Constructions Work

ADELE E. GOLDBERG[1]

Abstract

This paper provides responses to the points raised in this volume in an effort to evaluate, clarify and extend some of the arguments in Constructions at Work.

Keywords: constructionist approach, argument structure constructions, learning, categorization, island constraints, subject auxiliary inversion, argument realization, usage-based, universal grammar hypothesis.

It is gratifying to me for *Constructions at Work* (*CW*) to receive as much attention as it does in the present volume, and from such a wide range of respondents. I thank the respondents for the time and thought they have clearly put into their articles. In this response, I consider each of the points raised. Some of the critiques highlight questions that have been addressed in more recent work; others point the way to future research. Some, in my view, miss their intended mark, perhaps due to lack of clarity in my presentation.

As described in the companion summary article in this volume, *CW* is divided into three parts: i) Constructions, ii) Learning Constructions, and iii) Explaining Generalizations. I structure my response by addressing concerns that are raised about each part by the various respondents in the present volume. I would also like to acknowledge thoughtful reviews published elsewhere (Acuña-Fariña, 2006; Boas 2007; Hilpert, 2006; Bybee, 2007; Petré, 2007; van Valin, 2007; Ariel, forthcoming). For the sake of space, I state the content of each chapter only very briefly in this response.

1. Constructions

The three chapters contained in part I, "Constructions," provide an overview of the constructionist approach, an explicit defense of the eschewal of derivations in favor of surface structure generalizations, and an argument that we simultaneously learn both item-specific knowledge and generalizations over that knowledge. None of the respondents (nor other reviewers) explicitly take issue with these chapters. In the case of certain researchers (e.g., Bod, Croft, Langacker, Lieven) this is likely because although some of the case studies and specific arguments are new, the general approach is widely shared. But the silence from the mainstream generativist respondents (Lidz and Williams; Borsely and Newmeyer; Crain, Thornton and Khlentzos) to these arguments is somewhat surprising since the recognition of constructions, the adoption of a monostratal approach to argument structure, and the view of language as usage-based would seem to challenge basic assumptions of traditional generative grammar. Of course silence should not be taken as tacit assent.

On the Universal Grammar Hypothesis

At the same time that the specifics of the first part of the book are not challenged, Lidz and Williams and Crain et al. do take issue with an underlying point that is central to these chapters and indeed to the entire book: by emphasizing the role of domain-general factors, learning, and functional motivations for universals and syntactic generalizations, *Constructions at Work* implicitly questions the Universal Grammar Hypothesis. This hypothesis can be characterized by the following four interrelated claims:

1) Domain-specificity: Language acquisition is constrained by representations or principles that are specific to language.

2) Universality: These representations or principles are universal.

3) Innateness: These representations or principles are not *learned*.

4) Autonomous Syntax: These representations or principles depend on syntactic representations and not their functional correlates.

The fourth claim, Autonomous Syntax, is sometimes treated as independent of the first 3 claims (Borsely and Newmeyer argue in favor of 4 without defending 1-3; conversely Crain et al. argue in favor of 1-3 on the basis of a conceptual generalization, thereby ignoring 4). But traditionally the four tenets have all been embraced under the heading of Universal Grammar (as they are by Lidz & Williams).

Is the Universal Grammar Hypothesis true? Clearly no one today believes we are born blank slates. Our biological endowment is what separates us from the star-nosed mole. This is not the issue; the question is whether what separates us involves unlearned linguistic (i.e., *domain-specific*) representations concerning syntax. Clearly, too, everyone recognizes that there are some universals; the question is whether the universals make reference to autonomous syntactic generalizations, or whether instead they are explicable in terms of domain-general abilities and/or the semantics or pragmatics of the constructions involved (cf. also Bates 1993, Newmeyer 2005 for relevant discussion). Finally, we can all agree that adults have representations that are specific to language (for example, their representations of individual constructions); the question is whether these representations can be learned.

Lidz & Williams point out that we cannot conclude from the fact that *some* aspects of language acquisition rely on domain-general processes that *all* aspects of language rely on domain-general processes. Of course this is a logically valid point (see in fact Crain et al.'s discussion of the fact that all speakers readily recognize the lack of a valid inference in this general situation). I don't claim that every aspect of language learning is currently understood in terms of domain-general processes. The issue is, in accounting for language acquisition, should we assume that domain-specific representations that make no reference to function are required? Or should we instead *aim* to explain language learning on the basis of domain-general (i.e., independently needed) processes and constraints and/or by appealing to the functions of the constructions involved? Occam's razor would dictate that the latter is the

preferred strategy, since domain-general constraints and processes are by definition, independently needed, as are the functions of the constructions.

Of course, the simplest hypothesis is not always the right one. The notion of a "universal grammar" clearly captures the imagination; it would provide a tidy answer to the perennial question of what makes humans special. It made sense for researchers to explore the possibility of a universal grammar at the time it was proposed (Chomksy 1965), when an understanding of the power of statistical learning and induction were a long way off. But now decades have passed and we are no closer to knowing what sort of representations (or constraints) are included in "Universal Grammar" (see also Tomasello, 2004; Goldberg 2008).

The critical question is whether we need to appeal to unlearned, syntactic principles that are domain specific. In what appears to be a startling concession, even Chomsky himself has acknowledged that he remains unconvinced by any proposals for domain-specific linguistic principles, with the (possible) exception of recursion (Hauser, Chomsky & Fitch, 2002).[2] (Crain et al.'s defense of UG is discussed in section 4).

The second three chapters of *CW* focus on how constructions are learned, why they are learned, and how they are constrained.

2. Learning Constructions

The construction-learning experiments described in chapter 4 of *CW* demonstrate that a novel construction (i.e. a pairing of both novel form and novel meaning) can be generalized after only 3 minutes of exposure. Lidz & Williams note that these construction-learning experiments leave open the issue of exactly how specific the learning involved was. This is a reasonable point. More recent research has revealed that, in fact, the learning appears to be quite specific and also quite robust. Undergraduate learners are able to *produce* the novel construction after only three minutes of exposure. The fact that participants are able to use the newly learned construction to describe novel scenes indicates that they are treating what they have learned as language, and that their knowledge of this language is strong enough to be accessed for the sake of production. Moreover, these new results demonstrate an awareness of the specific linking pattern involved: subjects correctly produce $<NP_{theme}\ NP_{locative}\ V>$ order if they are trained with that order, whereas they correctly produce $<NP_{locative}\ NP_{theme}\ V>$ order if they are trained on the latter order (Boyd & Goldberg, forthcoming). Subjects also retain knowledge of the construction after a 7-day delay (Boyd, Gottschalk & Goldberg, forthcoming).[3, 4]

As discussed in *CW*, we have found that when overall type and token frequency are controlled for, input that is skewed such that a single nonsense verb accounts for half of the tokens leads to more accurate generalization than input that is more representative (Casenhiser & Goldberg, 2005; Goldberg, Casenhiser & Sethuraman, 2004). These findings are supported by work in general categorization that has similarly found that input that is skewed such that it contains more prototypical instances is advantageous as compared to more representative input.

Croft notes that the advantage of skewed input appears to be at odds with Bybee's observation that high token frequency correlates with irregularity (Bybee, 1985; 1995). As Bybee notes, isolated morphological exceptions require high token frequency to be effectively accessed; low frequency irregulars are more likely to be regularized, presumably because they are not sufficiently entrenched. But this fact should not be misconstrued to entail that the converse holds: that high token frequency necessarily inhibits generalization. *CW* observes a critical difference between the morphological cases of high productivity and low-generalization that Bybee discusses on the one hand, and our experimental work on novel construction learning on the other (pg 90). In the case of morphology, high frequency forms likely receive little internal analysis, as Bybee proposes. (This is possibly due to the fact that high token frequency leads to reduction, and reduction leads to internal opacity.) The novel argument structure construction on the other hand must be analyzed, since each token involves distinct arguments. Bybee herself does not take issue with our findings (Bybee, 2007; Bybee 2008). Moreover, the findings have been replicated in more than one experiment (cf. also Kidd, Lieven & Tomasello, 2006) and have now been successfully modeled (Borovsky & Elman, 2006; Perfors, Kemp, Tenenbaum & Wonnacott, 2007).

Whether or not a construction has "skewed (token) frequency" is determined by considering, the conditional probability that a verb occurs in a given construction ($P(verb_i \mid construction_k)$). For example *make* accounts for 20% of the tokens of the *way* construction, far more than any other single verb (Goldberg, 1996), so tokens of the construction are skewed toward *make* ($P(make \mid way\ construction) = .20$, $P(verb_{i=/=make} \mid way\ construction_k) < .05$)). It is not clear that the fact that *make* is frequent overall in the language with a range of different senses is relevant; the fact that the construction is relatively rare in the language is likewise probably irrelevant; whether or not *make* happens to be the very first verb uttered in the construction is also not relevant (see *CW* chapters 4-6). What *is* relevant to determining whether the frequency of an argument structure construction, K, is skewed is the set of conditional probabilities, for all verbs, i=1 to n, that appear in K, ($P(verb_i \mid construction_k$).

"Constraining generalizations" (chapter 5) suggests that statistical preemption can do much of the work often ascribed to frequency ("entrenchment") and that degree of openness may supplant the work often ascribed to type frequency (see Goldberg, this volume, for a brief summary). Thus Croft's description of the chapter, which does not adjudicate among the four factors, is not quite accurate. Both statistical preemption and degree of openness are argued to be in evidence in other cognitive domains so that the fact that they are operative in language learning is independently motivated. As Lieven's article emphasizes, the parallels in other cognitive domains is an intended theme of the book.

Lidz & Williams are critical of the claim that speakers have both verb specific and construction-level information about argument structure (Goldberg 1995; *CW* chapter 3-6). They appear to favor the idea of *only* recognizing construction-level generalizations, but they do not address *CW*'s critique of such

proposals (see pgs 205-213), nor the motivation for recognizing item-specific knowledge (*CW* 49-58; cf. also Goldberg 1995 chapter 5).

Lidz & Williams also point out the truism that learning requires prior biases of one sort or another. One wouldn't know on what basis to generalize if all input were weighted equally without any a priori similarity metrics or attentional biases. And yet this general fact does not require that the metrics or biases must be domain-specific.

"Why constructions are learned" (chapter 6) has two main parts. The first part offers experimental and corpus-based analyses that indicate that constructions are at least as strong indicators as verbs of "who did what to whom." Croft raises a concern that the analyses may be biased toward constructional meaning, but this is not the case. Verbs have long and widely been claimed to supply just the sort of meaning at issue ("who did what to whom"), and in fact, our analyses demonstrate that verbs *are* roughly equally good predictors of this level of meaning. The second part of the chapter discusses structural priming findings that indicate that we generalize on the basis of *both* form and function, an idea consistent with the constructionist approach.

3. Explaining generalizations

"Island constraints and scope" (chapter 7) brings together and extends a body of previous research that indicates that information structure properties of constructions underlie their sensitivity to long-distance dependencies and scope assignment. In particular, constructions that are "backgrounded" are islands, and more topical arguments have wide scope over less topical arguments.

In a separate review article, Bybee (2007) is critical of the use of constructed examples instead of attested data in this chapter. She argues that grammaticality should be equated with familiarity, and suggests that the apparent ill-formedness of certain island violations stems from the lack of similar examples in the input. It is true that intuitive judgments can be unreliable at times, but ill-formed examples must be invented rather than discovered, since they don't occur often in natural speech, and when they occur they don't come marked as ill-formed.

To further investigate the claims made in *CW*, and as a step towards addressing the issue of judgment reliability, Ambridge and Goldberg (to appear) collected judgments systematically in a carefully controlled study. We compare the relative island status of the following sentence complement constructions: "bridge" verb complements, manner-of-speaking verb complements and factive verb complements. These sentence types are investigated because they allow us to control for overall length and complexity in a straightforward way. We report a strikingly strong correlation between acceptability judgments and a negation test used to operationalize the notion of "backgroundedness." This provides strong empirical support for the claim that, in the case of sentence complement constructions, information structure properties underlie island effects.

Croft raises an important point when he notes that the cross-linguistic differences in island constraints cannot be fully explained by any categorical generalization, be it syntactic or information structure theoretic. The Ambridge and Goldberg (to appear) findings, in fact, strongly support the idea that constructions act

as islands to wh-extraction *to the degree* that they are backgrounded in discourse. Acknowledging certain cross-linguistic differences, we further note that languages appear to select different cut-off points in how backgrounded a constituent may be while containing a gap (cf. Erteschik-Shir, 1973; Fodor, 1991 for similar suggestions). Thus the recognition that the effects are gradient is central to our more recent work.[5]

Subject-Auxiliary Inversion

The discussion of subject-auxiliary inversion (SAI) in chapter 8 of *CW* is clearly, by far the most controversial part of the book. It is the sole focus of Borsley and Newmeyer's critique; it is also a centerpiece of Lidz & Williams's response. It is important to bear in mind that the functional motivation suggested for SAI in chapter 8 is the least important to the constructionist's overall aims (although see Goldberg, this volume, for further evidence in its favor). It may turn out that there are recurring surface syntactic patterns that have no functional motivation—we know that in the domain of phonology, generalizations about form are certainly made.[6] This is a point that Lieven and Croft also make. The existence of purely syntactic generalizations would only be a powerful blow to the constructionist (non-UG) approach if it turned out that such syntactic generalizations formed an unlearned system of domain-specific representations or principles. But the facts surrounding SAI do not involve a system of syntactic generalizations. Moreover, the surface form of SAI is learnable, and its complicated distribution requires recourse to a usage-based constructionist account.

Chapter 8 counters Newmeyer's (2000: 46) discussion of SAI in which he claims that SAI involves a system "with formally defined elements entering into systematic interrelationships governed by an internal algebra." He thus uses SAI to argue for Autonomous Syntax, traditionally a pillar of the Universal Grammar Hypothesis. Newmeyer had suggested exactly two interrelationships: 1) only the highest auxiliary in the full clause containing the subject is inverted and 2) SAI is claimed not to appear in subordinate clauses. *CW* points out that 2 does not hold (as is acknowledged by Borsley and Newmeyer); for example, exclamatives don't normally appear in subordinate clauses, but they can as long as a subordinate clause is used to convey the speech act of an exclamative as in (5):

(5) Junie B. knew that boy, was she in trouble! (*CW* ex. 36a pg 180).

So the "algebra" of "interrelationships" reduces to the fact that SAI involves the highest auxiliary in the full clause containing the subject. That this auxiliary is involved and not the first auxiliary in the linear string follows from the functional motivation suggested in *CW*,[7] since it is the auxiliary that may express the polarity of the clause containing the subject that inverts with the subject.

But even without any functional motivation, simple recurrent networks (SRNs) can learn to invert the correct auxiliary on the basis of simpler input that children uncontroversially receive (cf. Lewis and Elman 2001; Reali and Christiansen 2005). In particular, the models are able to generalize correctly to produce complex questions (e.g., *Is the man who was green here*?), after they received training only on simple questions and declarative statements with a relative

clause. The networks take advantage of the fact that both simple NPs (*the boy*) and complex NPs (*The boy who chases dogs*) have similar distributions in the input generally (see also Bod, forthcoming and Perfors et al. forthcoming for symbolic systems that learn SAI). The models predict that learners should occasionally make errors when they involve local strings that are likely to occur in the input, and just this sort of error does occasionally occur in children's productions (Rowland 2007; Ambridge et al. to appear).

Beyond the fact that the auxiliary inverts with the subject, there are a host of idiosyncrasies: in some constructions, only a subset of auxiliaries can appear (counterfactual conditionals, wishes/curses); some constructions prefer lexical subjects and disallow third person singular subjects (comparatives); in some constructions the inversion is obligatory (y/n questions) in others it is optional (comparatives); some constructions require an initial phrase of a certain type (exclamatives, negative and positive rejoinders); some constructions require VP ellipsis in the inverted clause (comparatives). Clearly any system that allowed any subject to invert with any auxiliary would overgeneralize rampantly in a way that children are not likely to do. A non-constructionist, general syntactic account would predict that children should produce the following sorts of overgeneralization errors found in 6a-e. However, it seems highly unlikely that children actually make such errors.

6. a. *Should Mom orphan you! (Curse with SAI involving *should* instead of *may*)

b. *I am faster than is it! (Comparative with third person subject referring to, say, a train)

c. *Many a night hasn't my sister wet her bed. (M*any* $NP_{\text{indefinite}}$ *SAI* construction; it cannot occur with negative auxiliaries)

d. *Actually have I learned this very well. (SAI with positive adverb *actually*).

e. *Better you go! (SAI with *better,* which otherwise acts as an auxiliary but does not allow SAI; see Sag forthcoming)

Moreover, children also do not suddenly produce all SAI constructions at the same time. For example, they learn to produce (certain) yes/no questions well before they ever produce SAI in comparatives, and many speakers never do invert in comparatives.

Borsley and Newmeyer observe, "It is certainly the case that within a construction-based approach it is necessary to recognize a number of SAI constructions, each with its own semantic-functional properties." Lidz & Williams adopt a generative proposal that SAI's properties follow from a general process of "head-raising" to a silent complementizer position (how example 5 would be generated on this view is decidedly unclear since there is an overt complementizer in the complementizer position); but there is no explanation of why this very general process results in SAI in only a handful of constructions in only a few languages, nor an account of how the functions of the inverted and uninverted versions differ.[8] Clearly recognizing the particular constructions involved is necessary for *any* account: each of the constructions has distinct formal and functional properties and

there is no way of strictly predicting which constructions will have SAI and which not. It is just as possible to stipulate in a constructionist framework as it is in a generative one (cf. Croft's suggestion), but the question is whether the set of constructions that exhibit SAI are best simply stipulated, or whether instead they should be recognized to form a network of related cases. The data discussed in *CW* (and in Goldberg this volume) argues that the constructions are related; psycholinguistic experiment would be required to demonstrate that these relationships are mentally represented. In any case, it is clear that the formal generalization does not extend beyond the simple fact that non-canonical ordering of the highest auxiliary and subject exists.

Universals of argument realization

Lidz & Williams appear unimpressed by the domain-general explanations of linguistic universals proposed in chapter 9, "Cross-linguistic generalizations in argument realization." But they do not address any of the proposals made in that chapter (e.g., about linking or argument omission; see Goldberg this volume). In my mind, perhaps the best candidate for a universal that is readily explained by domain-general processes involves the generalization proposed by Lidz himself as evidence for Universal Grammar (Lidz et al. 2003). Lidz had proposed a principle of UG that would dictate that the number of semantic arguments should match the number of syntactic complements (cf. the "theta criterion"). My critique of the claim is that the general tendency (and it is clearly only a tendency) follows from Gricean principles: if an argument is expressed it is assumed to be relevant and if an argument is relevant and non-recoverable then it must be indicated. There is a lengthy discussion of the idea in *CW* that I won't repeat here (pgs 187-197; cf. also the short description in Goldberg this volume, section 9). *CW*'s discussion also addresses Lidz's subsequent defense of his proposal, finding it lacking (194).[9]

4. Other generalizations

Crain et al.'s discussion actually does not make contact with the content of *CW*, but their observations are nonetheless worth addressing. Their critique rests on the existence of certain conceptual facts and a certain constraint on coreference possibilities. They claim that an account of these facts requires appeal to Universal Grammar. We could follow Scholz and Pullum and observe that "It would surely be a pyrrhic victory if linguistic nativism were true simply in virtue of one solitary unacquired or unlearned contentful linguistic principle, everything else being learned" (Scholz & Pullum, 2006). Yet it is worth trying to address the proposed universals insofar as they force us to think deeply about language.

It has been a strength of generative linguistic analyses that they have sought explanations for the often complex ways in which individual constructions interact: what are the scope possibilities when construction A unifies with construction B and why do only these possibilities exist? How does negation interact with construction C? Much work in constructionist approaches, including my own, has emphasized

the properties of individual constructions, but it is clearly important to focus on their interactions as well.

The semantic facts raised by Crain et al. culminate in the observation that "in negated universally quantified statements…disjunction licenses a conjunctive implication in the predicate phrase, but not in the subject phrase." As stated, the generalization sounds rather arcane, so that it really does seem to raise a learnability problem: what would possess a child to come up with such a generalization, particularly if we grant that the relevant sentences are likely rarely encountered? However, it is possible to untangle the generalization and see that only very basic entailments that any rational system would make are required.

We first need to recognize the basic fact that universally quantified statements can generally be captured by conditionals: All men are mortal if and only if, for all entities x, if x is a man, then x is mortal. Thus a negated universally quantified statement can be captured with the generalization in (i) (I use the formal semantic notation here only for convenience; the use of these conventional symbols is inconsequential to the point). From the representation in (i) we can deduce the representation in (iv).

i) $\neg \forall x (Px \rightarrow Qx)$ Representation of a negated universally quantified statement: "It's not the case that all P's are Q"

ii) $\neg \forall x (\neg Px \vee Qx)$ Meaning of $\rightarrow$, inclusive "or," negation

iii) $\exists x \neg (\neg Px \vee Qx)$ Meaning of "for all" and "there exists"

iv) $\exists x (Px \& \neg Qx)$ Meaning of "and" and inclusive "or" (DeMorgan's law)

Q, the predicate phrase in the statement being represented in (i) is negated in (iv); it is negated whether it is a simple predicate "is a tomato" or whether it happens to be a disjunction. Thus a negation of a universally quantified statement entails that the consequent but not the antecedent is negated. We have thus arrived at Crain et al.'s observation by appealing to simple principles of general rationality. Generalizations that depend on meaning are not evidence for the Universal Grammar Hypothesis insofar as UG is supposed to involve principles of syntax.

An important moral that can be drawn, however, from Crain et al.'s example is that interpretation often depends on larger sentential contexts. This is clearly true. In fact, interpretations often depend on discourse context and world knowledge as well. Consider the following examples:

(7) When it rains in California, everyone is happy. (Charles Fillmore, p.c. circa 1987)

(8) Is everybody present? (Geurts 2000:528)

In (7), the understood range of the universal quantifier is "everyone in California" since people in, say, New Jersey, don't generally care if it rains in California.[10] The narrowing of the domain of discourse is conveyed by the antecedent clause. In (8), the understood range of the universal quantifier is "everyone who is expected to be here" a narrowing of the domain of discourse that comes from the discourse context.

Thus although appeals to very local contexts depending only on *n* immediately surrounding words (n-grams) are currently popular in usage-based accounts, it is often clearly necessary to appeal to sentential and contextual information in order to arrive at natural interpretations (see, e.g., Lakoff 1987; Langacker 1987).

The second generalizations that Crain et al. offer is "Principle C," namely that lexical NPs or proper names must not be c-commanded[11] by their antecedents. Thus in *He said Max was drinking wine*, *he* cannot refer to Max since it c-commands *Max*. Crain et al.'s argument is stated in general terms: children are said to be unable to learn an absence of an interpretation. Therefore the generalization must be unlearned or innate. This argument is falsified by the observation that children routinely do learn the absence of interpretations when it comes to word meanings. That is, they learn what thousands of polysemous words mean and *what they do not mean*. Children have to learn that *man* refers to either all humans or only male humans, while *human* refers to humans and *not* only male humans. No one wants to say that the meanings of individual words and their corresponding labels in each individual language are in any sense innate; clearly children have to learn what the individual labels in their language refer to and what they do not refer to.

Still, the statement in terms of c-command remains sufficiently elegant, syntactic, and mysterious that it has convinced some of the validity of Universal Grammar. Unfortunately the facts on the ground are less elegant and less purely syntactic (albeit possibly as mysterious) as they appear at first. MacWhinney for example notes the following contrasts:

(9)a. *She$_i$ came back from vacation when Mary$_i$ saw a stack of unopened mail piled up on her front door.

b. She$_i$ had just come back from vacation when Mary$_i$ saw a stack of unopened mail piled up on her door. (MacWhinney 2005).

The syntactic relationship between *she* and *Mary* remains the same in both sentences (most analyses would recognize *she* to c-command *Mary*) and yet (9a) is unacceptable while (9b) is perfectly natural.)

Rinehart (1983) noted that an impossible interpretation exists across sentences as well as within them, a fact that is not easily explained by appeal to any existing version of Universal Grammar since UG pertains to isolated sentences not connected discourse.

(10) # She$_i$ called. Mary$_i$ came in.

Crain et al. do not discuss these or other existing counterexamples to the c-command account, and do not acknowledge that alternative proposals aim to address the fuller range data (e.g., Culicover and Jackendoff 2005; van Hoek 1995; Levinson 2000; MacWhinney 2005).

There have been many other intriguing proposals for aspects of UG that I have not discussed. Clearly each of these should ultimately be considered on its own merits. What chapter 9 of *CW* aimed to do was to consider several of what I took to be the *most* compelling generalizations that appear to argue in favor of Universal Grammar within the domain of argument structure; it was argued that in none of

these cases, is it not at all obvious that UG is required. As noted in section 1, whether this strategy is legitimate depends on what we take the null hypothesis to be. If we assume UG until proven otherwise, then I (and really anyone) am destined to fail, since it is impossible for any one person to fully address all of the UG oriented proposals that exist. The goal in chapters 7-9 was to convince readers that the other tack was worth pursuing: that by paying close attention to functions and frequencies as well as to form, insightful and non-stipulative explanations may be discovered.

5. On Formalization

Bod correctly observes that *CW* provides no "input-output procedure" or explicit formal model that generates new utterances on the basis of previous utterances. He cites much relevant exciting new work in computational modeling (although much of it is aimed at parsing and not at production or interpretation, the latter being much more daunting tasks).[12] Aside from the fact that I am not a computational linguist, my only hesitation in fully embracing symbolic algorithms is that they are typically based primarily if not entirely on distribution. As my own focus is on the specific lexical semantic and information structure aspects of constructions, the existing models do not serve my purposes, although they certainly provide existence proofs that many generalizations are implicit in the statistical information of large corpora.

Crain et al. are critical of the fact that I do not use formal semantic notation. I began graduate school in a joint math and philosophy program at Berkeley that focused on the study of logic. I do understand the appeal of necessary and sufficient conditions, set theoretic notations, and explicit deductive reasoning; I am also aware of the large body of insightful work within the formal semantics tradition. But early on, I became convinced that, formal semantics is not a very good representation for linguistic semantics because it is based on set theoretic notions and does not take into account lexical semantics, polysemy, motivation, or effects of world-knowledge (see Fillmore 1977; Jackendoff 1983; Lakoff 1987; Langacker 1987 for discussion of the relevant issues).

I am delighted that more than one formalism is available to constructionist approaches if one is so inclined. Sign-based Construction Grammar, being developed by Fillmore, Kay, Michaelis and Sag, uses explicit and comprehensive unification-based as well as formal semantics formalisms. Jackendoff's conceptual semantics offers a different multi-tiered symbolic representation. Langacker's Cognitive Grammar offers yet another comprehensive formalism with much merit. My own humble preference is to simply state ideas as clearly as I know how. Sometimes this requires formalization, but often it does not.[13]

6. On processing

Bod observes that a full processing account is not proposed in *CW*. We know many things about language processing: comprehension is incremental and involves an interaction of semantics, pragmatics and world knowledge along with syntax; we know that more than one sense of an ambiguous word is accessed in certain contexts; We know that the use of one word or construction can have a small but measurable positive impact on whether the same word or construction is reused. We know that

characteristics such as animacy, concreteness and frequency play a role in access. But at the end of the day we have very little understanding of exactly how we choose and combine constructions during on-line production, nor how constructions are deconstructed or interpreted in real contexts and in real time. Language processing strikes me as one of those fields in which everyone is examining a different small part of a large elephant; no one has managed as yet to see the entire animal. This issue is likely due to the fact that the issues involved are diverse and complicated. But the outstanding issues in this domain are in no way unique to the constructionist approach.

Some of Lieven, Bod and colleagues' recent work has begun to address a certain key part of the production process, as they propose constraints on how new utterances can be related to familiar or rote expressions. For example, Dabrowska and Lieven (2005) find that 90% of children's utterances in a dense database of 2 and 3 year olds' speech can be accounted for as either rote repetitions or minimal variations on children's own previous utterances. This work is very exciting and may well ultimately lead to clear constraints on the production of novel utterances.

They offer a "trace-back" model in which "the production of novel expressions involves the combination of symbolic units using two operations: juxtaposition and superimposition" (Dabrowska and Lieven 2005: 442). Superimposition (or "insert") involves filling a slot in a frame (or construction) with a phrase that fits the semantic and syntactic requirements of that slot (based on previously learned phrases) (cf. also Lieven et al. 2003; Bod 2006; Bannard et al. forthcoming for related models, although with somewhat less emphasis on semantics); this "slot-filling" is clearly important to allowing for generalizations and some version of it is assumed in all constructionist frameworks.

The trick is in deciding when a phrase fits the semantic and syntactic requirements. This is in fact the age-old question of partial productivity: determining when and to what extent slots are productive arguably depends on the degree of openness (the range of previously witnessed fillers in that slot) and statistical preemption (see *CW* chapter 6), although many NP slots are readily generalized from early on, even after exposure to a single NP filler (Tomasello et al. 1997). Processes of *coercion* are also relevant (Michaelis 2004; Jackendoff 1997).

Some versions of the traceback model allow for a second type of operation: juxtaposition. Juxtaposition involves simple concatonation of two frames when the two frames would be acceptable in either order (e.g., for vocatives and sentential adverbs). One question that arises is the following: If one generates a set of utterances using an analogous "trace forward" method on the basis of previously repeated utterances, is the resulting database consistent with the types of utterances children produce? Or would it overgenerate or undergenerate, producing strings that are more complex, more error ridden, or alternatively less creative than children actually produce? If the model does not overfit or underfit the data, it would provide a compelling demonstration that children's early utterances can be accounted for by these elementary operations (see Bannard et al., experiment 2, forthcoming for some relevant results).

7. A growing convergence

Langacker's article highlights the shared assumptions between Cognitive Grammar and the constructionist approach *CW* aims to support. This is of course no accident as "the influence of Cognitive Grammar upon Cognitive Construction Grammar is hard to overestimate" (Goldberg 2006: 220). Langacker argues that even the few points of disagreement noted in *CW* are overstated. I apologize if I misunderstood Langacker's (2005) remarks, and the agreement is welcome. In fact, reference to "a constructionist approach" throughout *CW* is explicitly intended as an umbrella term to unite the many grammars that have been given proper names, including both Cognitive Grammar and Construction Grammar.

As Lidz & Williams note, the constructionist approach shares much with LFG and HPSG as well, insofar as each is monostratal and eschews silent elements. Convergence across these frameworks is in fact steadily growing: Bresnan, the main architect of LFG, has moved toward a thoroughly usage-based, statistical approach to linguistic knowledge (e.g., Hay and Bresnan 2006). Sag, one of the major architects of HSGP, has been involved in developing a constructionist account of many traditional phenomena and is currently collaborating with Fillmore, Kay and Michaelis on a comprehensive monograph on (sign based) Construction Grammar (Sag forthcoming; Fillmore et al. forthcoming. Culicover and Jackendoff's "Simpler Syntax" proposals are also very much in the constructionist spirit (Culicover and Jackendoff 2005). Bod further notes much convergence within recent computational frameworks (e.g., Bod 2006; Steels 2005; cf. also Borovsky and Elman 2006; Perfors et al. 2007). Tomasello and colleagues' work adds evolutionary and comprehensive developmental dimensions (e.g., Herrmann & Tomasello, 2006; Tomasello, 2003; Tomasello, Carpenter & Liszkowski, to appear). Bybee and others' work provides an important diachronic dimension (e.g., Bybee 1985; 1995). When it comes to theory building, I am definitely a lumper and not a splitter. The (mostly minor) differences among various cognitive, functionalist or usage-based approaches pale in comparison to the stark contrasts between these approaches and traditional generative grammar.

Perhaps not surprisingly, computational linguists wish *CW* were more formal and computational (Bod); typologists wish the work was more typological (Croft); and generativists would prefer to see more discussion of universal grammar proposals (Crain et al., Lidz & Williams). I appreciate that the reviews published elsewhere as well as in this volume as well have generally asked how the proposals might work in new contexts, with the aim of better enabling us to address language's myriad complexities (e.g., Acuña-Fariña, 2006; Ariel, forthcoming; Boas 2007; Bybee 2007; Hilpert, 2006; Lieven, this volume; Petré, 2007; van Valin 2007).

The constructionist approach, like others, is clearly a work in progress. We need many researchers with different areas of expertise working together with a common aim of trying to understand Language without appeal to mysterious stipulations. I do not pretend to know all of the answers, or to have contributed much to many of the questions. Obviously a huge team effort is required. But it is clearly premature to retire constructions, as they remain at work.

References

Acuña-Fariña, J. C.
2006 Review of *Constructions at Work: the nature of generalizations in language*. *Folia Linguistica* 40, 391-9.

Ambridge, Ben, Carolyn Rowland, Anna Theakston and Michael Tomasello
2006 Comparing different accounts of inversion errors in children's non-subject wh-questions: 'What experimental data can tell us?' *Journal of Child Language* 33 (3): 519-557.

Ambridge, Ben & Adele E. Goldberg
To appear The island status of clausal complements: evidence in favor of an information structure explanation. *Cognitive Linguistics.*

Ariel, Mira
Forthcoming Review of *Constructions at Work: the nature of generalizations in language*. *Language.*

Bannard, Colin, Elena Lieven and Michael Tomasello
Forthcoming Modeling children's early grammatical knowledge. Max Planck Institute for Evolutionary Anthropology, Leipzig, Germany.

Bates, Elizabeth
1993 Modularity, Domain Specificity and the Development of Language. Center for Research in Language, UCSD.

Boas, Hans.
2007 Construction Grammar in the twenty-first century. Review of *Constructions at work*. *English Language and Linguistics*. 11 3: 569-585.

Bod, Rens
2006 Exemplar-Based Syntax: How to get productivity from examples. *The Linguistic Review* 23: 291-320.
Forthcoming From exemplar to Grammar: Integrating Analogy and Probability in Language Learning.

Borovsky, Arielle and Jeff Elman.
2006 Language input and semantic categories: a relation between cognition and early word learning. *Journal of Child Language, 33*, 759-790.

Boyd, Jeremy & Adele E. Goldberg
Forthcoming Linking rules of a novel construction are learned after three minutes of exposure. *Language Learning*.

Boyd, Jeremy, Erin Gottschalk & Adele E. Goldberg
Forthcoming Knowledge of novel construction is retained after a 7 day delay. Princeton University.

Bybee, Joan
1985 *Morphology: A Study of the Relation between Meaning and Form*. John Benjamins Publishing Company.
1995 Regular Morphology and the Lexicon. Language and Cognitive Processes 10, 425-55.
2007 Review of *Constructions at Work: the nature of generalizations in language*. *Journal of Child Language* 34, 692-7.
2008 Usage-based grammar and Second Language Acquisition. In P. Robinson and N. Ellis (eds.) *Handbook of Cognitive Linguistics and Second Language Acquisition*. 216-236.

Casenhiser, Devin & Adele E. Goldberg
2005 Fast Mapping of a Phrasal Form and Meaning. *Developmental Science* 8, 500-8.

Chomsky, Noam
2000 *New Horizons in the Study of Language and Mind.* Cambridge: Cambridge University Press.
2001 Derivation by Phase. In M. Kenstowicz (ed.) *Ken Hale, A life in Language.* Cambridge, Mass; MIT Press.

Culicover, Peter W. & Ray Jackendoff
2005 *Simpler Syntax.* University of Oxford Press.
Dabrowska, Ewa and Elena Lieven
2005 Towards a lexically specific grammar of children's question constructions. *Cognitive Linguistics* 16 3: 437-434.
Erteschik-Shir Nomi
1973 On the nature of island constraints. Indiana University Linguistics Club.
Fillmore, Charles J.
1977 Topics in Lexical Semantics. In R. Cole (ed.) Current Issues in Linguistic Theory. 76-138. Bloomington: Indiana University Press.
Fillmore, Charles J., Paul Kay and Mary Catherine O'Connor
1998. Regularity and Idiomaticity in Grammatical Constructions: the case of Let alone. *Language* 64:33, 501-538.
Fillmore, Charles J., Paul Kay, Laura A. Michaelis and Ivan Sag.
Forthcoming. *Sign-based Construction Grammar.* Stanford: CSLI.
Fodor, J. D.
1991 Sentence processing and the mental grammar. In P. Sells, S. M. Shieber, & T. Wasow (Eds.),*Foundational issues in natural language*. Cambridge, MA: MIT Press
Geurts, B.
2000 Review of Stephen Crain and Rosalind Thorton's 1998 Investigations in Universal Grammar: a guide to experiments on the acquisition of syntax and semantics. *Linguistics and Philosophy* 23, 523-32.
Goldberg, Adele E.
1995 *Constructions: A construction grammar approach to argument structure*. Chicago: University of Chicago Press.
1996 Making One's Way Through the Data. In M. Shibatani & S. Thompson (Eds.), *Grammatical Constructions: Their Form and Meaning* (pp. 29-53). Oxford: Clarendon Press.
2005 Argument Realization: the role of constructions, lexical semantics and discourse factors. In Jan-Ola Oostman and M. Fried (eds.) *Construction Grammars: Cognitive Grounding and Theoretical extensions*. Mouton. 17-44.
2006 *Constructions at Work: the nature of generalization in language*. Oxford: Oxford University Press.
This volume The nature of generalization in language. *Cognitive Linguistics*.
2008 Universal Grammar? Or prerequisites for natural language? *Brain and Behavioral Sciences*.

Goldberg, Adele E. & Devin Casenhiser
2006 Learning Argument Structure Constructions. In E. Clark & B. Kelly (eds.), *Constructions in Acquisition*. Stanford: Center for the study of Language and Information.
Goldberg, Adele E., Devin Casenhiser & Nitya Sethuraman
2004 Learning Argument Structure Generalizations. *Cognitive Linguistics* 14, 289-316.
Gurevich, Olya & Adele E. Goldberg
Forthcoming. Verbatim memory of language. Princeton University.

Hauser, Marc D., Noam Chomsky& Tecumseh W. Fitch
2002 The Faculty of Language: What is it, who has it, and how did it evolve? *Science* 298, 1569-79.
Hay, J. and Joan Bresnan
2006 Spoken Syntax: the phonetics of giving a hand in New Zealand English. *The Linguistic Review* 23: 321-349.
Herrmann, E. & Tomasello, Michael

2006 Apes' and children's understanding of cooperative and competitive motives in a communicative situation. *Developmental Science* 9(5): 518-529.
2007
Hilpert, Martin
2006 Review of *Constructions at Work: the nature of generalization in Language*. http://www.cognitivelinguistics.org/Reviews/goldberg.

Jackendoff, Ray
1983 *Semantics and Cognition,* Cambridge: MIT Press.
Jackendoff, Ray & Steven Pinker
2005 The Nature of the Language Faculty and its Implications for Evolution of Language (Reply to Fitch, Hauser and Chomsky). *Cognition.*
Jackendoff, Ray
2007 *The architecture of the language faculty*. Cambridge, Mass: MIT Press.
Kidd, Evan, Elena Lieven & Michael Tomasello
2006 Examining the contribution of lexical frequency and working memory to the acquisition of syntax. *Cognitive Development*.
Lakoff, George
1987 *Women, fire, and dangerous things: what categories reveal about the mind*. Chicago: University of Chicago Press.
Lakoff, George & Claudia Brugman
1987 The semantics of aux-inversion and anaphora constraints.
Langacker, Ronald W.
1987 Foundations of cognitive grammar Volume I. Stanford, Calif.: Stanford University Press.
2005 Construction Grammars: Cognitive, Radical, and Less So. F. J. Ruiz de Mendoza Ibáñez and M. Sandra Peña Cervel (eds.), Cognitive Linguistics: Internal Dynamics and Interdisciplinary Interaction, 101-159. Berlin and New York: Mouton de Gruyter.
Levinson, Stephen C.
2000 *Presumptive meanings: the theory of generalized conversational implicature*. MIT Press.
Lidz, Jeffrey, Lila Gleitman and Henry Gleitman
2003 *Cognition*
Lieven, Elena, Heike Behrens, Jennifer Speakers and Michael Tomasello.
2003 Early syntactic creativity: a usage-based approach. *Journal of Child Language* 30: 333-370.
Lewis, J. D. & Jeff L. Elman
2001 Learnability and the Statistical Structure of Language: Poverty of Stimulus Arguments Revisited. Paper presented at the Annual Boston University Conference on Language Development.
MacWhinney, Brian
2005 The emergence of grammar from perspective taking. In D. Pecher and R. Zwaan (eds.) *The grounding of Cognition: The Role of Perception and Action in Memory, Language, and Thinking*. Cambridge, UK: Cambridge University Press.
Michaelis, Laura
2004 Type shifting in Construction Grammar: an integrated approach to aspectual coercion. *Cognitive Linguistics* 15: 1-67.
Newmeyer, Frederick
2000
2005 *Possible and Probable Languages: a generative perspective on language typology*. Oxford: Oxford University Press
Perfors, Amy, Joshua B. Tenenbaum and Terry Regier
Forthcoming The learnability of abstract syntactic principles.
Petré, Peter

2007 Review of *Constructions at Work: the nature of generalization in Language*. linguist@LINGUISTLIST.ORG.
Pinker, Steven & Ray Jackendoff
2005 The Faculty of Language: What's special about it? *Cognition* 95, 201-36.
Reali, F. & Christansen, M. H.
2005 Uncovering the richness of the stimulus: structure dependence and indirect statistical evidence. *Cognitive Science* 29, 1007-28.
Rinehart, Tanya
1983 Anaphora and Semantic Interpretation
Rowland, Carolyn
2006 Explaining errors in children's questions. *Cognition* 104 1: 106-34.
Sag, Ivan
Forthcoming. English Filler-Gap Constructions. Manuscript. Stanford University.
2007 Remarks on Locality. *Proceedings of the HPSG07 Conference*. S. Mueller (ed.). Stanford: CSLI Publications.
Scholz, Barbara B., & Geoffrey K. Pullum
2006 Irrational Nativist Exuberance. In Robert J. Stainton (Ed.), *Contemporary Debates in Cognitive Science* (pp. 59-80): Blackwell.
Steels, Luc
2005 The role of Construction Grammar in Fluid Language Grounding. *Artificial Intelligence.*
Tomasello, Michael
2003 *Constructing a Language: A Usage-based Theory of Language Acquisition*. Cambridge, Mass.: Harvard University Press.
2004 What kind of evidence could refute the UG hypothesis? Commentary on Wunderlich. Studies in Language 28, 642-5.
To appear *The Origins of Human Communication*. MIT Press.
Tomasello, Michael, Nameera Akhtar, Kelly Dodson and Laura Rekau
1997 Differential productivity in young children's use of nouns and verbs. *Journal of Child Language* 24, 373-87.
Tomasello, Michael, Malinda Carpenter & Ulf Liszkowski
2007 A new look at infant pointing. *Child development* 78, 705-22.
Van Hoek, Karen.
1995 Conceptual Reference Points: A Cognitive Grammar Account of Pronominal Anaphora Constraints. *Language* 71 2:310-340.
Van Valin, Robert
2007 Review of *Constructions at Work: the nature of generalizations in language*. Journal of Linguistics 43, 234-40.

[1] I would like to thank Chuck Fillmore, Ken Goldberg, Paul Kay, Michael Israel, Mike Tomasello, Edwin Williams and C.J.W. Zwart for relevant discussions. The continuing work on language learning and production is supported by an NSF grant # 0613227.

[2] Pinker and Jackendoff have pled for caution and they do an impressive job detailing candidate domain-specific attributes; for the most part these arguments center on speech and semantics, not on universal principles of syntax. In fact, none of their proposed

domain-specific attributes lead to mainstream generative grammar. Jackendoff and Pinker offer a scathing assessment of the current minimalist approach and argue that a constructionist approach is a viable alternative.

[3] Lidz & Williams suggest that the novel construction might actually be interpreted as a normal English transitive construction with the object fronted as in something like: "The rabbit the hat produced." However, the semantics, the intonation, and the pragmatics of our novel construction are all inconsistent with this possibility. Most scenes did not involve scenes of creation that could be paraphrased with transitive verbs (e.g., a queen rolling in from off stage cannot be described with (*) *The queen the stage rolled*). The intonation used was also not one that would lend itself to a topicalization interpretation; In addition, the topicalization construction only occurs in certain discourse contexts that were not applicable in the context of the experiment.

[4] Lidz & Williams take issue with the wording of a passage describing honeybees' impressive ability to learn generalizations. As described in *CW*, honeybees are able to learn very general statistical generalizations such as "The food is in the compartment with the cue that <is the same as / is different from> the cue that was at the entrance to the first compartment" (pg 69-70). Strikingly, these insects readily generalize from experience with vertical or horizontal lines to smells or colors. The point of the honeybee discussion is to illustrate even this tiny insect's impressive ability to learn general "rules."

[5] Lidz & Williams argue against the idea that constructions that are islands are not within the focus domain, by noting that is possible to have contrastive stress within constructions that are islands. But any part of a sentence can receive contrastive stress. As *CW* tried to make clear, contrastive stress and metalinguistic focus are orthogonal to the determination of a focus domain (see pg. 131).

Lidz & Williams appear to accept that an information structure account works well for questions: a wh-filler phrase, being discourse prominent, cannot be coindexed with a gap within a backgrounded construction. But they argue that the account does not readily extend to relativization, since the head noun of a relative clause is not necessarily discourse prominent. But the head noun *is* discourse-prominent within the domain of the long-distance dependency; i.e., the head noun need not be discourse-prominent with respect to the entire sentence, it is discourse prominent as it relates to the predicational structure that involves the relative clause.

[6] I find it interesting that it is hard to find convincing cases of constructional ambiguity: shared syntax and completely unrelated function. If two phrasal patterns were truly associated with unrelated functions, such as the functions of conjunction and passivization, or reflexivization and modification, then their distributional behavior is not likely to be identical. When behavior diverges, we generally decide that the syntax involved is not the same. This is perhaps why purely syntactic generalizations are so hard to find. What is typical is neither ambiguity nor functional identity, but rather polysemy: the same form often has different but *related* functions.

[7] The suggestion in *CW* is that SAI appears in constructions that systematically deviate from prototypical sentences in sharing some subset of the following attributes: non-positive polarity, non-predicate focus information structure, non-assertive, non-declarative speech act function, and dependent clause status; non-positivity is argued to be the dominant attribute of the category (following Lakoff and Brugman 1987). Borsley and Newmeyer assert that non-positivity is not applicable to the following sorts of examples:

i. Under no circumstances will I be taking a leave of absence.

ii. Had I been thinking about the dangers, I never would have done that. (adapted from Borsley and Newmeyer's examples 3c-d).

But clearly, both of these examples are prime examples of non-positivity. Example (i) entails that the speaker will *not* be taking a leave of absence; and (ii) is a counterfactual: the speaker had *not* been thinking about the danger.

Borsley and Newmeyer also cite examples from Green and Morgan (1996) that we can categorize as instances of the following three constructions:

I. *particularly/especially/so* SAI: Her genius harmonized and made pleasant and agreeable the most antagonistic shades and tones of colors; and especially did she blend red and yellow… nal.usda.gov/speccoll/ collectionsguide/passmore/124ExtBio.pdf

II. *Many* $NP_{indefinite}$ SAI: . ….many a morning did he call upon him to try to get him to school… www.sermonindex.net/modules/ articles/index.php

III. Thus SAI : And thus did she firmly hold, and thus does she teach him that listens to her. www.newadvent.org/fathers/0860.htm

Each of these cases is severely limited, yet interesting. Each clearly warrants a constructionist analysis of its own. It is true that none of these patterns is non-positive, but they have other features common to SAI constructions including being non-independent (in the case of I and III) and having unusual information structure (in the case of I, II and III). Insofar as each of these patterns is highly restricted in various ways, these constructions provide evidence in favor of a usage-based constructionist approach.

[8] "Head raising" is supposed to capture the claim that heads 'move' only to head positions. In the case of SAI, it requires the assumption that the first auxiliary is the head of a clause and that it moves to a complementizer position which is analyzed as the head of a clause + complementizer (S|). Again, this analysis fails in the case of embedded instances of SAI that contain an overt complementizer (e.g., *that*). Chomsky (2001) has moreover claimed that head-raising has no semantic consequences and therefore is not part of "narrow syntax" but is only part of "PF" (phonological form); this analysis would leave the incontrovertible functional effects of inversion that exist in the case of many SAI constructions unexplained.

> At the same time, there is a general tendency for constraints to be *local*: a head generally constraints its daughter and sister constituents and not its nieces or grandnieces. Depending on where in the tree the auxiliary is assumed to attach, it may follow from a locality constraint that it is the highest auxiliary that inverts with the subject. Yet a full account of locality requires an analysis of exceptional cases (see Sag 2007), and the generalization cries out for a functional motivation, possibly in terms of possible relevant scope of predication. In any case, the general observation that constraints tend to be local clearly does not absolve us from an accounting of which constructions require or allow SAI and which do not allow SAI.

[9] Oddly, Croft seems to imply that I take the "deprofiled object construction" (Goldberg 2005) to involve definite (as opposed to indefinite) null arguments. It is not relevant to the pragmatic mapping principles proposed, but the construction is explicitly recognized to involve indefinite null arguments (pg 196). In his remarks, Croft also notes several of his own papers that I failed to cite (see also Petre 2007 for mention of this oversight). For this I apologize.

[10] It is possible to get an interpretation involving *schadenfreud*e among non-Californians in which *everyone* does refer to "all people" but this is not the most accessible interpretation.

[11] A is said to "c-command" B iff A does not dominate B, nor does B dominate A, and if the first (branching) node that dominates A dominates B.

[12] In my defense, the majority of the work Bod cites was published, as he acknowledges, in conference proceedings, and, in fact, well after *CW* was submitted for publication (which was in early 2005). The omission of reference to Bod's earlier work on DOP or to Steels' work on Fluid Construction Grammar was due simply to my ignorance of its existence at the time.

[13] If I dare exemplify the sometimes gratuitous use of formalization with an example from my own work, let me offer the following case. In Goldberg (1995:4), I defined constructions as follows:

> C is a CONSTRUCTION iff$_{def}$ C is is a form-meaning pair < F_i, S_i> such that some aspect of F_i or some aspect of S_i is not strictly predictable from C's component parts or from other previously established constructions.

This definition has been widely cited as an adequate (if incorrect) definition of what was intended by the term, *construction*; to my knowledge no one has ever claimed no definition was given. And yet, clearly, the subscripts, capital letters, and angled brackets add absolutely nothing of content. Recognizing their vacuity, I have simply omitted this affectation in later work. Amusingly, Bod now suggests that *CW* lacks a definition of construction, while noting "I understand (and agree) that 'any linguistic pattern is recognized as a construction […]' (Goldberg 2006:5)." If we spell out the ellipsis in the passage, we find that constructions are defined as follows:

> Any linguistic pattern is recognized as a construction as long as some aspect of its form or function is not strictly predictable from its component parts or from other constructions recognized to exist. In addition, patterns are stored as constructions even if they are fully predictable as long as they occur with sufficient frequency (see Chapter 3 for discussion).

One might legitimately worry about what counts as "sufficient frequency" (see in fact Gurevich and Goldberg for an exploration of this issue), but surely the newer definition is at least as sound as the earlier one (and, I believe, more valid).