Constructions are not explanations

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Goldberg's response to my 'Constructions and Grammatical Explanation' (Adger 2013) doesn't really address the various arguments I gave in the paper. In this very brief rejoinder, I'll go through her paper point by point.

The first point I made in my paper was that there seems to be no a priori argument for why proponents of Usage Based Construction Grammar (usage based CxG) single out the human capacity for linguistic cognition from other human cognitive capacities (e.g. say, visual cognition) and claim that the former has no specialised structure to it, while the latter does. The CxG argument is that (innate) processes or biases of categorization, social cognition and statistical learning are sufficient for linguistic cognition; my point was that there is no a priori reason to believe this. Goldberg's response to this is of an Ockhamian sort: it is generally preferable to appeal to independently motivated systems in an explanation, and she takes it to be the case (somewhat tendentiously, see below) that the various other processes are independently motivated. Let us accept this for the purposes of establishing the point. It is still the case that these domain general processes or biases have to interact with particular structures in different domains of cognition. Presumably they do so in different ways in visual cognition, or in planning, or navigation. So the point seems to be that they have no special structure that they interact with in linguistic cognition. But, as I said, there is no a priori argument for this: there is simply a claim that there is special structure for vision, but no special structure for language. Presumably the argument is that we need special structure to explain how vision works, but that we can get away without special structure to explain how language works. But this would be an empirical, not an a priori argument. Hence, my point stands.

Goldberg claims that 'the other processes are independently needed', but she gives no argument for this at all. Her statement, as it stands, is equivalent to 'other processes have been independently proposed'. But these proposals have never been shown to be empirically adequate for even the smallest part of the empirical phenomena uncovered in generative or typological linguistics over the past decades.

Goldberg's second point is that these non-linguistic processes and biases have plausible evolutionary histories, while the putative syntactic processes and biases don't. This is just incorrect. Pinker and Jackendoff (2005) argue that the syntactic system (UG) is an adaptation whose essential properties arise because it is an evolutionary solution to the problem of communicating, via a mainly serial interface (phonetics), hierarchical semantic structures (cf. Pinker and Bloom 1990), while Hauser, Chomsky, and Fitch (2002) argue that UG is so small that it could easily have evolved essentially via a single mutation. One could say that these are not plausible evolutionary histories, while the suggestions that Goldberg makes are, but again, there is no argument here.

Goldberg then appeals to the primacy of explanations based on function, saying that the discovery of functional motivations means that there is no need to invoke innate structural biases. I won't cavil here about the use of the word 'discovery' but merely note that function follows structure: there is no function without appropriate structure. The issue then is whether structural changes to the human linguistic capacity which are dependent on function have taken place in humans over their evolutionary history, and this is simply not known. Structural changes can take place in the absence of function. Contrary to Goldberg's claim, there are actually a number of 'exaptive' accounts of the emergence of human symbolic communication. Tattersall (2008) provides a review and a pertinent comment on why functional motivations are not to be priviliged:

> After all, the origin of biological novelty is essentially a random affair invoving genetic copying error, and as a result no novelty can ever arise for anything. Natural selection and those other processes that are active in shaping evolutionary histories cannot generate new features, however desirable their possession might be; they can capitalize only on what is spontaneously presented to them. For the most part, indeed, those forces act simply to eliminate deleterious novelties. Only much more rarely do they promote the new; and even then they are not they cannot be creative forces that drive the new into existence. Tattersall (2008, 109)

Goldberg then makes a general terminological point that knowledge is learned so UG as unlearned knowledge of syntactic constraints makes no sense. However, this is a straightforward misunderstanding of the way that the word 'knowledge' is used in generative grammar, which was clarified by Chomsky back in the mid to late 1970s (summarized in Chomsky 1980). 'Knowledge of language' is a technical concept. Perhaps it's easier to think of knowledge of language instead as a 'cognitive capacity'; UG, then, is a set of biases which constrain the development of the cognitive capacity that underlies human being's language use. This is directly connected to the point I made in 'Constructions and Grammatical Explanation' about the work of Gallistel and others: animals have extremely specific constraints on their cognitive capacities, language is a cognitive

capacity, it would therefore be surprising if there were no specific constraints on it (not logically impossible, just surprising).

Goldberg's final point is that human languages are very diverse and that if we suppose that all human languages are 'underlyingly the same' (which no one believes in the way that Goldberg means it, see below) then it is a 'cruel twist of fate that language learners must face such rampant superficial variation'. I don't really understand this argument, I must admit. Unity versus diversity is all in the way you look at language. What I find impressive is the way that the same principles for the organization of structural configurations appear over and over again. I refer Goldberg to Adger, Harbour, and Watkins (2009) for a detailed investigation of Kiowa, a Native American free word order language whose diversity of word orders appears initially perplexing, but, when looked at carefully, reveals the interplay of powerful abstract organizing principles.

Goldberg additionally states that there is no longer any consensus of what UG means among generative syntacticians. I'd suggest that noone rely on Tomasello (2004) for a neutral view of what generative syntacticians think. There is a consensus that there is an object of study, that its properties are open to scientific investigation, and there is in fact much consensus as to what those properties are. I refer Goldberg to my 2003 textbook (Adger 2003), or many other textbooks in generative grammar aimed at graduate students, for an outline.

Further, Goldberg misunderstands the import of my point about C, L and G. My apologies for not being clearer. I was making a logical point: We all agree that there is cognition (C). We all agree that it's logically possible that some subset of cognition might be used for language (L) and we all agree that logically it could be the case that some part of L may be used for nothing else but language (G). The question is whether usage based CxG adopts the view that L exists (that is, if you buy that there is no L, that commits you to also buying the idea that all kinds of cognitive capacity are brought to bear when learning a language). If usage based CxG does not adopt the view that L exists, then, logically, it should be possible for colour to be used to specify a category that syntax is sensitive to, or that social cognition should be usable to determine word order effects. This is the point about probabalistic learning: there has to be some prior constraint on the hypothesis space. That's all that L is. If CxG denies L, then it needs to come up with some way of constraining the hypothesis space. I reiterate: I was emphatically not making a proposal; I was trying to clarify the relevant concepts.

Turning now to what I called G, this would be a subpart of L that is used for nothing else except language. Lets take this to be constraints on trees, dependencies etc. I take it that usage-based CxG denies the existence of G. But then it owes an explanation of the kinds of facts that I discussed in the paper (e.g parasitic gaps etc). It is notable that Goldberg does not provide such an explanation.

Goldberg says that, if we abandon the notion of UG, we 'no longer need to assume that all languages must be underlyingly the same' but further we can maintain that 'variation is not un-

limited'. I don't think that anyone has said that all languages are 'underlyingly the same' since people were discussing the universal base hypothesis in the 70s. When Chomsky says that there is only one human language, he's saying that there is one set of principles that govern all human languages, not that all languages are underlyingly the same. Generativists argue that all languages obey a certain set of principles (and indeed make proposals as to what those principles are), and that individual languages vary from those principles in constrained ways. It's important, when one is criticising a framework of ideas, not to criticise proposals that have been abandoned for 40 odd years.

Goldberg says that the formalisms of CxG are not indended to 'constrain language; instead they allow us to capture facts that are discovered'. She then gives an argument from Croft that says that no syntactic test will pick out all the entities that 'one might wish to call nouns, adjectives, subjects, or objects'. Great. I completely agree. If after investigation you come up with the insight that what one might 'wish to call' things is not what they are, that shows that the common sense notions of these words (noun, adjective etc.) is false. But that's exactly what generativists have been saying for over half a century. In fact, a fundamental generativist point is that this holds for the concept 'language' itself. And of course the fact that 'finding two constructions in two different languages that are absolutely identical in form, function and distribution is a rare occurrence' is hardly surprising when one doesn't take constructions to be real.

I do want to point out that nowhere in my paper do I make an assumption "that phonology *never* constrains syntax". I point out the asymmetry between a theoretical system that allows, for example, a rule (or construction type) linking coronality to topicality (as a framework like usage based CxG does) and a system that does not. That is, there is an asymmetry in the way that phonology and syntax interact. If syntax does not determine a particular form, leaving, say, a movement rule optional (or allowing optional Merge, or whatever), then there is no reason why prosodic, information structural, processing, etc. properties should not influence the frequency of one particular form or another. This was discussed in some depth in generative grammar from the very start (see Chomsky's work with George Miller for example). If the phonology-syntax asymmetry has a functional or diachronic explanation, I'd love to hear it. Contrasts in segmental phonology are very salient, so why not take some subset of these to behave in one way for information structural purposes, while taking the remainder to work in another way?

I don't have any particular beef with the section where Goldberg discusses cross-linguistic generalizations. I'm sure some of these are derivative from general principles that link form with semantic or information-structural function (in fact, I have proposed such general principles in my own work, as have many other generative syntacticians). But nowhere does Goldberg address the point I made with parasitic gap constructions.

In the section on constructions and functional categories, Goldberg mixes up functional cat-

egories and her neologism (neophrasism?) 'functional features'. A feature is just a property of an item that creates a contrast (cf. Trubetskoy, Jakobson, etc). There isn't an 'ever growing list' of functional features, although some have expressed concern, within generative grammar, about the ever growing sequence of functional categories proposed by the 'cartographic' school (Cinque, Rizzi). My general point was that generative grammar and CxG both attach semantics to structure. Structure is abstract (in the sense that it is not phonological), and so both approaches attach semantics to abstract structure. They are not to be distinguished in this way. Somewhat tangential to the point I made, but perhaps helpful is that they are actually distinguished in whether they allow incorporation of a constraining theory of how meaning attaches to structure: researchers working within generative grammar typically assume that structure building is compositional, while usage-based CxG places no such constraint on the structure-meaning mapping.

Goldberg says that functional categories are assumed to be part of UG and thus unlearned and universal, existing in languages and in individual sentences that show no evidence of them. No they are not. Let's take my 2003 textbook again, for example. There, a hierarchy of functional categories is proposed, in fixed positions, but crucially, individual categories in that hierarchy may be absent. All that is required is that, if they are present, they are in the same scopal order. I think this is the more or less generally accepted view in generative grammar, and even those that propose that all functional categories are always present (Cinque 1999 et seq, for example) do so while acknowledging that that is a tendentious assumption, or a position that needs to be argued for.

On learning, Goldberg simply states that there are new and exciting statistical approaches to learning and new facts that make it more than reasonable to assume that language can be learned without innate domain specific biases. But she does not tell us how these approaches can handle even simple cases like the parasitic gap case that I discussed in my paper. She finishes that section with the statement that 'if Universal Grammar consists only of recursion, as Chomsky now proposes (Hauser et al. 2002), it is unclear how exactly it could even begin to address the purported poverty of the stimulus issue'. Well, Chomsky does not propose that UG consists only of recursion; the proposal is that it consists of Merge, which is responsible for building recursive structure, and the mapping of that structure to the interfaces in the mind that deal with sound, meaning, etc. In fact, I even spell these two aspects of the proposal out in my paper, using roman numerals just to make it clear ("(i) a cognitive capacity used to create recursive structures; (ii) a capacity which connects these structures to, on the one hand, systems that involve externalisation as physical linguistic acts (vocal, signs etc) and, on the other, systems that involve internal computations such as thinking, planning, etc."). Poverty of stimulus is solved by an architecture for the human linguistic cognitive capacity that includes both Merge and the mappings (what are called, in Minimalist generative syntax, the Interface Conditions). For example, in the parasitic gap case I discussed,

one way to address this would be are constraints on interpretation ('rules of construal') which limit how gaps can be connected to antecedents, allowing this only in particular structural positions.

I hope this short rejoinder goes some way to clarifying why I think that usage based CxG, while no doubt containing many interesting ideas and analyses one might learn a lot from, is fundamentally on the wrong track.

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