

whether a lexical item or construction that has undergone semantic change also has undergone syntactic reanalysis.

It is less clear, however, how to interpret situations in which priming effects appear in *different degrees* for different types of primes. B&P cite studies showing an enhanced priming effect when the prime and target share not only the same shallow syntactic representation, but also the same abstract syntax-semantics mapping (Griffin & Weinstein-Tull 2003; Raffray et al. 2014). Raffray et al. (2014) found that sentences with a coerced (missing) predicate (e.g., *The celebrity began the champagne*) primed target responses with a coerced predicate more effectively than did syntactically similar sentences with an event NP (e.g., *The celebrity began the speech*). However, these two types of NP-V-NP sentences were alike in failing to prime NP-V-VP responses (e.g., *The celebrity began drinking the champagne*). B&P take the latter fact to mean that the missing predicate of a coerced sentence is not represented in the syntax. But what, then, do we make of the fact that coerced predicate sentences primed coerced predicate responses more strongly than event NP sentences did? Raffray et al. (2014, p. 97) propose that speakers were sensitive to particular syntax-semantics mappings, in addition to being sensitive to shallow syntax. It seems, however, that one could plausibly interpret the different degrees of priming to signal some subtle difference in the syntactic representations.

Similarly, Griffin and Weinstein-Tull (2003) found that object-raising infinitives (e.g., *John believed Mary to be nice*) primed object-raising responses (as opposed to finite paraphrases) more effectively than object-control infinitives did (e.g., *John persuaded Mary to be nice*). Griffin and Weinstein-Tull (2003, p. 549) interpreted these findings to mean that language users are sensitive to the similarity of abstract syntax-semantics mappings between prime and target, because object-control infinitives have an additional argument role. As an alternative to this explanation, B&P suggest that perhaps the additional argument role in object-control sentences is associated with a distinct *syntactic* representation, meaning that reference to syntax-semantics mapping is not necessary to explain the results (note 6 in the target article). While Griffin and Weinstein-Tull's explanation is compatible with parallel-architecture theories such as *Simpler Syntax* (Culicover & Jackendoff 2005) and *Construction Grammar* (Goldberg 2006) – which do not permit null constituents in the syntax – B&P's alternative requires a null constituent in the syntax. Therefore, B&P's explanation is more compatible with the standard generative account of object control, in which the infinitive subject is a null pronoun (PRO) (Chomsky 1981). As B&P acknowledge, the evidence does not distinguish between these two possible syntactic representations.

Evidence from priming is useful for showing speakers' sensitivity to structural similarity. As such, it can indicate which elements must be included in syntactic representations (e.g., syntactic categories, constituent ordering) and whether historically related constructions continue to share a constituent structure. It cannot tell us, however, whether differences in structure, as reflected in differences in degrees of priming, are due to differences in syntactic representation alone or to syntax-semantics mappings. While structural priming provides valuable evidence for linguistic theory building, the abstract representational basis for any particular facilitation effect remains subject to interpretation. A psychologically plausible theory of syntactic and semantic representation, therefore, must take into account many different types of evidence, each of which has its own advantages and limitations.

The logic of syntactic priming and acceptability judgments

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Abstract: A critical flaw in Branigan & Pickering's (B&P'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.

Branigan & 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. Their case, however, 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 in which 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 diagnoses 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 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 converge straightforwardly. 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; this is a gift to the researcher rather than a threat, however, because it invites us to dig deeper and to understand better 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 are 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, in principle, could 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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Converging on a theory of language through multiple methods

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Abstract: Assuming that linguistic representation has been studied only by linguists using grammaticality judgments, Branigan & Pickering (B&P) present structural priming as a novel alternative. We show that their assumptions are incorrect for cognitive-functional linguistics, exposing converging perspectives on form/meaning pairings between generativists and cognitive-functional linguists that we hope will spark the cross-

disciplinary discussion necessary to produce a cognitively plausible model of linguistic representation.

We agree with Branigan & Pickering's (B&P's) general argument that, to understand how language works, we must take into account both representation and processing. However, we take issue with several of the specific arguments put forth by the authors. These are:

- That acceptability judgments are the only tool available to linguists;
- That structural priming is the only alternative to acceptability judgments;
- That linguists and psychologists don't work together to investigate linguistic representation.

These arguments are true only if we assume that linguistic representation is as defined by generative grammar: the result of an encapsulated system in which syntactic structure is divorced from meaning, and meaning is construed in formalist terms (Heim & Kratzer 1998; Schiffer 2015). Vis-à-vis the broader language research community, the generativist view is mischaracterized as enjoying global consensus when, in fact, it has been opposed rigorously for decades by other linguistic traditions—in particular, by researchers within the cognitive-functional tradition. The direct consequence of this myopia is imagining that the limitations built into the generativist paradigm, in fact, are limitations on the entire field.

It is a truism in science that how a question is asked determines the type of answers that can be sought. Cognitive functional linguistics diverges from the generative school by assuming that there is an intimate connection between the form of language and the meaning that it communicates, with the consequence that neither form nor meaning can be studied in isolation. With this assumption comes Lakoff's (1990) "Cognitive Commitment": to ensure that what is posited regarding linguistic structure and representation is in accord with findings from other disciplines regarding the mind and the brain, along with a recognition that the development of cognitively plausible theories of language will require dialog with scholars in neighboring disciplines (cf., Tomasello 1998; 2003b). These commitments establish from the outset that understanding linguistic representation must be a multidisciplinary undertaking. The fulfillment of these commitments can be found in the growing number of research articles and books investigating linguistic representation using dozens of methodologies either borrowed from, or developed in conjunction with, multiple neighboring disciplines. These commitments are so important that there is a long-standing workshop series, the Empirical Methods in Cognitive Linguistics Workshops, whose primary goal is to bring together researchers from a wide variety of fields who seek to strengthen their methodological repertoires for the study of language and cognition.

As a field, cognitive-functional linguistics has brought together the following methods, among others:

- Analyses of corpus data (e.g., Divjak & Arppe 2013; Yoon & Gries 2016),
- Behavioral experiments (e.g., Dabrowska 2014; Feist 2010; 2013; Liu & Bergen 2016; Bergen & Coulson 2006; Winter & Matlock 2013),
- Eye-tracking studies (e.g., Huettenlocher et al. 2014),
- Integration of acoustic and linguistic cues (e.g., Walker et al. 2013),
- Studies of the interface between language and perception (e.g., Winawer et al. 2008; Lupyan 2012),
- Gesture research (e.g., Núñez & Sweetser 2006),
- Linguistic analyses (e.g., Haspelmath 2008),
- Sensorimotor concepts (Ströbel 2016),
- Statistical analyses of cross-linguistic variation (e.g., Croft & Poole 2008; Feist 2008; Levinson & Meira 2003),
- Neuropsycholinguistic experiments (e.g., Van Petten et al. 1999; Coulson & Van Petten 2002; Saj et al. 2014; Perry & Lupyan 2014).