Assessing the morphosemantic program for phi-features:
the prospects of a cross-modularly stable representation
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Thanks in large part to the research program detailed in Harley & Ritter 2002, the issue of the algebraic structure of phi features now sits at the forefront of much work in morphology, syntax, and semantics. But consensus on what the correct algebraic structure actually is has remained elusive (consider, for example, the conflicting proposals in Ackema & Neeleman 2017; Bale, Gagnon & Khanjian 2011; Cowper 2005; Harbour 2016; McGinnis 2005; *i.m.a.*).

The reason for this lack of consensus, I argue, is that work on this topic too often mixes semantic evidence with syntactic evidence, syntactic evidence with morphological evidence, and so on. The mapping between these different modules of grammar is of course *often* transparent (indeed, transparent mapping is a reasonable null hypothesis in the general case), but it is by no means guaranteed that the mapping *will* be transparent in every case we come across. I will argue that, in fact, we already have sufficient evidence that there can be no transparent cross-modular mapping when it comes to the structure of phi features.

1. Criteria for featural representations in syntax: Let us start with an illustrative example from an entirely different empirical domain. I take it as a given that there are no syntactic probes in natural language that target exclusively *non*-wh phrases. (See Richards 2010 for discussion of some apparent counterexamples.) This typological gap can only be derived if we assume that there is no featural specification, borne exclusively by non-wh phrases, that can serve as the search criterion for a syntactic probe. There are a couple of conceivable implementations of this idea, but the differences among them do not seem (syntactically) consequential: perhaps the '−' polarity of a feature (as in "[−wh]") cannot be probed for; or perhaps [wh] is privative, and syntax simply cannot probe for the *absence* of a feature. The important thing is that some such property had better be true, if we have any hope of capturing the absence of exclusively non-wh probes in natural language—and I think this is something that any grammatical theory worth its salt had better capture.

If this thinking is on the right track, then we can draw similar conclusions about the feature structures of PERSON and NUMBER, as it regards their <u>syntactic</u> representations. I know of no convincing case where a probe seeks exclusively singular DPs (to the exclusion of plural ones), nor where a probe seeks exclusively 3rd person DPs (to the exclusion of 1st/2nd person ones). The converse patterns, however, are attested: omnivorous number agreement (e.g. Nevins 2011 on Georgian) and omnivorous person agreement (e.g. Preminger 2014 on K'ichean) involve probes that seek only plural targets, and only 1st/2nd person targets, respectively. By parity of logic with the [wh] case, the conclusion is that "singular" and "3rd person" are represented in a way that is syntactically inactive (either because '−' features are generally inert in syntax, or because singular and 3rd person are simply represented by the absence of NUMBER and PERSON features, respectively). As with the [wh] case, the absence of "omnivorous singular" and "omnivorous 3rd person" should be on the agenda of any theorist working on the syntactic representation of phi features—and so something like this had better be true, if we hope to account for the data at hand.

2. Some fairly clear cross-modular mismatches: If the syntactic conclusions above are correct, then it is already clear that syntax and morphology cannot possibly involve the same representation for phi features. The reason is as follows: we know that morphology is capable of exponing the *3rd singular* cell in a paradigm to the exclusion of all other cells (as is the case in English nonpast main verbs). Because the remainder of the paradigm (excluding 3sg) is not a natural class, the exponent of 3sg presumably is not the result of an 'elsewhere' rule.

But if the representation of phi features in syntax and morphology were identical, direct reference to the 3sg cell would involve the "dot-product" of two specifications—*singular* and *3rd person*—each of which is featurally inert (and has to be, to capture the absence of omnivorous agreement targeting these categories; see sec. 1). Syntax and morphology, then, cannot possibly involve the same representational system for phi features.

A similar argument could be furnished—though somewhat less decisively—based on the representation of NUMBER in semantics and its representation in morphosyntax and morpho-phonology. It is often assumed that 'plural' is semantically inert, whereas 'singular' means something like *λx.Atomic(x)* (Sauerland 2003 *i.m.a.*; but see Bale, Gagnon & Khanjian 2011, Martí 2017). If this is correct, then there is a clear and obvious mismatch between the semantics of NUMBER and its morphophonology, since, in the latter, 'plural' is clearly the marked member of the 'singular-plural' opposition (Greenberg 1963). So somewhere between semantics and phonology, the wires of markedness would have to cross.

There are other relevant case studies, omitted here for reason of space—e.g. cases of morphological syncretism between 2nd and 3rd person, despite the fact that neither semantic criteria nor syntactic ones indicate this is a natural class; cf. Ackema & Neeleman 2012).

3. The proposed alternative: To solve these apparent paradoxes, I suggest something rather basic: using syntactic evidence to adjudicate among proposed syntactic representations, morphological evidence to adjudicate among proposed morphological representations, and so on. This means taking seriously the idea that the primitives of syntax might be different from the primitives of morphology, which might be different from the primitives of semantics, etc.—but, crucially, that is something we already know to be the case. It forms the basis for the hypothesized separation of grammar into these sub-modules, in the first place. My suggestion is that we stop treating the study of phi features as if it should somehow be exempt from the same considerations.

4. Broader considerations: If we accept the premises outlined above, then we must address the question of restrictiveness, and the dual question in the domain of language-acquisition.

One option is that, while the representation of phi features may (and in some instances does) vary across modules, the entire picture is cross-linguistically fixed, and there is simply nothing left to *acquire*, in this domain. While this may work on the syntax-semantics front, it seems rather hopeless on the morphological front. Considering NUMBER once more, 'plural' is clearly the marked member of the 'singular-plural' opposition, morphologically speaking. At the same time, English nonpast main verb agreement requires direct reference to 'singular' (or to the absence of 'plural') to capture the distribution of the */−z/* affix. There must be a way for the child to acquire this exceptional specification, then, on a language specific—and, possibly, morpheme-specific—basis.

As it concerns morphology, it is not hard to imagine what an appropriate strategy might look like. A built-in pressure to avoid accidental homophony would suffice for the child to conclude that—in the English nonpast—the null exponent must be the 'elsewhere' case, and that, consequently, the insertion rules must make direct reference to the *3sg* cell (i.e., to the absence of syntactic [participant] and [plural]).

To the extent that direct evidence of this sort is not available to the learner as it concerns the representations of phi features in syntax and in semantics, these must indeed be cross-linguistically fixed. Crucially, however, the demand that these fixed syntactic and semantic representations be identical to one another is an illegitimate demand, given that we already know that such strict correspondence is not what we find on the syntax-morphology side.