Syntactic Reconstruction:
The Correspondence Problem Revisited

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Abstract

This paper, attempts to walk through fundamental aspects of the process of syntactic reconstruction, building upon relatively explicit conceptions of (1) syntax, (2) change, and (3) reconstruction (see Hale 2008). It seems clear *a priori* that one could define these terms such that syntactic reconstruction is 'impossible', while under other definitions of these terms, syntactic reconstruction might be said to have been being successfully done since at least Bopp. The basic scientific approach to phenomena seems to entail, in this domain as in all others, trying to define one's core terms in such a way that science is possible, and then seeing if the result of investigating this domain within the parameters established by the proposed definitions seems to move our understanding of the relevant phenomena forward. If it does, it would seem there is something useful in those definitions. If it does not, and instead leads us to the conclusion that science in this domain is an impossibility, we might want to revisit the definitions, rather than give up on science altogether.

Under the proposed conceptions, which we attempt to make minimalist (in the sense that they contain as little as possible beyond what appears to be conceptually necessary), it will be seen that there should be no special problems associated with syntactic reconstruction beyond those associated with doing syntax generally (just as in phonology) and that, indeed, such reconstruction has been commonplace for some time. As in all other domains, reconstruction of syntax does not give certainty: reconstructions are hypotheses about anterior states evaluated against their capacity to explain in the simplest way the conditions observed in the daughter languages. As our understanding of the syntax of the daughter languages improves, we must revisit reconstructions performed without the benefit of our newly developed understanding. One of the major difficulties facing those attempting to undertake diachronic syntax in Indo-European (for example) is the weak foundations for such work provided by our vague and superficial understanding of core properties of the syntax of archaic Indo-European daughter languages. As we make progress on these matters, our capacity for reconstructing ever more refined aspects of the proto-language syntax will be increased.

The paper is programmatic, with a brief walk-through of some core ideas from the past, both relatively distant (Behagel, Wackernagel) and more recent.

0. Preliminary Remarks

1. It is a simple truism that Indo-European studies has not 'lead the way' in the development of the Comparative Method in the domain of syntax the way it clearly has in the morphological and phonological domains. We want to look today at the degree to which this belief is true and, to the extent it is true, what some of the reasons for it might be. First, it will be necessary to establish some clarity about the nature of syntactic change and syntactic reconstruction.

2. Much of the debate surrounding the issue of syntactic reconstruction seems to us to be confounded by a certain degree of terminological (and perhaps conceptual) imprecision. In today's talk we would like to present some aspects of the problem, walking through how they look to someone who makes the set of assumptions that we make regarding how historical linguistics works, what syntax is, what change is, and what reconstruction is.

3. While, of course, all of the assumptions we make are correct and you should just adopt them at once, you may decide to persist in your own orientation to these matters. We hope that seeing how the matters play out under our 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.

4. There are many aspects of this issue which are inordinately complex, some of which we couldn't talk about no matter how much time we had (because we don't understand them well enough), some of which cannot be meaningfully explored in the timeframe afforded this talk. We 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?.

5. Our musings on the latter question will obviously be connected to our 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 we will not be able to treat in any meaningful way today.

6. Finally, we will take 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. Correspondence in Comparative Linguistics: the "turtle" problem

7. The notion of 'corresponding' linguistic elements, which determines what gets 'compared' in comparative reconstruction, has, in our 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 of the type shown below, which we am confident you have all seen. It lists cognate forms in many languages; here we give a Polynesian example because of its extreme transparency (in the phonological domain).

Hawai'ian	Maori	Samoan	Tongan	English
hua	hua	fua	fua	*fruit*
hulu	huru	fulu	fulu	*hair*
heʔe	ʌeke	feʔe	feke	*octopus*
hiku	ʌitu	fitu	fitu	*seven*
haku	ʌatu	fatu	fatu	*stone*
niho	niho	nifo	nifo	*tooth*
honu	honu	—	fonu	*turtle*
mahana	mahana	mafana	mafana	*warm*

9. Such data, revealing as they do systematic and pervasive sound and meaning 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. We 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 we 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 we 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 'fruit bat' 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[1], 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 our 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. This is obviously different from the type of 'correspondence' that allows us to establish language relatedness—it would be absurd to assert that the fact that the Samoan word for 'turtle' is *laumei* is evidence that Samoan is related to languages for which the word for 'turtle' is a descendent of Proto-Polynesian *fonu!

18. 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 changes affecting 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.

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[1]The innovation could have preceded the *ō>ā change, and thus might have been *-ōnōm; the point remains the same, of course.

19. 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'?

20. 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.

21. 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.

II. 'Syntactic' Correspondence

22. 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, we 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 *phonological* relationship between semantically-related lexemes and is an important mechanism for establishing genetic relatedness—and *laumei* is not the *phonological* correspondent of **fonu*, but the lexical replacement thereof.

23. The syntactic question, therefore, is not necessarily about *cognate sentences*, whatever exactly that might refer to, because that is not the notion of 'correspondence' we necessarily need to develop a meaningful diachronic hypothesis, contra e.g. Lightfoot (2002a,b). 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).

24. So, if we wanted to talk about the diachronic syntax (into later stages of 'English') of an element such as Old English *sceal* 'shall', we would need to draw structures from our two (or more) stages of 'English' which stand in a meaningful correspondence relationship.

25. 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).

26. 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'!

27. This seems to be the working procedure of everyone who does this kind of thing, so we 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.

28. Here we are 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 we 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 daughter reflexes of that PWGmc. 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, we find **skal-* correspondences within such sentences.

29. 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' in one case, 'syntactic analysis' in the other) and deduces from that starting point and the attested data point(s) a history.

III. What is Syntax?

30. We'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. That is, 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).

31. 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 ours is now. Ta-da! (Well, kind of minimal 'ta-da', because we don't really know much about what ours is now, but still… ta-da!)

32. 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 'syntactic change' as well.

33. 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 a system that *generates* proto-language sentences.

34. 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 configurations 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?

35. Let us be clear about what we think a 'sentence' is, because it probably isn't what you think a sentence is. We will assume that the syntax has no access to 'encyclopedic' content of lexical items (e.g., whatever makes 'duck' different from 'jacket' or 'chair') nor, of course, to their phonological content. 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 content, 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 of the lexical items, fed into the conceptual interpretive system), but not in their syntax (nor in their syntactic representation).[2]

36. Some such conception of what syntax is is required in any event, if we are to understand why there are processes such as the movement of relative pronouns (*wh*-movement) and auxiliaries (as in I>C movement for question formation), but no such thing as "*duck* movement" or "trisyllable fronting" in human languages. The easiest explanation for this cross-linguistic fact is that the syntax can identify *wh*-elements and auxiliaries (and thus can manipulate them), but can't identify 'ducks' or 'trisyllables' (and thus *cannot* manipulate them).

37. Everyone seems to agree (except us) that we cannot reconstruct sentences. This may be the result of a lack of general acceptance of our definition of 'sentence' (in 35), but it also seems (on our reading of the literature) to be tied in some way to the fact that people think that 'words' are 'stored' (and thus "transmitted" as part of the knowledge state of the proto-speakers) but sentences are 'built' (and thus not "transmitted" in this way).

38. This attempted contrast between 'words' and 'sentences' is misguided, as far as we 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

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[2]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.

Austronesianists (and probably others, but we don't know about them), are built, not stored. A quick rather toy-ish example will suffice here:

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	[*gʷōws]	/*gʷōw/ + /s/	'cow' (nom.sg.f.)
gā+m	bō+n	[*gʷōm]	/*gʷow/ + /m/	'cow' (acc.sg.f.)

PIE Phonological Rules:

(1) This class of consonant-stem root nouns have 'lengthened grade' nominatives, full-grade accusatives.

(2) /w/ deletes with compensatory lengthening before word-final /m/

39. Proto-Indo-Europeans, we believe with some confidence, 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].

40. If 'reconstruct' means 'formulate a scientific hypothesis about the proto-language form of', then we have reconstructed the built (not stored) form **dyēm*.

41. We 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., **speḱyeti* 'sees') and that the result would be a VP which meant 'sees (the/a) 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 3rd person singular subject in the nominative case to that tree in some kind of UG-acceptable position for a subject (or if we add a *pro*),[3] We again know of no one who does not believe, and cannot conceive of how one would argue against the proposition, that the result (once we add in the relevant functional structure, of course) would be a (proto-)sentence. **If it *would not be*, then our reconstruction of IE morphology and lexicon is wrong.**

42. Indeed, we 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 (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.

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[3]We 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.' We 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.

43. 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(s) (i.e., those with appropriate features) that licensed *that* displacement.

44. 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 a lexicon from which we can build output sentences in a protolanguage. So, we see nothing standing in the way of reconstructing syntax in very much the same way as we reconstruct phonology and morphology.

45. 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.

IV. So, if we think it's so easy, why are we not there yet?

46. Some excellent work has been done in the history of the field. A key insight is summarized in Behagel's First Law (e.g., Behaghel 1932 §1426):

> Das oberste Gesetz ist dieses, daß das geistig eng Zusammengehörige auch eng zusammengestellt wird.

47. This is the central notion, to this day, of *constituency*. If we take it seriously as a fundamental law of syntax, then we need to conceive of apparent violations as just that, merely apparent. An object 'focus fronted' away from the rest of its constituent ('*bananas*, I really like') appears to violate this law, but it must not (because it's a law!). Instead, such an element belongs 'conceptually' to more than one element of the syntactic representation. As an object, it needs to enter into a relationship (i.e., be 'zusammengestellt') with the verb, as a [+focus] element, it needs to enter into a relationship with the Focus head (which sits high up in the left edge of the tree). 'Movement' is a metaphor we use to explain *how* it is possible for a single word to do both these things.

48. Another insightful achievement was of course Wackernagel's Law (Wackernagel 1892, also as cited below, 1920:7, emph. ours), which asserts that there is a strong tendency for it to be the case

> … dass schwach betonte Wörtchen, **welches immer ihre syntaktische Beziehung sei,** unmittelbar hinter das erste Wort des Satzes gestellt werden.

49. While I do not believe any such law exists, the methods used are foundational. Wackernagel's analysis explicitly leverages two concepts which persist as standard tools of syntactic research to this day:

50a. 'constituency' (just as in Behaghel's First Law), leveraged in the following ways:

> 50a.i.A clitic in second position may stand apart from its *constituent of origin*.
>
> 50a.ii.A clitic in second position may interrupt an otherwise syntactically coherent constituent.

50b. 'Movement', leveraged in the following ways:

> 50b.i.When the clitic stands apart from its constituent of origin, some process has intervened to 'move' it from its 'expected' position.
>
> 50b.ii.When the clitic intervenes between the elements of what would otherwise be a constituent, some process has intervened to 'move' the clitic in such a way as to disrupt the constituent it is interrupting.

51. Both phenomena are exemplified in an example such as AVŚ 3.12.4c: *nas* stands apart from its 'constituent of origin' (in apparent violation of Behaghel's First Law), since it is the possessor of distant *kṛṣím*. In addition, the constituent *bhágo rā́jā* has been 'interrupted' (again, in violation of Behaghel's First Law) by *nas*.

> (1) | *bhágo* | *no* | *rā́jā* | *ní* | *kṛṣím* | *tanotu* |
> | --- | --- | --- | --- | --- | --- |
> | Bhaga-NSG | our-cl | king-NSG | down-PV | ploughing-ASG | let extend |
>
> "let king Bhaga deepen our ploughing" (AVŚ 3.12.4c)

52. The basic claim of Wackernagel's paper is that *a single process*—the one we now call Wackernagel's Law—is responsible for both of the displacement phenomena mentioned above, and thus that the two kinds of violations of Behagel's First Law have a unified explanation.

53. Although he uses none of the familiar terminology, Wackernagel has a conception of syntax which involves a sentence formation process which builds coherent syntactic constituency, and another process ('Wackernagel's Law') which may disrupt that constituency in the two ways outlined above. These correspond roughly to, e.g., the minimalist program's MERGE (build constituency) and MOVE (satisfy additional constituency demands with elements already merged).

54. However, Wackernagel's system is *probabilistic* ("clitics *tend* to occupy second position in the clause"). As such, it has the negative properties of any probabilistic model, i.e., it does not explain any actual individual clause. Probabilistic systems do not explain any *individual* piece of data, because, if they could, they would not be probabilistic. Such systems only make claims regarding the statistical behavior of a *set* of data.

55. Because such models do not make claims regarding individual pieces of data, they are inherently discouraging of further research into factors that might be responsible for a given example, and thus discourage the research which would show that the assumption of an underlying probabilistic dynamic is false.

56. Under most modern conceptions of Wackernagel's Law, what is involved is not the single dynamic Wackernagel envisioned (his 'Law'), but rather an interaction between principles of syntactic placement (of a relatively poorly understood type) and principles of prosodic placement (ditto).

57. So, why did Behaghel and Wackernagel's Laws fail, in the sense that they did not lead us into a position of now, more than a hundred years later, having a firm grasp on the syntactic structures of archaic

Indo-European languages, on the nature of syntactic change, and on the methods to be used in syntactic reconstruction?

58. Behaghel's Law failed because *we* failed to take it seriously enough: in cases where the 'conceptual relationships' into which an element entered were *complex* (i.e., greater than one), as in WH-movement or clitic dislocation, we simply said that these purely syntactic operations took precedence over the 'semantic' argument of Behaghel. But syntactic movements happen in order to establish the relationships between elements that are required for an appropriate interpretation. That the relationship is sometimes with something *phonologically* null (like a FOCUS head) does not make it less a relationship.

59. The result of not taking Behaghel's First Law seriously enough is that we took it as a probabilistic claim—i.e., it was a rule, but could be violated. As pointed out, probabilistic analysis does not give us an account of any actual data, and impedes asking the questions that deterministic assumptions compel us to ask.

60. Wackernagel's Law was a valiant attempt to present a unified account of what, in the end, are distinct phenomena—some syntactic, some prosodic. By mixing together a variety of different phenomena, the resulting generalization could be no more than probabilistic (since the causal factors were not properly distinguished), with similarly negative results.

61. 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.

62. Unhelpful, in this regard, was the fact that in general Indo-Europeanists were (and are!) working with corpora. Corpora have properties which 'languages' do not, and thus permit a 'raw empirical' approach to the data: by 'raw empirical' we simply mean that many, many *absolutely true* and *absolutely uninsightful* things can be said about a corpus which could not be said about a 'language'. For example, Avery (1881), in describing the position of relative pronouns within relative clauses, which occupy a position (as expected in a WH-movement language) in the CP, that they occur in first position of their clause 65% of the time, in second 24.4%, in third 6.5%, in fourth 2% "and so on in decreasing frequency down to the ninth place." If we were talking about the *linguistic capacity* of a speaker of Vedic Sanskrit, it would simply make no sense to generate such numbers. The ability of earlier scholars to do *accurate* work of this type encouraged an impression of the nature of the language under study, and of course of language in general, which has been detrimental to our pursuit.

63. This lack of knowledge about the *synchronic* syntactic properties of the most archaic daughter languages is an obvious impediment to reconstruction. Imagine that we had a set of partially-deciphered languages which showed the following correspondence set (where we use symbols to represent the undeciphered bits):

Language A	Language B	Language C	Language D	gloss
▶tati	☺tasi	●taʃi	⫼tat^{s}i	"tree"
ta▶	ta☺	ta●	ta⫼	"fish"
ti▶	si☺	ʃi●	t^{s}i⫼	"cow"

64. What would we reconstruct for the ▶: ☺: ●: ⫼ correspondence set? We have no idea whether the segment is a stop, a fricative, a labial, a velar, a nasal, a glide, or even a vowel. We don't know if a change of the ▶ > ☺ is more likely than the inverse, assuming, indeed that there has been a change. There is nothing surprising in this: it is just Saussure's primacy of synchronic linguistics argument. You cannot explain the history of ☺ or ⫼ without a theory of what they *are*.

65. So let's take a fairly straightforward aspect of word order, the syntax of the demonstrative **tó-* of PIE. Imagine, in this toy example, that the only languages we had to compare were Attic Greek prose and the Rigveda. As is well-known, the Attic Greek reflex of **tó-* is a definite article, and in this function it is not separated from its head noun.[4] By contrast, the Rigvedic reflexes of **tó-* are (weak) deictics, and are regularly separated from their head nouns (see Hale 1990 for a discussion of their syntax, with previous literature).

66. Since, to take a concrete example, the Greek acc.sg.m. τόν and the Sanskrit acc.sg.m. *tám* are both reflexes of IE **tóm*, their syntax must be the reflex of the syntax of IE **tóm*. Since both elements are "determiners" (i.e., members of the D-class of morphosyntactic objects), we need to ask what feature it is that the Greek D-element τόν has (or lacks) which blocks syntactic discontinuity from its head. Or, alternatively, which feature does Rigvedic Sanskrit have which allows such discontinuity? One thing we know for sure, however, given the assumption of a uniform syntactic component: there is some feature or set of features, obviously implicating DPs, which differ in the two systems. So:

Attic Greek	Rigvedic Sanskrit	PIE
τόν	*tám*	**tóm*
acc	acc	acc
sg	sg	sg
masc	masc	masc
☕	⚽	**???*

67. Since we don't at present have a formal *feature-based* account for the grammaticality of this type of discontinuity in the Rigveda and its ungrammaticality in Attic Greek (and English, and German, etc.), we don't know what the relevant features are (hence the ☕ and ⚽). Since we don't know what they are, we don't know how or why they would change, what directionality effects there might be, or even how many features there are or how they relate to one and interact with one another.

68. This is parallel to the (non-comparative, simple diachronic syntax) problem raised in Hale (1998) regarding discontinuity in Latin. That paper cited these examples:

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[4]We will ignore, though the matters are of significant interest, both the possibility of clitic/postpositive intervention—which in our view is prosodic in nature, rather than syntactic—and the "doubled articles" which function like *izafe* particles.

68a. **pater** huc me misit ad uos oratum **meus**

"my father sent me here to speak to you" [Plautus, *Amphitruo* 20]

68b. …ego …qui **Iouis** sum **filius**

"…I…who am the son of Juppiter" [Plautus, *Amphitruo* 30]

68c. **reges** quo ueniant **et di**…

"in which kings and gods shall appear…" [Plautus, *Amphitruo* 61]

69. The situation that was lamented at that time—that "[i]t is not at all clear at present how such discontinuous constituency is to be handled within contemporary syntactic theory"—persists, unfortunately. It is true that as Latin develops into Romance, these types of discontinuity (which may well not be all of the same type) disappear. They do not appear to fade "one word at a time" (e.g., first for *pater* or *meus*, later for *frater* and *tuus*)—this probably indicates that the relevant features are located on those elements of the syntactic representation which one cannot *see* in the strings: the functional structure.

70. Functional heads of the relevant type are frequently phonologically null, their presence detected by (1) their effects on the semantics of the sentence (where would the meaning come from, if it was never put in?) and (2) their effects on the behavior of the 'visible' material.

71. Thus many an interesting and significant morphosyntactic correspondence may be of the $\emptyset : \emptyset : \emptyset$ etc. type, where each null is a corresponding morphosyntactic functional element. Such elements have not been reconstructed for PIE, and without them, we have no comprehensive syntax of the IE clause. These functional heads appear to affect "surface order" particularly strongly (the way the interrogative head triggers subject-aux inversion in English or German, or the wh-head triggers wh-movement in those languages). Our "reconstruction" failure is thus strongest in that domain where our knowledge is most limited—a perfectly understandable state of affairs, with an obvious remedy.

V. Conclusions

72. Syntactic change and syntactic reconstruction can both be meaningfully investigated, and to a certain extent have been. The elements which 'correspond' in the relevant sense in syntactic reconstruction are precisely the same set of elements which correspond in phonological reconstruction: lexical items. In phonological reconstruction it is the phonological feature bundle associated with such items which is subject to analysis using the comparative method; in syntactic reconstruction, it is the morphosyntactic features that we must work with. Since the lexical items *correspond*, they are both descendents of a single proto-lexical item. That proto-lexeme, like all lexemes, had both a phonology and a morphosyntax. It is the history of that lexeme, in all of its aspects, that we must reconstruct.

73. The primary shortcoming of previous work on IE syntax arose in the domain of what we might call 'configurational syntax', and it arose here because the archaic languages were, for the scholars doing this work, relatively 'exotic' in precisely this domain.

74. Someday, we will move towards a more *explanatory* account of change events. This account will be based on a coherent and explicit theory of UG and of syntactic parsing and acquisition (and, by the way, our diachronic work will inform that theory). In phonology, we are just in the

past twenty years making in our 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).

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