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The logic of syntactic priming and acceptability judgments

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Abstract

A critical flaw in Branigan and Pickering's advocacy of structural priming is the absence of a theory of priming. This undermines their claims about the value of priming as a methodology. In contrast, acceptability judgments enable clearer inferences about structure. It is important to engage thoroughly with the logic behind different structural diagnostics.

Main text

Branigan and Pickering (B&P) wish that structural priming would get more love as a source of evidence about linguistic structures. We certainly welcome all sources of relevant evidence. But their case would be helped if they engaged more thoroughly with the logic underlying priming and standard acceptability judgment (AJ) evidence. Their disparagement of linguistics is unhelpful, but we leave it to other commentators to address those issues.

Most importantly, B&P provide no account of syntactic priming and the circumstances where it should obtain, beyond the obvious fact that priming reflects similarity of some kind. This makes it difficult to draw clear conclusions from priming findings. Meanwhile, they miss key aspects of how acceptability judgments are used to draw inferences about linguistic structure.

B&P claim that AJ merely diagnose 'set membership', i.e., whether a sentence is possible or impossible, whereas priming is more powerful because it additionally diagnoses similarity between strings. This is incorrect. AJ is routinely used to test paradigms of closely related sentences, in order to diagnose specific representational properties, e.g., identity (coordination, ellipsis), structural hierarchy (binding), or locality (relativization, wh-movement). These tools do not diagnose all properties that we might care about, but when they work, they allow precise inferences. In contrast, priming diagnoses similarity in a less precise fashion. A pair of sentences can be similar in multiple different ways, so the finding that they prime one another allows us to draw only weak inferences. A more explicit account of structural priming could sharpen B&P's arguments. We suspect that they assume that priming requires a form of structural identity in which the prime and target include a single piece of structure that is identical between the two, e.g., a verb phrase, and that structure has identical daughters, e.g., the verb phrase has a ternary branching internal structure. This is different from the weaker hypothesis that, for example, the prime and target must share a sequence of syntactic nodes,

even if those nodes are not structured identically. The evidence for the stronger hypothesis is not provided, and it is not clear what such evidence would look like.

Priming is not well suited to identifying differences rather than similarities between structures. We contrast this with arguments that can be constructed using patterns of AJ. These sometimes provide evidence that superficially different constructions are structurally similar (e.g., comparative constructions and wh-questions are subject to the same locality restrictions, Bresnan 1975). Sometimes the converse is true: constructions that are superficially similar are actually structurally different (e.g., control vs. exceptional case marking constructions).

To take a specific example from the paper: priming between unaccusative and unergative constructions in Spanish only provides evidence that the two constructions are similar at some level, as is evident from their surface syntax. Nobody disputes that they have something in common. The interesting contribution from various kinds of AJ evidence is that unaccusatives and unergatives are not structurally identical, and have differences that are generally not obvious in surface forms (Levin & Rappaport 1995).

Therefore, from our perspective, AJ is the more versatile and cost-effective tool, with a more varied set of specific diagnostics that can identify hierarchical relations, constituency relations, and varieties of long-distance relations, in contrast to priming's rather vague indication of structural similarity at some level. However, we acknowledge that there is a potential for priming and AJ to provide complementary evidence, under a more developed theory of priming than is currently offered.

B&P are quick to dismiss results from standard AJ diagnostics of constituency, pointing out the well known fact that different diagnostics do not always straightforwardly converge. This strikes

us as an odd strategy. Reliably conflicting results should prompt one to re-examine, rather than discard the data or the methodology. Constituency diagnostics do indeed produce apparent conflicts, under standard assumptions, but this is a gift to the researcher rather than a threat, as it invites us to dig deeper, and to better understand how the diagnostics work. Such efforts have proven fruitful (e.g., Pesetsky 1995, Steedman 2000, Phillips 2003).

As far as we can tell, the priming literature, including almost all studies carried out by the authors, depends on properties that are independently diagnosed by AJ. For example, in many studies the authors presuppose the existence of PPs, VPs, and NPs, all of which are constructs derived from AJ. If AJ data is as flawed as the authors suggest, then this is problematic for priming studies that take basic AJ findings as a starting point. It is unclear how constituency could be identified solely by priming.

Finally, B&P argue that priming evidence supports a "monostratal" view of syntax, i.e., a single level of syntactic representation, in contrast to transformational accounts in which multiple representations are related to one another by movement operations. We acknowledge the importance of the issue, but it is unclear how the priming evidence bears on it. Everybody acknowledges that sentences encode different types of relations: thematic roles, grammatical relations (e.g., subject, direct object), scope, topic/focus, etc. This is not in dispute. The disagreements surround the question of how these various relations are structurally encoded, and how the structural encodings are related to one another. Transformational accounts are one hypothesis about the relation between the encodings, but all accounts must offer an account of the same problems. What kind of priming result could, in principle, falsify the authors' belief that syntax is monostratal, and provide positive evidence for more than one level of representation? We know of no such evidence.

In sum, the authors should articulate a more explicit theory of priming, what it can and cannot diagnose, and how it relates to evidence from other tests. Priming evidence has the potential to complement AJ data. But priming evidence will be taken more seriously by those steeped in AJ (and other methods) if there is a genuine attempt to engage with the logic behind the various tests.

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