

## PERSPECTIVES

### **Explanatory adequacy is not enough: Response to commentators on ‘Child language acquisition: Why universal grammar doesn’t help’**

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In this response to commentators on our target article ‘Child language acquisition: Why universal grammar doesn’t help’, we argue that the fatal flaw in most UG-based approaches to acquisition is their focus on describing the adult end-state in terms of a particular linguistic formalism. As a consequence, such accounts typically neglect to link acquisition to the language that the learner actually hears, instead assuming that she is able, by means usually unspecified, to perceive her input in terms of high-level theoretical abstractions.\*

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Since its first issue in 1925, *Language* has been at the forefront of linguistic research. The new Perspectives section will help to ensure that the journal maintains this position as academic discourse moves increasingly from off- to online, and we are honored to have been its first contributors (Ambridge, Pine, & Lieven 2014). In the first volume of *Language*, Weiss (1925:52) offers the opinion that ‘when the psychologist finds himself confronted with the request to make a “psychological” explanation ... of a careful and detailed linguistic investigation, he is unable to add anything and if anything is added it often only obscures the investigation’. While some of our commentators would no doubt agree, in this response we argue the contrary: that linguistic explanations of a particular phenomenon succeed only when they can be shown to have some psychological reality (i.e. when they can be tied to the language that the learner ACTUALLY HEARS, rather than assuming that she is able, by means unspecified, to perceive her input in terms of high-level theoretical abstractions). The fatal flaw in most UNIVERSAL GRAMMAR (UG)-based approaches is their failure to take this problem seriously. As long as this remains the case, increasingly detailed theoretical accounts of the adult grammar—which seem to be the target for most UG-based approaches—will get us nowhere.

If there is one thing upon which we and Weiss (1925:57) can agree, it is that explanation of ‘language behavior ... demands a type of investigator trained in both linguistics and psychology’. Many of the shortcomings in the present debate—both those that we discuss with reference to UG-based accounts and those that we exhibit ourselves—would seem to stem from the fact that many of us know too little about one of these disciplines (and, perhaps, too much about the other). We therefore end this preamble by thanking our colleagues—linguists and psychologists, philosophers, and practitioners—not only for taking the time to write such detailed and insightful commentaries, but also for maintaining a collegial tone, something that we cannot necessarily take for granted from either side in this long-running and often heated debate.

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We begin by considering the more sympathetic commentaries. Behme's (2014) argument is essentially that we did not go far enough, that we could have strengthened our critique by going into greater detail on (i) findings from developmental psychology, (ii) the different challenges posed by different languages, and (iii) internal inconsistencies in the UG approach. We certainly agree that many findings from developmental research are problematic for UG-based accounts. In many different domains, including WH-questions (e.g. Ambridge et al. 2006), root-infinitive errors (e.g. Freudenthal et al. 2010),<sup>1</sup> determiners, and basic word order (see Ambridge & Lieven 2015 for a review), the gradual, piecemeal, lexical nature of acquisition is difficult to reconcile with UG-based accounts such as semantic/phonological bootstrapping or parameter setting. We also agree with Owen Van Horne, Hall, and Curran (2014) that UG-based accounts are difficult to reconcile with findings from studies of specific language impairment, particularly in the domain of root-infinitive errors (e.g. Leonard et al. 2015). We agree, too, that the degree of variation between languages makes it hard to posit innate knowledge that is both sufficiently general to be part of UG and sufficiently precise to be useful to learners of a given language (e.g. Evans & Levinson 2009). But the point of our critique was to argue that even if we give UG accounts a free pass with regard to all of these factors (e.g. one could argue that inconsistencies in the stimulus-poverty argument are not fatal, since this is not the only argument for UG), these accounts fail on their own terms: the innate knowledge posited under such accounts not only fails to bridge the gap between finite input and (arguably) a potentially infinite output system, but also fails to make learning any easier at all.

Lin (2015) takes a similar approach, setting out yet another apparent problem for UG that both we and Behme failed to discuss: that UG theorizing involves simply inspecting 'some grammatical and ungrammatical sentences in various languages', ignoring factors such as 'memory, attention, information-retrieval speed, and information-processing speed' (2015:e29). While this might have been true in the bad old days, many modern generativist researchers are sensitive to the need to take such factors into account when proposing UG-based learning models (Pearl's (2014) commentary is a case in point; see also Sprouse et al. 2012 on subadjacency). Certainly, some generativist researchers—just like some nongenerativist researchers—could use tighter, better-controlled methods. But, again, the point of our critique was to show that even if UG theories are allowed whatever generalizations they like, no matter how they were arrived at, these theories still fail in their goal of simplifying the learning task.

We now move on to consider two commentaries that bridge the gap between those that are broadly anti- and pro-UG. We agree with Beekhuizen, Bod, and Verhagen (2014) that, for the problems of learning word classes and syntactic roles, there exist few computationally explicit non-UG proposals for how a learner could solve both of these problems at once (though the dual-path model of Chang et al. 2006 arguably comes close). Does this mean that, as these authors argue, 'the linking problem is everyone's problem' (2014:e91)? Well, it depends on what you mean. At a general level, yes, the problem of linking the input to underlying representations that allow adult speakers to generate novel output applies to all approaches. It seems to us that this is pretty much exactly the argument that we made in our original article. Nobody has a precise computational model of how learn-

<sup>1</sup> Pérez-Leroux and Kahnemuyipour (2014:e117) cite Valian et al. 2009 and Yang 2013 as evidence in support of generativist accounts of determiner acquisition and against input-based learning accounts. In fact, the debate has not ended with these articles, but continues with a tightly controlled study by Pine, Freudenthal, Krajewski, and Gobet (2013) and modeling studies by McCauley and Christiansen (2014) and Meylan, Frank, and Levy (2013).

ers solve this problem (though pretty much everyone agrees that distributional clustering is part of the solution; see pp. e55–e56<sup>2</sup>). But at a more specific level, the problem of linking the input to innate syntactic categories is not everyone’s problem. Rather, it is a problem specific to accounts that propose innate word class and phrasal categories (e.g. NOUN, NP, VP), even though there are no feasible proposals for how children could identify instances of these categories

In a similar vein, our position on the prosodic bootstrapping hypothesis depends on exactly what is meant by the term. Again, we can distinguish between a more general and a more specific reading. If, as Soderstrom (2014) understands the hypothesis, the claim is simply that learners exploit regularities between prosody and syntax in order to aid in their acquisition of the latter, then we agree that this is not only perfectly plausible, but also consistent with a considerable amount of existing empirical evidence. We also agree with Soderstrom that this version of the hypothesis is neutral with regard to the question of innate knowledge; the prosody-syntax correlations could have been entirely learned, and be just one of a number of cues to syntactic structure (rendering moot the problems of crosslinguistic variation and cue mismatch). But if the claim is that learners use the correlation between prosody and syntax to BREAK INTO THE SYSTEM—that is, to set grammatical parameters (e.g. Mazuka 1996) or identify word class or phrasal categories (e.g. N, V, NP, VP) before any other meaningful acquisition of syntax has taken place—then this claim seems entirely implausible. The main problem is the considerable amount of crosslinguistic variation with regard to prosody-syntax correlations discussed in Soderstrom’s commentary (and—if a little obliquely—in our target article; e.g. p. e59). So, if nobody is in fact making this claim, then great!, because it seems unlikely to be correct.

Soderstrom ends by suggesting that it is high time that nonnativist researchers move away from simple explanations and toy problems, and begin to produce computational models that can simulate important aspects of language acquisition in all of their complexity. We agree completely, but also agree with Pearl that the same standard applies to both UG and anti-UG proposals. We accept, too, Pearl’s point that our claims about the more general learning mechanisms that we discuss in our target article also need to be tested computationally. Indeed, this process has already begun (e.g. Foraker et al. 2009, Perfors et al. 2011, Fitz & Chang 2015).

We would like to reemphasize, however, that the general learning mechanisms that we propose are, in most cases, not ALTERNATIVES to UG-based accounts, but mechanisms that UG-based accounts will have to incorporate in some form IN ADDITION TO their more specific linguistic assumptions. For example, distributional learning is not an ALTERNATIVE to innate syntactic classes, but a key part of UG proposals for how innate classes are identified (see the quotations from Chomsky (1965), Pinker (1984), Yang (2008), and Valian, Solt, and Stewart (2009) on pp. e55–e56). Learning the semantic distinction between *her* and *herself* is not an ALTERNATIVE to principles A and B, but a key part of UG proposals for how these principles work (e.g. Chien & Wexler 1990, Grodzinsky & Reinhart 1993; see p. e80). The fact that NP complements contain background information that cannot be denied using sentential negation (e.g. *Bill heard the rumor that Sue stole the files. No, he/\*she didn’t*; p. e74) is not an ALTERNATIVE to an innate subjacency principle, but something that needs to be learned even with this principle in place.

<sup>2</sup> All page references given without a date and/or author indicated are from our target article, Ambridge et al. 2014.

We consider now the commentaries that dispute our original conclusions and/or defend UG-based proposals, beginning with that of Pearl (2014), who outlines a computational model of how a learner could use innate knowledge to acquire island constraints. While we applaud the computational precision of this account (and, again, agree that this is something that is all too often lacking from both sides in this debate), it reveals that this proposal constitutes another case of cascading UG assumptions that become increasingly problematic (see p. e59). As Pearl notes, '[t]his successful strategy requires the learner to characterize WH-dependencies at a particular level of granularity, namely what might be considered "standard" phrasal nodes (e.g. IP, VP, NP, etc.), with the exception of CP, which is subcategorized by the lexical item in complementizer position (e.g. CP<sub>null</sub>, CP<sub>that</sub>, etc.)' (2014:e112). But, as we pointed out in our target article, there are no workable proposals for how children might recognize in their linguistic input instances of even basic phrasal categories such as NP and VP, let alone the more abstract categories IP and CP. Solving one relatively marginal problem (learning island constraints) involves first solving a much harder and more fundamental problem (the linking problem) for which no solution is in sight.

Schütze, Sprouse, and Caponigro (2015; henceforth Schütze et al.) are also concerned with island constraints. Their commentary sets out four desiderata for any successful proposal, and argues that the traditional UG-based account meets these desiderata better than our alternative pragmatic account. In fact, as we argue below, it is far from clear that the traditional account does indeed outperform the pragmatic account on these criteria. But this debate misses our original point: even if (i) the traditional UG account were to offer a perfect characterization of island and related phenomena across all constructions and all languages and (ii) this abstract knowledge is built into an innate UG, it is still of absolutely no use to a learner, unless he has some way to link this knowledge to the language that he hears. And, as we saw above, the only existing account of how a child might solve this linking problem (Pearl) presupposes that he has already solved a harder problem (identifying instances of NP, VP, IP, and CP).

Anyway, for the sake of argument, here are Schütze et al.'s (2015:e33) four desiderata for a successful account of island constraints, and our reasons for disputing that the traditional UG account necessarily outperforms the pragmatic account with regard to these criteria.

**(i) A way to account for the attested crosslinguistic variation in island effects (without being so liberal that no bounds can be placed on this variation).** Under the pragmatic account, it is anomalous for a constituent to be both backgrounded (i.e. assumed, old news, not up for debate) and questioned. As Schütze et al. note, our explanation of crosslinguistic variation is that 'backgroundedness is a graded notion; hence, different languages are free to "choose" the extent to which a constituent may be backgrounded and still permit extraction' (p. e73). An advantage of this account is that—as Schütze et al. (2015:e35, n. 5) point out—it makes two testable predictions: that island constraints should be affected by the containing discourse, and that 'no language should have constituents that do not behave as islands that are more backgrounded than any constituent that does behave as an island' (though, given the existence of memory and processing factors, upon which all sides generally agree, we would argue that the appropriate prediction is of a CORRELATION between backgroundedness and islandhood, rather than absolute correspondence). Given the existence of the negation test as an independent measure of backgroundedness, testing these predictions in a negation test + grammaticality-judgment study (e.g. Ambridge & Goldberg 2008) would be relatively straightforward. Subjective intuitions will clearly not suffice here, since our own intu-

ition—contra Schütze et al. and Pérez-Leroux and Kahnemuyipour (2014:e122, ex. 17)—is that the acceptability of a sentence such as *\*What did Bill hear a rumor that Sue stole?* is indeed improved by the addition of a context sentence such as *Bill heard a rumor that Sue stole something.*

In contrast, the UG explanation of crosslinguistic variation—that different languages have different bounding nodes—does not make testable predictions for two reasons. The first is that a degree of circularity is inevitable, as the bounding node for a particular language is determined partly on the basis of the island phenomena shown by that language. The second is that the account has too many free parameters. For example, lexical differences in island behavior (which fall naturally out of a pragmatic account) are explained simply by stipulating that certain verbs are ‘bridge verbs’.

**(ii) A way to account for the cross-construction generality of island effects.**

Schütze et al. argue that a pragmatic account cannot explain a variety of noninterrogative islands. Their argument is that the displaced element is not backgrounded. Although we concede that ‘backgroundedness’ is perhaps not quite the right LABEL, a major advantage of the pragmatic account is that—in the form of the negation test—it operationalizes this construct in a graded quantitative way, and seems to make the right predictions here (cf. Schütze et al. 2015:e35, exs. 7–11, for the examples here and below).

- (1) \*I would pity a man who<sub>i</sub> Sue wonders <whether she should dump  $t_i$ >. **No, she doesn’t (wonder)/\*No, she shouldn’t (dump him).**
- (2) \*I wonder whether John likes most of these cars, but THAT car<sub>i</sub>, I wonder <whether John LOVES  $t_i$ >. **No, I don’t (wonder)/\*No, he doesn’t (love that car).**
- (3) \*Humiliated<sub>i</sub> though I wonder <whether Jane might be feeling  $t_i$ >, I’m still going to call her. **No, I don’t (wonder)/\*No, she mightn’t (be feeling humiliated).**
- (4) \*Please don’t tell me again that it is Judy who<sub>i</sub> you wonder <whether John should marry  $t_i$ >. **No, you don’t (wonder)/\*No, he shouldn’t (marry Judy).**
- (5) \*What<sub>i</sub> I wonder <whether John should buy  $t_i$ > is a sports car. **No, I don’t (wonder)/\*No, he shouldn’t (buy a sports car).**

Schütze et al. also argue that (i) no island constraint holds for equivalent sentences with complementizer *that*, and that this phenomenon (ii) is not explained by the pragmatic account but (iii) is explained by the standard subjacency account.

- (6) I would pity a man who<sub>i</sub> Sue knows [that she should dump  $t_i$ ].
- (7) I think that John likes most of these cars, but THAT car<sub>i</sub>, I think [that John LOVES  $t_i$ ].
- (8) (?)Humiliated<sub>i</sub> though I suspect [that Jane might be feeling  $t_i$ ], I’m still going to call her.
- (9) Please don’t tell me again that it is Judy who<sub>i</sub> you think [that John should marry  $t_i$ ].
- (10) What<sub>i</sub> I think [that John should buy  $t_i$ ] is a sports car.

We suggest that none of these three claims are uncontroversially true. Without a judgment study, it is premature to assert that all of these sentences are fully acceptable (or more acceptable than the *whether* equivalents). Without a negation-test study (e.g. Ambridge & Goldberg 2008), it is premature to assert that the pragmatic account cannot predict the relative acceptability of these sentences and their *whether* equivalents. Informally, it seems that the negation test might do rather well. For example, of the twelve

verbs rated by Ambridge and Goldberg's (2008) participants, *think* was—by some distance—the verb for which sentential negation was judged to most imply negation of the complement clause (presumably because it is a light verb that often acts as a kind of hedge, rather than expressing true propositional content). Thus a sentence such as *I don't think that John {loves that car/should buy a sports car}* is actually rated as implying quite strongly that John does not love/should not buy the car (and certainly more so than for an equivalent sentence with, for example, a manner-of-speaking verb). Consequently, the negation test predicts that Schütze et al.'s *think* sentences should be relatively acceptable (apparently correctly, though a judgment study is needed). Finally, suppose that Schütze et al.'s claim that each *that* sentence is more acceptable than its *whether* equivalent is borne out by a judgment study. It is unclear why this pattern would constitute support for a formal account, given that the two sentences in each pair are structurally equivalent.

- (11) a. I would pity a man who<sub>i</sub> Sue knows [that she should dump *t<sub>i</sub>*].  
 b. \*I would pity a man who<sub>i</sub> Sue wonders [whether she should dump *t<sub>i</sub>*].

The two sentences receive a different analysis only under the assumption that *whether* is not a complementizer, even though it seems to be functioning as one here and does not seem to fit the distributional criteria for membership of an alternative category such as WH-word (e.g. Batson 2012).

**(iii) A way to account for the effect of resumption on island behavior.** The situation is similar for Schütze et al.'s third criterion. Can the pragmatic account explain the pattern observed, on the basis that a dependency with a gap is more backgrounded than a dependency with a resumptive pronoun? We will not know until we have a study that combines graded measures of backgroundedness and grammatical acceptability. But this is exactly what one would expect on the basis of something like Gundel, Hedberg, and Zacharski's (1993) givenness hierarchy: elements that are omitted altogether (rather than referred to with a pronoun) are those that are backgrounded, old news, already the topic of the ongoing discourse.

In contrast, the explanation offered by the UG account again rests crucially on an additional free parameter that is not a core part of the theory. Schütze et al. 'postulat[e] two dependency-forming mechanisms: movement in the case of gaps, and something else, which is not constrained by islandhood (e.g. binding), in the case of resumptive pronouns' (2015:e37). But if the only reason for positing binding rather than movement in a particular case is that no island effect is observed, then the explanation is circular.

**(iv) A way to account for the effect of WH-in-situ on island behavior.** Schütze et al. point out that, in languages such as Japanese, adjunct WH-phrases (e.g. *how* and *why*) show island effects even when left in situ, while argument WH-phrases (e.g. *what* and *who*) do not. Can the pragmatic account explain this pattern, on the basis that adjuncts are more backgrounded than arguments? Again, we will not know until we have a study, but the negation test seems to make the right predictions, with sentential negation denying arguments more straightforwardly than adjuncts.

- (12) John saw Sue. **No, he didn't.**

- (13) John saw Sue behind the bike shed. **No, he didn't/\*No, she wasn't.**

Again, the formal explanation depends on an additional free parameter, in this case 'parametrically allowing the argument/adjunct distinction to govern which dependencies are or are not sensitive to islands' (Schütze et al. 2015:e38). If one's approach to crosslinguistic variation is simply to posit a parameter that yields the observed varia-

tion, with no limit to the number or nature of these parameters, then the explanation becomes nothing more than increasingly fine-grained stipulation.<sup>3</sup>

In fact, one could even argue that the very existence of island constraints in WH-in-situ languages is explained more naturally by the pragmatic than the subjacency account (e.g. Van Valin 2005). Because the former posits no movement, the prediction—that a backgrounded WH-constituent cannot be questioned, topicalized, and so forth—holds regardless of its position (in situ or not). In contrast, the subjacency account is forced to posit covert movement (i.e. movement after SPELL-OUT). Again, this explanation risks circularity, because at least part of the evidence for covert movement with regard to a particular sentence type is the existence of island effects.

Also taking up the subject of islands, Pérez-Leroux and Kahnemuyipour (in addition to making many points echoed by Schütze et al.) object that our negation-test measure of backgroundedness measures ellipses rather than focus. They also object to the fact that this test excludes negation by recasting, asking: ‘Why is recasting a problem at all?’ (2014:e121). The answer is that we are not interested in negation per se; the negation test is just a measure of the information structure of the sentence—what the sentence is primarily about. We would happily admit that, as a measure of information structure, the negation test is rather rough and ready, but it has the virtue of yielding a measure of backgroundedness that is both quantitative and independent of the phenomenon that we are trying to predict (island status). As we argued above, neither of these virtues is shared by positing free parameters with regard to the bounding node for the relevant language, bridge verbs, whether the language cares about the argument/adjunct distinction, the overttness of WH-movement, and so on. If, as Pérez-Leroux and Kahnemuyipour imply, the negation test is not an appropriate measure of backgroundedness, then it will predict nothing about island status. By the same token, if the negation test turns out to be a good predictor of island status, then whatever property of information structure it is measuring is clearly one that is relevant. To our knowledge, the relevant studies have yet to be conducted, but the signs from a preliminary study (Ambridge & Goldberg 2008) are promising.

Like Schütze et al., Pérez-Leroux and Kahnemuyipour are premature in asserting that the pragmatic account, as operationalized via the negation test, cannot explain certain phenomena before it has been tested on the relevant sentence types. For example, consider the phenomenon that *when* can refer only to the time of the telling in 14, but to either the time of the telling or the time of the injury in 15.

(14) When did the boy tell his father how he hurt himself?

(15) When did the boy tell his father that he hurt himself?

Again, the negation test seems to make the right prediction here, with sentential negation denying arguments (e.g. *that*) more straightforwardly than adjuncts (e.g. *how*).

(16) The boy told his father how he hurt himself. **No, he didn’t (tell him/\*hurt himself).**

(17) The boy told his father that he hurt himself. **No, he didn’t (tell him/hurt himself).**

Are our intuitions correct here? Although we do not have a comparison with *how* sentences, we know from Ambridge and Goldberg (2008) that the construction *X said that*

<sup>3</sup> Worse, any particular claim regarding the existence of a particular principle, parameter, or universal becomes very difficult to falsify, since any potential counterexamples can be explained away by positing a further parameter.

*Y* foregrounds the complement clause to a relatively large extent, predicting that questioning of this clause should be possible. Furthermore, if these intuitions are correct, this account would explain why the same pattern holds for the equivalent WH-in-situ echo questions (or dialogue between two speakers).

- (18) The boy told his father how he hurt himself WHEN? (= When did he tell him/\*hurt himself?)  
 (19) The boy told his father that he hurt himself WHEN? (= When did he tell him/hurt himself?)

Finally, the developmental findings cited by Pérez-Leroux and Kahnemuyipour are an example of how theory-internal assumptions can lead to theory-internal solutions to problems that do not even arise in the absence of those assumptions. For example, if one is committed to the assumption that a sentence such as *What does Bill know ~~the fact~~ that Sue stole?* contains a silent *the fact*, then a problem arises as to why ‘children seem to violate the factive island’ (2014:e123). But if we choose not to posit invisible silent elements (which seems to be a sensible default position), there is no island to violate. To the extent that the sentence is ungrammatical at all, it is due to the fact that factive verbs presuppose the truth of—and hence background—their complements, as demonstrated empirically by Ambridge and Goldberg (2008).

Although the extended discussion of the relative merits of the formal and pragmatic accounts of island constraints above is, in a sense, orthogonal to our argument, the diversion is an instructive one as it illustrates in microcosm the fundamental disagreement between ourselves and many (most?) proponents of UG approaches. The disagreement is a fundamental one because it is not a dispute about empirical data or its interpretation, or even about theory per se, but about the very nature of the enterprise in which the language acquisition researcher is engaged.

Chomskyan approaches (e.g. Chomsky 1964) distinguish between DESCRIPTIVE and EXPLANATORY adequacy (e.g. Boeckx & Hornstein 2003). A theory of grammar is DESCRIPTIVELY ADEQUATE if it correctly describes the intuitions of an idealized native speaker (e.g. with regard to which sentences are grammatical versus ungrammatical). A theory of grammar has EXPLANATORY ADEQUACY if it provides a way of choosing between different possible descriptions of a particular language, in terms of some overarching theoretical principle. Such a grammar will have predictive power (e.g. if a language exhibits this feature, it will also exhibit that feature). On our reading of the field, many Chomskyan language acquisition researchers see the primary goal of the field as characterizing the system to be acquired in terms of a theory of grammar that achieves explanatory adequacy (in the sense outlined above). Accordingly, most theoretical work involves coming up with descriptive grammars of particular languages that are consistent with an overarching grammar that has explanatory adequacy (and hence that constitutes an appropriate characterization of UG). Most empirical work involves demonstrating that children’s knowledge is consistent either with these descriptive grammars, or with the overarching UG (and hence with possible but nonexistent languages). The main criticism that is made of alternative approaches to acquisition is that they do not achieve this type of explanatory adequacy and are therefore not even worth taking seriously. From this perspective, if we can characterize the UG that makes language learning possible, then we have explained language acquisition. With some exceptions (e.g. Pearl), explaining how children link up their UG knowledge to the language that they hear does not seem to be a major concern, presumably because—if we assume this perspective—the fact that children acquire language then constitutes sufficient evidence that, somehow or other, they must do so.



We have two objections to this approach. The first relates to our protracted discussion of island constraints. The reason for our championing of the pragmatic account is not so much that we are wedded to this particular account (which might well turn out to be on the wrong tack), but that it is the TYPE OF ACCOUNT that—setting aside the question of acquisition—researchers interested in the adult end-state should be striving for. It is an account that, in the sense of Chomsky (2001:2), goes BEYOND EXPLANATORY ADEQUACY: ‘In principle, then, we can seek a level of explanation deeper than explanatory adequacy, asking not only WHAT the properties of language are, but WHY they are that way’ (emphasis original). For all of its faults, the pragmatic account treats the formal description (i.e. the finding that island constraints are sensitive to the identity of the particular verb and complementizer, to argument versus adjunct status, and so on) as something to be explained, rather than as an explanation in its own right. Thus, whether or not this particular account turns out to be correct, it at least offers the hope of taking us beyond explanatory adequacy. In contrast, because it sees subjacency as a ‘classic example of an arbitrary constraint’ (Pinker & Bloom 1990:717), the formalist account, almost by definition, rules out the possibility of explaining island constraints in terms of some deeper underlying phenomenon.

Our second objection to the Chomskyan approach to language acquisition (at least, as we have characterized it above) ends this response by bringing us back to the original point of our target article. Even if it were possible to come up with a UG that achieved perfect explanatory adequacy, being born with this grammar would help the learner only if she had some way to relate this knowledge to the language that she hears. We have yet to see a successful proposal for how this might be done in any domain of acquisition. Furthermore, of all the responses to our target article that have appeared here and elsewhere, only one (Pearl) has even attempted such a proposal. Thus the challenge to advocates of UG that we set out at the end of our target article still stands: rather than simply presenting arguments against rival approaches, explain how a particular type of innate knowledge would help the child to acquire a particular piece of linguistic knowledge, given the input that she receives.

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