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Consigning Phenomena to Performance: A Response to Neeleman

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Ad Neeleman is a good representative of the large majority of modern theoretical linguists who work with generative-enumerative syntax (GES) theories. Like all GES theorists, he firmly defends the distinction between competence and performance, and of course I agree with him on that: no sensible grammarian wants or expects grammars to yield direct representations of the raw reality of human linguistic behaviour with all its flubs, false starts and lost trains of thought.

However, like most linguists who work with GES frameworks, Neeleman almost totally ignores the possibilities of model-theoretic syntax (MTS), and like many I have previously interacted with on this topic, in his response to my article he exploits the competence-performance distinction as a rationale for avoiding the force of my arguments. I do not find such conservatism surprising, but I do find it unfortunate.

My article gives an informal survey of what MTS is, and of why I think it deserves more attention than it has received. Neeleman basically says the same thing about everything I present or could present on the topic: whatever I bring up, he claims 'there is no need to burden the competence grammar with it' because it 'can and should be dealt with at the algorithmic level.'

What he is proposing is that linguists should cling to the hope that the almost entirely unknown mechanisms of human utterance processing will be able to deal appropriately, in currently unknown ways, with the entire array of phenomena that I suggest might make MTS look attractive as a general design for grammatical theories. Neeleman believes that syntacticians can rest easy with their decision to stick to GES theories for now, because the competence-performance distinction will rescue them at all necessary points.

In his 'can and should be dealt with at the algorithmic level' remark, I have to admit that he is probably right about the 'can' part. I am sure it is not beyond human ingenuity to invent algorithms that could in principle compensate for (or simply disguise) the problematic features of GES frameworks that I have pointed to. However, Neeleman does not do that. His references to algorithms are like cosmologists' references to dark energy, known to be out there somewhere but as yet neither empirically located nor theoretically accounted for in detail.

And his 'can and should' is overreach. It hardly counts as a response to my arguments simply to recommend that everything problematic *should* be allocated to

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the human parsing capability. My article makes a proposal entailing that grammars, viewed as theories of the structure of human language expressions, can be brought closer to the phenomena than GES allows them to be. I am proposing a way of narrowing the gap between grammar and facts, between theory and phenomena. It is hardly satisfactory to respond by saying never mind, we can think of ways in which we might be able to compensate for the consequences of not narrowing it.

I find Neeleman's response, therefore, a singularly disappointing one; it amounts to a refusal to engage.

Take the section of my article in which I point out that GES grammars define only a binary grammaticality distinction. They classify everything as either perfect or nonexistent—the only alternative to being grammatically impeccable is being nonlinguistic garbage and not having grammatical properties at all. But in reality, I point out, ungrammaticality seems intuitively to be a matter of degree: some utterances are much more ungrammatical than others. So there is a gulf between the phenomena and the theoretical account.

All Neeleman says to this, really, is that he doesn't intend to worry about that. The matter can be treated as relating to performance rather than competence. The judgments of grammaticality people make are a matter of performance, and 'we expect, irrespective of the nature of the competence grammar, to find variation in grammaticality judgments.'

I am familiar with this view, naturally. It is basically the one defended at book length by Carson Schütze (1996): it says that grammaticality is strictly boolean, and only the variable and idiosyncratic data of acceptability judgments exhibit gradience. I believe, on the contrary, that we should give some consideration to a way of formalizing grammars that rejects Schütze's view, and instead takes the view adumbrated by Howard Lasnik and others (see e.g. Lasnik and Saito, 1984, pp. 266ff; Lasnik, 2004, pp. 219ff): that grammaticality is gradient, probably along several dimensions.

And Neeleman simply says let's not go there.

In another section I point out that GES grammars make syntax depend on the accidental facts of what words there are at present. We ought to be considering a kind of grammar that permits the syntax of a language to be defined independently of the properties of particular currently existing words. Here Neeleman's response strikes me as completely off the rails (not that it isn't familiar from similarly misguided suggestions that others have made). He says that for utterance containing unknown words 'a good strategy might be to store the relevant form in a ''temporary lexicon'', and to try and identify a meaning for it (presumably, this is how new words are learned).'

Strategy? For 'how new words are learned'? Neeleman seems to be confusing grammars with people. I am not talking about the psychological matter of what adult or infant humans do when faced with a novel situation. I am talking about what a theory of grammar ought to say about linguistic phenomena suggesting that human languages do not have fixed a collection of words on which the definition

of their syntax dependes; rather the syntactic well-formedness of the expressions containing the words is defined independently of them.

The phenomena at issue for me do not have to do with the reactions of either adults or babies to linguistic material including words they are unfamiliar with; it has to do with the fact that expressions containing nonexistent words have syntactic and semantic properties. That is, I take it as a fact that *Pirots karulize elatically* is not nonlinguistic gibberish, it is a grammatical and meaningful declarative clause with a plural subject, a correctly inflected verb, and an adverb modifying the verb. Part of the evidential support that tells me this is that adult humans who know English respond to it thus (in that they seem to grasp that it refers to the elatic karulization capabilities of pirots, whatever they might be), but the claim is about grammatical properties, not about the millisecond-by-millisecond functioning of psychological mechanisms.

My suggestion is that we should consider the merits of a kind of grammar that makes a somewhat closer approach to accounting for such phenomena, by finding a way to define syntactic structure in a way that does not depend on the current list of lexical items. And Neeleman simply says, why bother.

Instead, Neeleman wants us to fantasize about hypothetical parsing mechanisms inside us that, in real time as we try to parse sentences containing *pirot* or *karulize*, will store each unknown word in a special mental box and 'try and identify a meaning for it.' This seems like anthropomorphic nonsense to me: I don't know what our unconscious parsing mechanisms are like, but I know that they operate below the level of consciousness, and talk about such mechanisms trying to identify meanings is talk about conscious purpose-driven efforts at understanding, which makes no sense here. It is you that tries to identify meanings of words, using the whole of your real-world knowledge and common sense; it is not some unobservable automatic subconscious mental system that you are complete unaware of.

My claims are about what sort of grammar best represents the apparent independence of syntactic structure from the accidents of the present contents of the dictionary. I want to minimize the need to imagine up mechanisms inside our heads that permit us to cope despite the (perhaps quite unsuitable) GES grammars inscribed in their brains.

The problem in attempting to discuss this with Neeleman is that he seems to be quite happy to indulge such imagining and shows no sign of wanting to give it up.

I also point out the peculiar phenomenon of 'quandaries': constructions that are mired in a sort of grammatical gridlock where no way of inflecting the words is correct. And Neeleman has nothing more to say than this:

> [I]t is entirely possible that in performance some of the ungrammatical candidate structures can be associated with the intended interpretation through repair mechanisms of various types, giving rise to an experience in subjects that is neither one of grammaticality nor one of ungrammaticality.

He seems to be saying nothing more than that maybe a performance mechanism can be rigged up that will have the right effect.

The competence/performance distinction is what GES-favouring linguists have relied on ever since 1965 on as a promissory note for future bridges over the chasm between what grammars say and what linguistic experience is like. I propose that we should narrow that chasm a little. But GES thinking and the distinction between competence and performance have both been so deeply embedded in the consciousness of linguists these last 50 years that linguists will only very rarely entertain a radical reconsideration of their assumptions.

My article presents a few points that are intended to spark a conversation. On the topics reviewed above, Neeleman just points to the competence-performance distinction. On acquisition, where I point to the radical difference between exact identification of the right GES grammar and incremental adoption of constraints until a rough approximation to consensus is attained, Neeleman says nothing at all. The conversation I hoped for hasn't really started yet.

Let me make one more point before closing this brief response. I presented the issue I wanted to discuss in terms of a question needing to be answered. The question is whether there are grounds for thinking that it has been a mistake for syntactic theory to concentrate so completely on GES modes of theorizing, and largely ignore the properties and consequences of MTS theories. This sharpening of the issue tends to make things adversarial, and that was my intention: when attempting to start a conversation about theoretical matters, it is useful to set out starkly a choice that has to be made, and to defend, as if against an adversary who thought otherwise, one of the choices. There is a danger, however, that the reader might think that I am presenting a case that GES as a whole has been falsified, or that some battle can be staged between MTS and GES, and MTS will triumph.

That would be unfortunate. My remarks are at way too high a level of abstraction for talk of falsification to be appropriate. We are not going to find some hitherto little-studied language, or some overlooked corner of English syntax, that shows us by its structure that GES is refuted and MTS is vindicated. The idea is preposterous.

The main reason it is preposterous is that, in certain defined ways, MTS approaches, as restricted to finite models, can be made to mimic GES accounts of linguistic data. Therein lies the importance of results like those of Büchi, Doner, and Rogers.

Büchi showed (and I will take the liberty of translating his theorem into modern terms adapted to linguistics) that for any GES grammar defining a regular (finite-state) set of strings there is an MTS grammar using monadic second-order logic on string models that defines exactly the same strings as grammatical, and the converse holds.

Doner showed, in effect, that for any GES grammar generating a context-free set of strings there is an MTS grammar using monadic second-order logic on tree models that defines exactly the same strings as grammatical (and in a definable sense exactly the same set of trees as well). Rogers generalized the

results of Büchi and Doner to an infinite hierarchy of larger families of formal languages.[1]

One of the most important aspects of MTS, in fact, is that it provides new ways of obtaining insights into the abstract character of the sets of structures defined by particular types of GES grammars, and particular varieties of parser. Let me give an example that touches on the topic of computational processing of syntax, on which Neeleman lays such emphasis.

Neeleman cites the work of Mitchell Marcus, who designed a parser adapted to transformational GES grammars. A careful description of this parser is presented in Marcus, 1980. That presentation is opaque concerning the characteristics of the languages that could be parsed by a Marcus parser. However, Nozohoor-Farshi (1987) was able to show that such a parser can only recognize context-free stringsets. By putting that result together with Doner (1970) we can immediately say, without any further work being needed, that if a language can be accurately parsed by a Marcus parser then there exists an accurate description of that language as a set of constraints in weak monadic second-order logic interpreted on trees.

Indefinitely many other results follow from that; for example, that there can be no arbitrary subtree identity constraints (parts of a sentence required to be structurally identical to some other part) in a language that has a Marcus parser. Copy theories of movement are thus incompatible with Marcus parsers, and so are theories of verb phrase ellipsis that posit syntactic identity between antecedent and unpronounced material.

What I am pointing out is that it is not necessary to treat GES as a theoretical citadel that needs to be protected from MTS attack (and Neeleman does seem to be pulling up the drawbridge and letting extended appeals to conjectural performance mechanisms serve as a moat). For those too conservative to like the idea that MTS offers better theories of linguistic phenomena, there is still a reason to pay close attention to MTS work, because it provides conceptual tools for gaining formally grounded insights into what precisely framed GES theories will (or will not) be able to describe, and why.

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References

Büchi, J. R. 1960: Weak second-order arithmetic and finite automata. *Zeitschrift für Mathematische Logik und Grundlagen der Mathematik*, 6, 66–92.

[1] The original papers (Büechi, 1960; Doner, 1970; and Rogers, 2003) are highly technical, and presuppose a professional level of acquaintance with automata theory and mathematical logic. For a presentation somewhat better adapted to the needs of linguists and philosophers, see Rogers, 1998.

Doner, J. 1970: Tree acceptors and some of their applications. *Journal of Computer and System Sciences*, 4, 406–51.

Lasnik, H. 2004: Pronouns and non-coreference. *University of Maryland Working Papers in Linguistics*, 13, 214–27.

Lasnik, H. and Saito, M. 1984: On the nature of proper government. *Linguistic Inquiry*, 15, 235–89.

Marcus, M. 1980: *A Theory of Syntactic Recognition for Natural Language*. Cambridge, MA: MIT Press.

Nozohoor-Farshi, R. 1987: Context-freeness of the language accepted by Marcus' parser. In *Proceedings of the 25th Annual Meeting of the Association for Computational Linguistics*. Morristown, NJ: Association for Computational Linguistics, 117–22.

Rogers, J. 1998: *A Descriptive Approach to Language-Theoretic Complexity*. Stanford, CA: CSLI Publications.

Rogers, J. 2003: wMSO theories as grammar formalisms. *Theoretical Computer Science*, 293, 291–320.

Schütze, C. 1996: *The Empirical Base of Linguistics: Grammaticality Judgments and Linguistic Methodology*. Chicago, IL: University of Chicago Press.