either Indo-Hittite or Chinese. As fate would have it, even as Jespersen wrote his strongest argument against the theory of the Linguistic Cycle—the fact that such a phenomenon was unattested—the needed example was known to the scholarly world: the history of Egyptian. Unfortunately, this history had not yet been conveniently summarized and made available. Sethe's *Verbum* (1899-1902), of the greatest importance in this respect, cannot readily be used by the non-Egyptologist.

The stages of the Egyptian language as now recognized are Archaic Egyptian, Old Egyptian, Middle Egyptian, Late Egyptian, Demotic, and Coptic (cf. Gardiner 1957: 5; chart: Hodge 1970: 38; cf. those in 1968: 14, 16; sketches of OEG, MEg, LEg, C: Kees 1959: 62-104). There are a number of discussions of this history, but no over-all detailed presentation (see Korostovcev 1963: 192-225, with bibliography, 241-42). Hintze has an interesting discussion of the direction of linguistic change in Egyptian (1947), describing the position of determiner and determined as reversing through

time. It is Korostovcev, however, who gives us a clear statement on the question at hand. In his section on the "General tendency of the development of the Egyptian language", he says, "...the Old Egyptian synthetic verb of the suffix conjugation changed into the analytic Late Egyptian verb, and finally into the Coptic synthetic verb of the prefix conjugation" (1963: 225 tr.). The following discussion endeavors to make clear the kind of evidence we have for this interpretation, without going into great detail.

It is to be understood that Egyptian developed in the gradual manner characteristic of language. The six 'periods' or stages into which the history has been divided are the result of the work of scribal schools ('education') as affected by the changing political situation. Different dialects existed at all times, and several are reflected in the extant documents. It was thus a normal language, the stages as reflected in writing being normative.<sup>4</sup>

The approach is simple: contrast the oldest period with the latest period, then examine the intermediate stages for evidence on the structure of the transitional linguistic type. The two major morphological categories throughout the history are noun and verb. Although both are relevant to our thesis, only the verb will be used to exemplify the changes. This is, I feel, sufficient for our purpose. (The many other aspects of this history that need explication will hopefully be taken up elsewhere.) As the extant records of Archaic Egyptian are insufficient for our purpose, Old Egyptian will be contrasted with Coptic.

Edel lists twenty-three "verb forms" for Old Egyptian (1955: 4\*). As the vowels are unwritten the precise number is uncertain; twenty-three is more likely an underestimation (cf. Polotsky 1944: 93, who gives nine suffix forms to Gardiner's six, where Edel has five). Fourteen of these forms

have personal suffixes (fifteen if the imperative is included), five are particles, and two or three are verbal nouns.<sup>5</sup>

Only one of the forms, the Old Perfective, has personal suffixes that are also present with a so-called nominal subject. The others (Edel's "Younger Inflection," Gardiner's "Suffix Conjugation") have either a noun subject or a personal pronoun.<sup>6</sup> Seven of them may have a "passive" suffix, increasing the total number (by Edel's count) of forms inflected for person to twenty-one. One may, I think, safely categorize the system as one of fair morphologic complexity.

The situation is similar in Coptic. Polotsky distinguishes eight "basic tenses" (four affirmative, four negative), and twenty-six "satellite" constructions, with four additional satellites based on other satellites (1960: 400). This is indeed a complex system. Not a single one of these forms may be equated with any of the forms noted above for Old Egyptian. In Old Egyptian the personal suffixes followed the verb root; in all the above Coptic forms they precede it. There are some "adjective verbs" in Coptic that take suffixes in the Old Egyptian manner, but it is not certain that they are remnants of the older conjugation system (Polotsky 1960: 412). There are some fossilized fragments of the latter, but they are just that (Steindorff 1951: 135-36). The form of the verb used in the basic tenses and satellites of Coptic is the infinitive of the older periods.

One of the earlier verb forms, the Old Perfective, survived into Coptic. In Old Egyptian it has, as noted above, inflectional endings; in Coptic these are completely lost.

This situation can hardly be described as simplification. (Jespersen, it may be noted, included Egyptian to Coptic among his examples of the complex to the simple (i.e., of sM to

Sm; 1922: 365).) The Coptic picture would appear to be just as complex as the older one. The interesting aspect is the almost total loss of the earlier inflected forms—with suffixes—and the later appearance of inflected forms with 'prefixes' (or noun subjects in that position). While there is still room for discussion concerning details, there is no question about the manner in which the later forms arose: they result from syntactic constructions utilizing 'auxiliaries'. These latter may be verbs or particles. For example, the verb 'to do' is so used from Old Egyptian on, as is the verb 'to be'. A Middle Egyptian example may be given, illustrating 'to do' as an auxiliary in the *sḡm-xr-f* form (a suffix verb with formative *xr*): *'rxrk w3h-tp m X* 'then you (-k) are to (*xr*) do (*r*) multiplying by (*m*) X'. Just as in present-day English, the auxiliary has the inflection. The verb 'to be' is used in many different constructions, e.g., *wnn 'r'(w) mytt r 'št-žn 'n ('m(y)-xt-žn* 'the like (*mytt*) will be (*wnn*) done (*'r'w*) to your property by those [who come] after you'. The uninflected particle *w* is frequently used, and constructions such as *'wf ḥr sḡm* 'he is upon hearing' = 'he hears', and *'wf r sḡm* 'he is toward hearing' = 'he will hear' arise in the course of time. Syntactic combinations such as these lie behind the Coptic verbal system (see Till 1961: 153-168, where older Egyptian origins of forms are noted; for numerous examples of auxiliaries in the older language see Edel 1955-64, Gardiner 1957 passim).

A good example of the syntactic origin of a Coptic verb form is that of the so-called 'Finalis': *tarefsōtām*. The 'conjugation base' is *tare-*, the subject slot is filled by a pronoun (*f*) or a noun, and the infinitive takes third place (type verb: *sōtām*; see Polotsky 1960: 393). Polotsky has shown that *ta-* is from *d'* 'I give', i.e.,

Late Egyptian d'Orbiney 6.5 (after Möller)

'I cause', and *-re-* from 'r' 'do' (1944: 1-19). In Coptic, the 'I' is completely lost, and the form is used in such a sentence as *sbte pekšag'e tarousotmef* 'prepare (*sbte*) your (*k*) speech (*šag'e*) and the result will be that (*tar-*) they (*ou*) hear (*sotm-*) it (*-ef*)' (RSV 'Prepare what to say and thus you will be heard', Sir. 33.4a; example from Polotsky 1944: 4, who gives the Greek: *hetoimason lōgon kai hoútōs akous-thēse*).

The entire set of Coptic forms with subject plus infinitive are derivatives of like syntactic constructions, some traceable to Old Egyptian, others to later stages of the language, though not all origins are known or certain. We have, then, to consider the intermediate periods of the language. As one would expect, Middle Egyptian is close to Old Egyptian while Demotic largely uses the auxiliary type of construction. Late Egyptian would be a kind of median. It is therefore of particular interest to read what the standard grammar of Late Egyptian has to say about it. Just as Friedrich sees in the Hittite use of the auxiliary 'have' something curiously modern (1960: 14), so Erman in his characterization of Late Egyptian compares it to our own times:

"Es ist das die Stufe, die uns aus unsern modernen Kultursprachen vertraut ist, die Stufe, wo die Substantiva sich mit Artikeln behelfen und wo die Verben ihre Flexion durch Hilfsverben ersetzen. . . Bei den Verben existieren zwar noch einzelne flektierte Formen, aber sie sind schon ein Ballast, und statt 'er hört' braucht man schon 'er ist beim Hören', 'er tut hören' u.s.w. Im Koptischen ist die Entwicklung dann noch weiter gegangen, auch die letzten Reste der alten Flexion sind verloren und die Hilfsverben beherrschen als 'Präfixe' allein das Verbum" (1933: 5).

We may then interpret Late Egyptian as a predominantly syntactic stage of the language, as did Korostovcev. Our cycle is complete:

Old Egyptian	sM
Late Egyptian	Sm
Coptic	sM

Despite the lack of vowels in hieroglyphs, the consonants are well enough understood for Egyptian to furnish a much better documented and clearer example of a linguistic cycle than does Chinese. Unfortunately, the language is dead, and we cannot prove that the next stage would have been a predominantly syntactic one.

We may, however, be forgiven for speculating on pre-Egyptian or the proto-Afroasiatic stage. If the Old Egyptian verb morphology is inherited, it should be relatable to some of the other Afroasiatic languages. Aside from the imperative, only one inflected form is to be found elsewhere: the Old Perfective. That this is basically the same form as the Akkadian permanent has been recognized from the very first (Eg. *šmk?* 'I in a state of hearing'; Akk. *parsāku* 'I in a state of being divided').<sup>7</sup> None of the other forms is to be found elsewhere in Afroasiatic. Their secondary origin within Egyptian is generally admitted. Even should related forms be found, they would demonstrate closer relation of that language to Egyptian; they would not indicate that the forms were common Afroasiatic.<sup>8</sup> This would argue that proto-Afroasiatic was a period of flexibility, that is, a predominantly syntactic stage that would

give rise to the variant morphologies to be found in the daughter branches. We may therefore preface our history with a probable \*Sm:

proto-Afroasiatic	*Sm
Old Egyptian	sM
Late Egyptian	Sm
Coptic	sM

*Die Hypothese erfüllt alles, was man von einer Hypothese erwarten darf, sie erklärt sämtliche Erscheinungen (Hirt 1904-05: 44).*

One cannot assume that because Egyptian has such a history and the data from Indo-Hittite, Uralic and Altaic are compatible, we may conclude that the linguistic cycle is a universal phenomenon. We can, however, say that it may serve not only as a reasonable hypothesis but that there are many lessons to be learned from the manner in which syntax and morphology are seen to interact. It is impossible to test this theory with most languages of the world. It is possible, however, to examine those histories we have to see whether they fit this pattern or whether we should look for another.

The implications of this interaction are far-reaching. At no stage do we have the isolating purely syntactic stage apparently envisaged by Bopp. We have, however, vindicated his belief that morphology is the result of syntactic constructions. Some of the elements survive thousands of years relatively unchanged (e.g., the morpheme *k* for 'you (m.sg.)', used above). Sometimes they are associated with the same semantic motifs (as *k*), sometimes not (as *ta-*, above). The survival of the motifs themselves and their reassignment to forms is, of course, another facet to be studied in this new context. Polotsky sees in one such history a cyclic phenomenon: "L'histoire du Parfait Second présente un exemple intéressant d'évolution 'en spirale': très semblables en égyptien classique, la forme 'emphatique' et la forme relative, après s'être écartées l'une de l'autre pour quinze siècles, finissent par se rejoindre sous un aspect changé mais équivalent" (1944: 96).

TE PET-ΘΟΥ ΝΕΙ  
 ΟΥΩΨ ΜΝ ΠΕ ΕΤΑ  
 ΨΕ ΠΨΑΧΕ ΕΠΕ  
 ΖΟΥΟ ΕΤΒΕ ΝΑΪ  
 ΑΛΛΑ †COOYN ΧΕ  
 ΝΑΝΟΥ ΠΨΙ ΖΝ ΖΩΒ  
 ΝΙΜ ΜΑΙCΤΑ ΧΕ  
 Α ΠΝΑΥ ΨΩΠΕ Ν  
 ΤΕΝΕΠΙΤΕΛΙ ΝΤΕ

Coptic (after Worrell)

As others have previously conjectured, language typology may be in some respects the description of the stage of development vis-à-vis the cycle rather than the basis for more fundamental dichotomies.

There is much more to be said: The morphology is frozen syntax, sooner or later doomed to be replaced. There is, however, a syntactic component that remains and that creates new morphology. What is the nature of this 'constant'? Are pidgins and creoles the utilization of this constant in both (or all) the languages involved, with minimum morphologic carry-over?

Another implication is that during the course of such a development, a given morphologic structure will vary in its segmentability. Originally free forms, or largely free forms, the chromorphs are still readily identifiable in the next stage.<sup>9</sup> They remain easily segmentable (as in Turkish) for some time, but then become, with phonologic change, much less subject to analysis (cf. Sanskrit and Latin, the former being more easily segmentable than the latter, though much of it is certainly not easily analyzable; cf. Burrow 1955: 117-218 on noun formation). The final stage of complex morphology includes much that is totally obscure unless the history is known. This obscurity pertains only to the chromorph itself, as the semantic motifs 'lost' by the form would normally have been kept (not restored) by the syntax (cf. Polotsky as quoted above 1944.96).

The study of auxiliaries and periphrases is, of course, central to this topic. Their use in language change has been noted repeatedly. It is now time to reexamine them as part of the syntactic constant. In other words, even the period of greatest morphologic complexity must contain the possibility of constructing clauses that would serve as the models for auxiliary formations.<sup>10</sup>

*Ob aber der Weg, den ich eingeschlagen, und der mir einigermaßen vertrauenswürdig erscheint, überhaupt der richtig ist, dass muss ich abwarten (Hirt 1904-05: 84).*

## NOTES

1. I should like with this publication to pay tribute to my first teacher of Egyptian, the late William F. Edgerton, with whom I studied Middle Egyptian and the History of Egyptian at the 1939 Linguistic Institute. It was, to Professor Edgerton's knowledge, the first time that a course had been given in the history of the language. I cannot pretend to reach his standards; I can only record my gratitude for his teaching, which has remained a constant inspiration.

This is the third publication in which this subject is broached by the present writer. The other two are briefer expositions included in reviews (of *Studies in Egyptology and Linguistics*, forthcoming in *Language*, and of Faulkner, *The Ancient Egyptian Pyramid Texts*, forthcoming in *SIL*). A related paper is "Egyptian and survival," presented at the Colloquium on Hamito-Semitic, School of Oriental and African Studies, London, March 1970, and forthcoming in the proceedings. Only the broad outlines have been sketched here and in the reviews mentioned. The writer intends to investigate the subject much more thoroughly over the next several years. This much will at least alert linguists to the primary importance of the syntactic-morphologic interchange in historical linguistics.

2. For a brief recent statement on the origin of the personal endings of the verb, see Burrow 1955: 316-18. The syntactic origin is no longer upheld, but the basic similarities between nominal and verbal affixes is reaffirmed.

3. Benveniste and Burrow may be taken as recent representatives of the two views: "Le hittite a constitué son système de présents en réduisant fortement la variété des types représentés dans les états anciens de l'indo-européen et en spécialisant ceux qu'il a gardés dans des fonctions définies" (Benveniste 1962: 20). "It appears that originally Indo-European distinguished in the indicative simply between the present and preterite. . . This state of affairs is continued in Hittite. . ." (Burrow 1955: 296).

Interestingly, the situation that would allow the second interpretation had already been conjectured for proto-Indo-European before Hittite had rejoined the family: "If, as seems incontestable, the tense system of the Indo-European languages is by no means primitive, but a secondary structure, into which material of a previous period was built by charging

old forms with new meaning, it is not necessary to assume that this new system was uniformly worked out in what is called Indo-European times. . ." (Oertel 1906: 57).

4. The hypothesis that Egyptian is a Mischsprache, arising from the confrontation of Semitic with some unknown African language is not considered here. It is irrelevant to our present thesis, unless one were to maintain that cycles were only characteristic of creoles, or that change as drastic as is evidenced could only occur with the intrusion of an alien element.

5. No account is here taken of other morphologic processes, such as the causative prefix *s* etc. (see Edel 1955: 183 ff.).

6. Note Satzinger's stress of this point (1968: 162 fn. 3).

7. See Erman 1889: 17, where he compares the Semitic perfect (hence the Egyptian name Old Perfective) and also refers to the Akkadian form in *-ku* (now known as the permansive).

8. Satzinger (1968) treats a formal Amharic parallel to the Egyptian suffix form. This is not an effort to establish the two forms as descended from a common pAAs one.

9. The term chromorph has been coined to designate a form which is historically recognizable (with the aid of historical linguistics, if necessary) as the 'same'.

10. For a recent discussion of 'auxiliation' in language see Benveniste 1968.

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## The Second International Congress of Mongolists

Ulan Bator, September 2-11, 1970

John R. Krueger

During a previous trip to Mongolia in 1968, I learned from the Academy of Sciences their intention to hold the Second Mongolist Congress in 1970. The first Congress was held in 1959 (for a report on the participants and proceedings, see *Central Asiatic Journal* 5.233-249), which would lead one to expect the second ten years later, hence 1969, but it proved impossible to meet this deadline. As 1971 will mark the fiftieth anniversary of socialism in the Mongolian People's Republic, and will be the occasion for many other celebrations, that left only 1970 as a suitable year. About April of

1969, invitations were extended by an organizing committee to Mongolists in various countries. The United States received three official bids, to Prof. Cleaves of Harvard (who did not attend), my colleague Dr. John Hangin, and myself. In addition, general invitations were extended to many Mongolists and Asian scholars to attend at their own expense. For quite some time, we knew only that the Congress would take place during the first ten days of September, and would be followed by a brief study tour. The actual effective dates were not announced until about June, 1970.

I left Bloomington at noon on Monday, August 24th, in company with Dr. Hangin, who was proceeding directly to Moscow and Ulan Bator. After a few days in Copenhagen, I met the scheduled flight in Moscow, and arrived on Tuesday, September 1st, in Ulan Bator along with many other delegates.

About one hundred foreign delegates attended the Congress. In addition, dozens of Mongols with interest or background in language, literature, history, or economics took advantage of the occasion to be present at the sessions and listen to papers. Thus, the