

Syntax and Morphology are Different: Comments on Jonas' "Residual V-to-I"

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A long time ago, well before the first DIGS conference, I participated in a conference held at the University of California, Santa Barbara, devoted to "Mechanisms of Syntactic Change." In his later comments on a collection of papers from that conference (Li 1977), Lightfoot (1979b) observed that very little of the substance of any of the papers in the volume actually had anything much to do with historical **syntax** — nearly all of the analyses and discussion concerned historical change in the content and use of morphology.

Lightfoot's remarks were offered in a typically acerbic style, *pour épater les bourgeois*, and if it is difficult to capture the dismissive tone of his characterizations today, it is because it is hard to recall the time when literally everything of significance for generative linguistics was either phonology or syntax. If there was anything at all that deserved the name "morphology," it was just a list of relics and the undigested residue of historical change.

Since then, of course, much has changed: in substantial part due to Lightfoot's own work (1979a, 1999, and elsewhere) and that of other contributors to the DIGS conferences, we now really do have a substantive and interesting study of genuinely syntactic change. For another thing, we have serious theories of morphology that do not just reduce to the detritus that fails to attract the interest of either phonologists or syntacticians, so his characterization would not have nearly as pejorative a connotation today.

In fact, I think that it is quite accurate to describe much of what was presented at the conference in question, and much other "historical syntax" over the century or so that led up to it as really limited to morphology.¹ Which, of course, makes us want to know why so many otherwise intelligent people could be so hopelessly confused about the difference between word structure and syntax.

In the nineteenth century and for more than half of the twentieth, it might be possible to blame this on the fact that there really wasn't a theory of syntax *per se* to be drawn on. Remarks on "syntax" in traditional grammars were really confined to a sort of "applied morphology": remarks on the uses of the inflectional forms and word classes of the language. In structuralist work, syntax was really just a promissory note to the effect that if linguists kept on doing morphology long enough, this work would eventually encompass phrases and clauses as well as words. But by the 1970's, that was no longer enough, because of course we did have an emerging sense of the richness of syntax.

What is to blame for the confusion noted above, rather, is a pervasive assumption that the categories of morphology — inflectional morphology, at least — are pretty much isomorphic to those of syntax, at least if viewed in a sufficiently abstract light. And I submit that even though both morphology and syntax have evolved greatly over the past

¹ Including, naturally, my own paper (Anderson, 1977) at the Santa Barbara conference.

three decades or so, some of that assumption still lingers. Partly that is because it would be really interesting if it did, in fact, turn out that morphological categories reflected syntactic structure in such a faithful way. Another reason for the persistence of this notion, though, is the fact that not enough syntacticians actually get involved in the dirty work of morphology (and of course, *vice versa*). And that is where the material discussed in Jonas' paper finds its greatest importance, at least for me.

There is a long tradition of attempting to derive the syntactic organization of a language from the outward and visible signs provided by its morphology. This includes the ancient observation that exuberant case morphology allows for greater freedom of word order, or that rich verbal agreement makes it possible to omit overt pronominal subjects. Less successful perhaps, but along the same lines, is the frequently recurring suggestion that ergative case marking reflects an underlyingly "passive" organization of grammatical structure within the clause.

In the present context, this is the methodology of a substantial literature that attempts to find some property of a language's verbal agreement morphology that can be correlated with syntactic movement of the Verb to the **I**(nflexion) position. Ignoring the fact that this discussion seems to take only Germanic and (a few) Romance languages into consideration, one's immediate impression is that it tends to be extremely legalistic, relying on conditions like 'if and only if at least one number in at least one tense of the regular verb paradigm distinguishes first from second person, then **V** moves to **I**' or 'if and only if there is person morphology in all tenses, then **V** moves to **I**,' and so on. This kind of thing has persisted at least through May, 2000 (when I attended a paper along these lines, Vikner (2000), at a Scandinavianist meeting in Lund).

What Jonas (2001a) shows in her paper is that all such efforts must necessarily fail: dialects of Faroese with identical morphology can nonetheless differ with respect to the presence of **V** to **I** movement. There cannot, in such a case, be any morphological predictor of the type many have sought. This is entirely comparable in its force to the argument for the necessity of extrinsic rule ordering that arose in the early literature on generative phonology from the demonstration that dialects with the same rules and underlying forms could nonetheless display distinct surface forms when those rules interact in different orders. A further piece of evidence for the same syntactic point, of course, can be derived from the observation that Scots and Shetland English, languages which do not differ in any relevant way in the richness of their verbal morphology from standard English, nevertheless differ from it in the presence of **V** to **I** movement.

Now why, in fact, would we expect to find a correlation between verbal morphology and Verb Movement? What connection is there between the two? Presumably, this comes from the expectation that suitably "rich" agreement projects some functional structure through which verbs would have to pass, and this requirement would trigger the relevant movement.. The presence of this structure would then be identifiable with some property of the overt morphology.

Once we put it that way, though, it seems extremely unlikely that the divine hand of natural selection would have endowed Universal Grammar with anything so legalistic as what is implied by the existing proposals. We might possibly expect that the presence (*vs.* the absence) *tout court* of agreement morphology as a whole might have significant

structural implications, but of course that is not what we find. Rather, it seems that some agreement morphology is found in the presence of **V** to **I**, while other quite similar morphology consorts with its absence. The kind of quasi-Talmudic specificity that seems necessary to distinguish the two sets of circumstance is a poor candidate for a universal of human language. Fortunately, Jonas' arguments make it clear that we need have no fear of that, since no such condition can apparently be formulated.

In fact, the assumption that the presence of structure motivating **V** to **I** can be correlated with some overt characteristic of inflectional paradigms is just another instance of the "Correspondence Fallacy" noted above: the assumption that syntactic and morphological (especially inflectional) structure ought to be isomorphic. The two are surely related — and the relations are at least partly systematic — but each has its own partially independent properties such that neither can be exhaustively reduced to the other.

Since overt morphology potentially provides the kind of observable feature that might be of use in the process of acquisition, it might be feared that such a devaluation of its evidential value would be a blow to the ideal of cue-based learning. This need not be the case, however. As pointed out by Gleitman (2001), in order for a child to make use of surface marks to infer grammatical structure, s/he must already have quite a sophisticated command of what the possibilities are for that structure. What is involved, that is, is a choice on the basis of surface cues from among the limited set of possibilities provided by Universal Grammar, not the induction of the entire structure *ab initio* from such cues.

In the specific case before us, if we believe that V to I movement is an option available to the grammars of specific languages, we must imagine that some property of readily observable sentences indicates for the learner whether the language in question employs that option. But the child certainly does not need to see overt inflectional morphology that would require the positing of a layer of functional structure forcing such movement in order to make this decision. How, after all, do syntacticians diagnose the presence of **V** to **I** movement? Taking Jonas' paper as our model, it would appear that this is commonly done by looking for instances in which the verb precedes negation (assumed to appear at the left edge of **VP**); and if that is good enough for syntacticians, it ought to be good enough for the child as well.

There is another point in Jonas' paper which piques the morphologist's sense of *amour propre*: the description of affixal negation, especially with regard to Scots and Shetland English. Jonas suggests that suffixal *-na* in these languages is comparable to the *-n't* of standard English *didn't*, *don't*. Now in fact, Zwicky and Pullum (1983) showed some time ago that affixal *-n't* in standard English is not, as commonly assumed, simply a phonologically reduced form of *not*, but rather an unusual instance of inflectional negation, a category limited in English to the class of auxiliary and modal verbs. While Zwicky and Pullum's arguments have been largely ignored by the syntactic community, they were actually quite persuasive and remain so today, at least in my opinion.

This should lead us to ask whether Scots and Shetland *-na*, similarly, represents inflectional negation in those languages. As Jonas shows, older Scots and Shetland show *-na* with what looks like a full range of lexical verbs, though in modern Scots, as in standard English, it only occurs with auxiliaries and modals. One possibility of course is

that the inflectional category of negation was generalized to all verbs (though later restricted again in Scots). Much more likely, however, is the suggestion that in e.g. Shetland English, *-na* really does represent simply a phonologically defective (i.e., clitic) form of full lexical negation.

This provides us with a necessary intermediate stage in the development of the modern English (and Scots) situation. We presume that negation was originally a full lexical item, introduced adverbially and adjoined at the left edge of the negated **VP**. At some point, the negative word was presumably reduced phonologically, though this did not automatically alter its structural position. Since the reduced negative was typically attached *phonologically* not to a following element of the **VP**, however, but rather to a preceding auxiliary, it came to be re-analyzed by later generations of learners as part of the inflection of that element as in modern English.

The Shetland (and older Scots) situation represents the intermediate stage in which *-na* was a simple clitic form, as opposed both to its earlier status as a full word and to its later status as inflectional morphology on the auxiliary verb. If *-na* in fact differs from *-n't* in this way, then its appearance following a full range of verbs in Shetland is indeed consistent, as Jonas assumes, with the notion that it continues to mark the left edge of **VP** and thus indicates the presence of **V** to **I** movement. The modern English (and Scots) situation, in contrast, results when the auxiliary and modal elements still found in **I** are reanalyzed as potentially inflected for negation in the absence of **V** to **I** movement for lexical verbs. This would be entirely consistent with the suggestion of Jonas (2001b) that there are two independent positions in which sentential negation may be generated: one associated with **IP** (and realized with the inflectional content of **I**), and one associated with **VP** (and realized adverbially).

The moral for syntacticians of the possible non-isomorphism of syntactic and inflectional structure is straightforward. Respect the overt morphology that you find, for it can be your friend. In many instances, systematicity of the relation between syntax and inflectional can provide useful clues to structure. But do not expect it to do your analytic work for you, because it has concerns of its own which may not be identical with yours.

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