

# What Changes in ‘Syntactic Change’? Some Implications for Syntactic Reconstruction

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*Dedicated to the memory of Calvert Watkins on the 50th anniversary of his foundational paper in Celtica VI*

## 0. Preliminaries

1. Much of the debate surrounding the issue of syntactic reconstruction seems to me to be confounded by a certain degree of terminological (and perhaps conceptual) imprecision. In today’s talk I would like to present some aspects of the problem, walking through how they look to someone who makes the set of assumptions that I make regarding how historical linguistics works, what syntax is, what change is, and what reconstruction is.
2. While, of course, all of the assumptions I make are correct and you should just adopt them at once, you may decide to persist in your own orientation to these matters. I hope that seeing how the matters play out under my assumptions may help us all to track the source, and implications, of any disagreements we may have, opening the door to somewhat clearer dialogue on the matter.
3. There are many aspects of this matter which are inordinately complex, some of which I couldn’t talk about no matter how much time I had (because I don’t understand them well enough), some of which cannot be meaningfully explored in the timeframe afforded this talk. I will focus today on a narrow topic (sometimes called ‘the correspondence problem’) and a very general question (is it likely that we will be able to reconstruct syntax in something like the sense that we regularly say we can successfully reconstruct phonology and relatively complex morphology?).
4. My musings on the latter question will obviously be connected to my discussion of ‘the correspondence problem’, but will not be fully grounded, there being many other aspects of the ultimate question (e.g., the ‘directionality’ issue) that I will not be able to treat in any meaningful way today.
5. As a final warning, I would ask you to pay attention to what I mean by the various terms I use (when I tell you), since it is unfortunately the case that I use many standard terms in what people tend to think (incorrectly, in my view, but that’s a very long discussion) are idiosyncratic ways.
6. Because I believe it to be an established fact that meaningful reconstruction can be done (and, of course, has been done) in the areas of phonology and both derivational and inflectional morphology (if, indeed, that contrast is meaningful), I won’t spend any time justifying them. If you have hesitations about these matters, I can give a quick 60 second demonstration of why they must be valid (based on their strong capacity for predicting previously unobserved data) in the question period.

## 1. The ‘Correspondence Problem’: A Definition

7. The notion of ‘corresponding’ linguistic elements, which determines what gets ‘compared’ in comparative linguistics, has, in my view, two rather different uses in the literature, not generally, or perhaps ever, contrasted. The uses correspond in some sense to different ‘steps’ in the analysis.
8. The first use can be seen in a table such as the one below, which I am confident you have all seen. It lists cognate forms in many languages; here I give a Polynesian example because of its extreme transparency (in the phonological domain).

Hawai‘ian	Maori	Samoan	Tongan	English
hua	hua	fua	fua	<i>fruit</i>
hulu	huru	fulu	fulu	<i>hair</i>
heʔe	meke	feʔe	feke	<i>octopus</i>
hiku	mitu	fitu	fitu	<i>seven</i>
haku	matu	fatu	fatu	<i>stone</i>
niho	niho	nifo	nifo	<i>tooth</i>
honu	honu	—	fonu	<i>turtle</i>
mahana	mahana	mafana	mafana	<i>warm</i>

9. Such data, revealing as they do systematic and pervasive correspondences across a set of languages, provide key evidence for the establishment of a ‘language family’, as you all know. Systematic correspondences whose nature is such that alternative explanations (borrowing, iconicity, chance) can be excluded license the assumption of genetic affiliation.
10. As you also all know, from such data one may reconstruct anterior linguistic objects, and, from the reconstructed forms of such objects, draw inferences about change events. For the data above and other data not presented one may reconstruct Proto-Polynesian *\*fua* ‘fruit’, *\*fulu* ‘hair’, *\*feke* ‘octopus’, *\*fitu* ‘seven’, *\*fatu* ‘stone’, *\*nifo* ‘tooth’, *\*fonu* ‘turtle’ and *\*mafana* ‘warm’. Hopefully you all already know this.
11. From these reconstructions, direct inferences about what must have happened in the history of the languages in the table above can be drawn. Since *\*fua* ‘fruit’ shows up as *hua* in Hawai‘ian, for example, it would appear that *\*f* has become Hawai‘ian *h* (perhaps, of course, via some intermediate developments). Since the *\*k* of Proto-Polynesian shows up as *ʔ* in the Samoan word for ‘octopus’ (and in hundreds of other cases of PPn *\*k*), we conclude that there has been a change of *\*k > ʔ*. And so on in the familiar fashion.
12. I go through these obvious and well-established procedures to introduce the second use of ‘correspondence’, which is a little bit (or maybe completely) non-standard. It concerns the Samoan word for ‘turtle’, which is not ‘—’ (whatever that means), but rather *laumei*. It seems fairly odd to say it this way, and no historical linguist ever does as far as I can see, but Samoan *laumei* corresponds to PPn *\*fonu* in a very direct sense, often exploited in historical work.
13. It corresponds to *\*fonu* in that from the fact that the Proto-Polynesian word for ‘turtle’ is *\*fonu* and the Samoan one is *laumei* we may infer a change event (just as we can from the fact that the ‘corresponding’ Hawai‘ian word is *honu*): the inherited word for ‘turtle’ underwent a lexical replacement (a change event) in Samoan. As far as I can see, it is only by bringing linguistic entities

into ‘correspondence’ that we can infer change events (otherwise, non-corresponding forms allow no inference: from the fact that the Proto-Polynesian word for ‘fruitbat’ was \*peka and the Hawai‘ian word for ‘vomit’ is *lua*, no change event can be posited).

14. Although comparativists do not talk about this kind of ‘correspondence’ very much, it is central to core aspects of their methodology. For example, as often noted, the best evidence for subgrouping within a language family is *shared morphological innovation*. But what does that look like, in terms of correspondence? When Indo-Iranian languages show the ‘shared morphological innovation’ of replacing the IE thematic genitive plural ending \*-ōm by \*-ānām<sup>1</sup>, we get a ‘turtle’ problem: in sense (1) above the correspondence set has ‘—’ in it for archaic Indo-Iranian languages, in sense (2) it has -ānām.
15. But if we don’t believe that this -ānām ‘corresponds’ in any relevant sense to PIE \*-ōm, then it doesn’t belong in the table at all, and no inferences about ‘change’ can be drawn (since the forms are not ‘in correspondence’), and there is no ‘shared morphological innovation,’ and, sadly, the entire method falls apart.
16. Since the method works (in my view), if we take a conceptual position that requires that it be true that it not work, we have made an error. Therefore, the forms must stand ‘in correspondence’.
17. So, under this second definition of ‘correspondence,’ two linguistic objects are ‘in correspondence’ if the history of one is to be accounted for with reference to the other. Indo-Iranian \*-ānām is ‘in correspondence’ with PIE \*-ōm because it is the descendant of \*-ōm which underwent morphological replacement in the shared morphological innovation that \*-ānām represents. Samoan *laumei* is ‘in correspondence’ with PPn \*fonu because it is the descendant of \*fonu that underwent the change event of ‘lexical replacement’ in the history of Samoan.
18. It is important to recognize (though this seems to be sometimes overlooked in the literature) that this same notion of ‘correspondence’ is required if we are to do historical work to uncover diachronic events which unfolded during the *attested* (rather than reconstructed) history of a language. This is not surprising, since the intellectual task is virtually identical: the question in both cases is ‘which forms do we draw from these two (or more) grammars to compare in trying to establish the history of the grammars under examination’?
19. When dealing with sister languages we want forms that correspond in the way Sanskrit -ānām corresponds to PIE \*-ōm (as well, of course, as cases of simple, non-replacive descent). When dealing with two grammars drawn from Stage I and Stage II of the same linguistic system, we want, again, to draw forms from the Stage II grammar whose properties show a diachronic dependence (through change events) on those we have taken from the Stage I grammar.
20. For example, in the later Vedic period, the Sanskrit genitive takes over more and more functions of the earlier dative. This is a change, and it is to be accounted for by bringing the dative functions of the earlier stage into correspondence with the genitive functions that they map to in the later stage, and developing a diachronic account of the established correspondence. All of this is quite standard.

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<sup>1</sup>The innovation could have preceded the \*ō>ā change, and thus might have been \*-ōnām; the point remains the same, of course.

## 2. 'Syntactic' Correspondence

21. The question that arises next is whether there is a limit to the kinds of linguistic object that can be brought into correspondence (in the second sense). Again, I note here that in this second sense is it *not correct* to say something like Samoan *laumei* is cognate with Hawai'ian *honu*. Cognancy, in the relevant sense, is intended to cover cases of *purely phonological* relationship—and *laumei* is not the *phonological* correspondent of *\*fonu*, but the lexical replacement thereof.
22. The syntactic question, therefore, is not necessarily about *cognate sentences*, because that is not the notion of 'correspondence' we need to develop a meaningful diachronic hypothesis. It is quite normal in work on the syntactic history of languages during their attested stages to bring elements from distinct grammars into 'correspondence' in the relevant sense—indeed, it is impossible to do that kind of diachronic syntax without this notion (just as it is in morphology).
23. So, if I wanted to talk about the diachronic syntax (into later stages of 'English') of an element such as Old English *sceal* 'shall', I need to draw structures from my two (or more) stages of 'English' which stand in a meaningful correspondence relationship.
24. One can only imagine that this would include looking at structures which show the descendant of the object under study (*shall*-sentences, for example) as well as sentences which express meanings earlier expressed by *sceal* sentences, but now no longer involving the descendant of that morpheme (i.e., cases of lexical replacement, which are changes we must account for as well).
25. To the degree the sentences culled by this procedure show the same syntax and a reflex of *sceal*, the syntax of *sceal* has not changed. To the extent they show the same syntax but no reflex of *sceal*, there has been some lexical change event. To the extent they show different syntax, and a reflex of *sceal*, the syntax of *sceal* has changed, and we need to explain the change event(s) involved. To the extent they show a different syntax and no reflex of *sceal*, you probably were confused in calling them 'correspondent'!
26. This seems to be the working procedure of everyone who does this kind of thing, so I apologize for spending so much time on it. The question we now face is this: does it make sense to call forms in sister languages (i.e., in grammars which descend from a common, unattested ancestor) 'corresponding' as we have done for grammars which we assume to stand in a descent relationship with one another.
27. Here I am at a loss for why we would not. To do the work on the history of *sceal* within English we needed a procedure which licensed treating distinct structures drawn from distinct grammars as being 'in correspondence'. We face no challenge identifying other, let's say, West Germanic cognates of *sceal*. It is, as far as I can see, inconceivable that Proto-West Germanic had some proto-form of *sceal* (let's call it *\*skal-*) that had no syntax at all: therefore, descendant grammar uses of the daughters of that form represent developments of that ur-syntax. The syntax of these daughter forms must show a diachronic dependence on the syntax of the relevant item in Proto-West Germanic (or they are not daughter forms); therefore, the sentences are 'in correspondence' (with respect to the syntax of *\*skal-*).
28. Our reconstruction of the Proto-West Germanic syntax of *\*skal-* will be that reconstruction which provides the most plausible and simplest account of the historical syntax of the West Germanic daughter languages. Again, there is nothing unfamiliar in this requirement: our account of the history of the 'modals' in English takes a sampling of data from grammars at a variety of points in

time and posits the most plausible and simplest account for the historical developments which link the data points. If we have no capacity for determining what is the ‘most plausible and simplest’ account for diachronic syntactic data, we can’t do diachronic syntax at all. If we do have such a capacity, it can be applied just as coherently to competing theories of the trajectory from Proto-West Germanic to its daughters as it can to competing theories of Old English to its daughters. One hypothesizes a starting point (this is called ‘reconstruction’) and deduces from that starting point a history.

### 3. What is Syntax? Can We Reconstruct It?

29. Let me now turn to the bigger-picture question as to whether or not syntactic reconstruction is possible. Let me first deal with a few preliminary matters, however. There appears to be some confusion about the nature of reconstructed (and non-reconstructed) data. In tables like the one below

Proto-Polynesian	Hawai‘ian	Maori	Samoan	Tongan	English
*fua	hua	hua	fua	fua	<i>fruit</i>
*fulu	hulu	huru	fulu	fulu	<i>hair</i>

it is traditional to mark the reconstructed Proto-Polynesian word forms with an asterisk. The use of this asterisk is generally described as marking the form as hypothetical (meaning ‘arising from the hypothesis that the Comparative Method generates’). This invites the inference that the forms not marked by an asterisk are not derived by scientific hypothesis, as e.g. Harris & Campbell (2002:601) seem to explicitly state:

...as all historical linguists acknowledge—the attested forms in the compared languages are ‘real’ while the reconstructions are hypotheses about what was ‘real’...

30. I’d like to take this opportunity to assert, as a historical linguist, that I do not acknowledge this at all. The forms in a table such as that above represent scientific hypotheses about the nature and properties of objects to which we, as scientists, have no direct access. The precise nature of the indirect access we have in the cases marked by the asterisks differs from that which we have for the forms not marked by an asterisk—the former leverages the scientific technique known as the Comparative Method to operate on a set of data to generate a hypothesis regarding the form of the word for ‘fruit’ in the mind of Proto-Polynesians, the latter uses scientific techniques of synchronic linguistic analysis to generate a hypothesis about the form of the word for ‘fruit’ in the minds of speakers of some living Polynesian languages. If you believe the latter scientific techniques generate a kind of immediate, obvious and true fact of the matter, you have not paid sufficient attention to modern debates on the form of phonological storage, computation, and form generation. Everything in the table, including the labels at the top, represent provisional scientific ‘best-guess’ hypotheses. It is an open and non-trivial question whether in their current state of development the theories we use to generate hypotheses about synchronic linguistic objects provide more veridical results than the Comparative Method, or less.

31. I’ll assume, in keeping with contemporary minimalist theory and Occam’s razor, that the syntactic computational device is universal and invariant, and that all observed apparent differences between syntactic structures result not from differences in the underlying computational system,

but from differences in the lexical content fed into that computational system. I.e., all syntactic diversity, variation and change is to be attributed to differences in the morphosyntactic features of individual lexical items (Hale 1998, stealing from Chomsky 1995, with citation).

32. Syntactic reconstruction is thus trivial, within stable evolutionary time scales. The syntactic computational system of Proto-West Germanic, and Proto-Indo-European, was the same as mine is now. Ta-da! (Well, kind of minimal ‘ta-da’, because I don’t really know much about what mine is now, but still... ta-da!)
33. Okay, trivial but not very interesting. We can make it more interesting if we decide to call changes in the morphosyntactic properties of individual lexical items which affect the treatment of those lexical items by the computational system (and thus cause the behavior of those elements and, of course, elements with which those elements interact, to become different, syntactically) ‘syntactic change’ as well.<sup>2</sup>
34. Now the question reduces to this: can we recover the morphosyntactic properties of lexical items? To the extent we can, and to the extent those features determine the properties of the syntactic output generated by the (invariant) syntactic computational device, we will have reconstructed *sentences*.
35. That some morphosyntactic features are recoverable (i.e., can be reconstructed) is quite clear: that a given reconstructed object is an N, or a D, C, P, A, or V-element is part of any reconstruction. IEists reconstruct grammatical gender, person and number, as well as case (structural and lexical) on nominals, tense, mood, and subject agreement on verbs (including some peculiar aspects of that agreement, such as the failure for neuter plural nouns to trigger plural agreement), participial marking, and the like. Each of these elements was reconstructed from the belief that the structural positions in which these morphological elements appeared in the various daughter languages stood in the type of *correspondence* relationship that made them appropriate comparanda. Without controlling for such correspondences, how are we to know that the thing we call an ablative in one language is to be compared to the thing we call an ablative in another for reconstruction purposes?
36. And again, this is not about *identity* across the daughter languages. When Garrett (1990) reconstructs the history of the Hittite ergative marker, he does not place it into ‘correspondence’ with an earlier ergative marker: there isn’t one! Instead, he presents a plausible account for how it could be ‘in correspondence’ with the Hittite ablative/instrumental.
37. Let me be clear about what I think a ‘sentence’ is, because it probably isn’t what you think a sentence is. I will assume that the syntax has no access to ‘encyclopedic’ content of lexical items (e.g., whatever makes ‘hat’ different from ‘jacket’ or ‘chair’) nor, of course, to their phonological content (hence no ‘hat’-fronting, or ‘word-with-*b*-in-it rightward extraposition). Thus, assuming identity of all syntactically-relevant features (grammatical gender, grammatical animacy, etc.), ‘the cats ate this bread’ is *the same sentence* as ‘those maruts drank the soma’. The two utterances generated from these sentences differ in their phonology (because of the phonological features of their lexical matter, not visible to the syntax, and thus not part of the syntactic computation or output) and in some aspects of their interpretation (because of the encyclopedic semantics

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<sup>2</sup>Presumably any change in any morphosyntactic property will cause an element to be treated differently by the computational system, since otherwise it is hard to see why we are calling the feature morphosyntactic, but whatever...

of the lexical items, fed into the conceptual interpretive system), but not in their syntax (nor in their syntactic representation).<sup>3</sup>

38. Everyone seems to agree (except me) that we cannot reconstruct sentences. This somehow seems to be tied to the fact that people think that words are ‘stored’ (and thus part of the knowledge state of the proto-speakers) but sentences are ‘built’ (and thus not).
39. This is wrong, as far as I can see. Morphologically complex words which show regular phonology and morphology, whose reconstruction and diachrony is treated with great scientific insight on a daily basis by Indo-Europeanists and Austronesianists (and probably others, but I don’t know about them), are built, not stored. A quick example:

Vedic Sanskrit	Greek (dor.)	PIE ‘phonetic’	PIE ‘phonemic’	
dyāw+s	zew+s	[*dyēws]	/*dyēw/ + /s/	‘sky’ (nom.sg.m.)
dyā+m	zē+n	[*dyēm]	/*dyew/ + /m/	‘sky’ (acc.sg.m.)
gāw+s	bow+s	[*g <sup>w</sup> ōws]	/*g <sup>w</sup> ōw/ + /s/	‘cow’ (nom.sg.f.)
gā+m	bō+n	[*g <sup>w</sup> ōm]	/*g <sup>w</sup> ow/ + /m/	‘cow’ (acc.sg.f.)

PIE Phonological Rules:

Consonant-Stem Root Nouns have ‘lengthened grade’ nominatives, full-grade accusatives

/w/ deletes with compensatory lengthening before word-final /m/

40. Proto-Indo-Europeans, we believe quite confidently, said something like [dyēm] for the accusative singular of ‘sky’, but they didn’t *know* (i.e., have in long-term storage) the form—they knew *how to build it*: take the stem (/dyew-/), add the accusative ending (/dyew-m/), put the result through the phonological computation system, which will produce the phonetic target [dyēm].
41. If ‘reconstruct’ means ‘formulate a scientific hypothesis about the proto-language form of’, then we have reconstructed the built (not stored) form \**dyēm*.
42. I do not believe there is a single Indo-Europeanist who would not accept that that same form, the accusative singular of ‘sky’, could be inserted into the direct object position of a sentence built around a transitive verb (e.g., \**spekyeti* ‘he, she it sees’) and that the result would be a VP which meant ‘sees the sky’. Indeed, given the functions of the accusative morpheme, and the nature of UG, it is hard to see how anyone could deny this. Moreover, again given UG, if we add a singular 3rd person subject in the nominative case to that tree in some kind of UG-acceptable position for an agentive subject (or if we add a *pro*),<sup>4</sup> I again know of no one who does not believe, and cannot conceive of how one would argue against the proposition, that the result would be a (proto-)sentence.
43. Indeed, I do not know of any Indo-Europeanist who does not believe that the proto-language had (1) *wh*-movement of the correlative pronoun \**yo-*, (2) fronting for ‘information structuring’ reasons

<sup>3</sup>The ‘direct object’ status of ‘the bread’ and ‘the soma’ is represented in the syntax, and thus, to the extent it feeds the interpretation, that aspect of the interpretation of the sentence is ‘syntactic’. But the breadiness and soma-properties of these objects is not visible to the syntax.

<sup>4</sup>I do not know where Lightfoot (2002:612) gets the idea that ‘nobody has any reliable idea of whether the speakers of the parent language, perhaps akin to some form of reconstructed Proto-Indo-European, allowed null subjects or not.’ I know of no dispute on the matter; null subjects are confidently reconstructed by all practicing Indo-Europeanists for the protolanguage, on the basis of the overwhelmingly clear evidence of the most archaic daughters.

(e.g., topicalization) to a position higher than the landing-site for *wh*-movement (Hale 1987), (3) overt gender and number agreement with adjectives and determiners within the DP (and when used as ‘secondary predicates’), etc. We could all be wrong, of course, as is always possible in scientific pursuits, but it can hardly be said that no syntactic reconstruction has been done. There has been no special procedures required to achieve these reconstructions: posit an antecedent state which best (i.e., in the simplest and most coherent fashion) accounts for the observed data in the daughter languages.

44. Each of these reconstructions has direct implications for the morphosyntactic features we would need to posit on functions heads such as  $C^0$ ,  $D^0$ ,  $Top^0$  etc. That is, we cannot reconstruct *wh*-movement and *not* reconstruct the particular set of features on the  $C^0$ -lexeme that triggers that displacement. Yet not all daughter languages of PIE show overt *wh*-movement (Hindi is *wh-in-situ*, for example). And only a few daughters (but those the most archaic) show ‘fronting’ around the landing site for *wh*-movement, and thus possessed the particular functional head (with appropriate features) that licensed *that* displacement. We are not talking about identity across the family in these cases.
45. These reconstructions continue to provide us with valuable insight into both the diachronic and synchronic syntax of the archaic Indo-European languages. Held’s work on the ‘specificity’ entailed by particular syntactic positions for the Hittite relative pronoun turns out to be helpful in Jamison’s analysis of the syntactic difference between Vedic *anya-* in its ‘another’ vs. ‘the other’ function. Positing correlative structures for Proto-Indo-European turns out to be helpful in determining the coming into being of embedded relative clauses in the immediate prehistory of Greek.

#### 4. Conclusions

46. If we can reconstruct the morphosyntactic features of lexical items and functional heads, and we assume the syntactic computational system is universal and invariant, we can reconstruct output sentences for a protolanguage. They are what gets built when you run the lexical items and functional heads through the syntax.
47. And if we can establish ‘correspondence relationships’ between sentences drawn from different grammars, including antecedent and descendant ones, we can ask what reconstructable IE sentences stand ‘in correspondence’ with attested sentences in the daughters. And we must be able to do this, since all of diachronic syntax depends upon it.
48. So, I see nothing standing in the way of reconstructing syntax in very much the same way as we reconstruct phonology and morphology. Of course the question of developing ‘directionality’ preferences and triviality indices (which would allow us to distinguished shared innovation vs. parallel independent development) is outstanding—but the evolution of these issues in phonology and morphology is instructive. First, we reconstruct, and see what the most appealing reconstructions yield as change events, and which of those change events appear to be recurrent (and thus seem trivial) and which rarer or unattested (and thus less trivial). We then revisit the reconstructions in light of this gained insight in to the processes which favor one reconstruction or subgrouping over another. Then we re-evaluate the results of that re-establishment.
49. Someday, if we are dedicated and work hard, we will move towards a more *explanatory* account of change events. I agree with David Lightfoot that this account will be based on a coherent and ex-

PLICIT theory of UG and of syntactic parsing and acquisition (and, by the way, as he also believes, our diachronic work will inform that theory). In phonology, we are just in the past twenty years making in my view significant progress in this domain: not surprisingly, this progress depends directly on theories of phonological UG and phonological parsing and acquisition (for a recent discussion, see Hale 2012).

50. The failure of 19th and 20th century linguists to reconstruct syntax is, to the extent it is true (which is far less than generally asserted), to be attributed to the fact that the tools required to understand the nature of the surface syntactic structures of the most archaic daughter languages—which differ significantly (though of course in the end only superficially) from those familiar to the Western European scholars doing the bulk of the research—have only barely begun to come on line at present.
51. Progress in Oceanic syntax, for example, where the languages are in many cases more superficially accessible to European scholars, has been much more rapid—though of course, as always, problems remain. These problems are normal, for science, the grand unifying theory of everything still being a few years away.
52. Since Cal Watkins' name appears regularly in the 'syntactic reconstruction' literature, and since I have dedicated this talk to his memory, let me close by quoting his foundational paper in *Celtica* VI:

In considering syntax from the historical point of view, it would be absurd to think that the physical body of a sentence or similar utterance can be historically transmitted. The same is valid for phonology or morphology; it is the phonological and morphological structure of the language which constitutes a set of linguistic systems which undergo historical transmission, not any given physical realization. The underlying syntactic structure of a sentence, for example the rules of arrangement of its constituent elements, can be presumed in the same fashion to form a linguistic system or set of systems functioning in time, and historically transmittable. As such, it is susceptible to analysis by the comparative method, as well as by other techniques of historical linguistics.

53. The linear sequence of words in the clause is part of the 'physical body' of the particular sentence under investigation. While of course it provides evidence for the *linguistic* (rather than physical) structures upon which it depends, that linguistic structure is much more articulated, and much richer than a row of aligned elements. Cal knew that a long time ago, but the exploration of the nature of this more abstract structure in the most archaic daughters—a necessary precursor to any reconstruction, as Cal also noted—is still in its infancy.

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