



PERSPECTIVES

All hands on deck: In defense of the prosodic bootstrapping hypothesis and multiple theoretical approaches (Commentary on Ambridge, Pine, and Lieven)

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Two weaknesses of Ambridge, Pine, and Lieven's (AP&L) argument against UNIVERSAL GRAMMAR are discussed in this commentary. First, their article treats the prosodic bootstrapping hypothesis (PBH) as a nativist theory, but PBH is entirely neutral with respect to the nativism-empiricism debate. Additional discussion of the plausibility of PBH is presented. Second, the rigor that AP&L direct toward nativist ideas must also be directed at empiricist claims. An understanding of how children acquire language will require nativist ideas, empiricist ideas, and ideas that are neutral on this dimension.*

Keywords: prosodic bootstrapping hypothesis, PBH, nativism, empiricism, universal grammar

Ambridge, Pine, and Lieven (2014; AP&L) have put together an important challenge to specific mainstream ideas about the components of UNIVERSAL GRAMMAR (UG). In this response, I focus on two particular issues among many that it raises: first, the validity of the prosodic bootstrapping hypothesis, and second, the larger problem of the relationship between UG and distributional models.

1. THE PROSODIC BOOTSTRAPPING HYPOTHESIS. AP&L discuss the PROSODIC BOOTSTRAPPING HYPOTHESIS (PBH) specifically as a mechanism within the nativist framework. However, the PBH is itself entirely neutral with respect to the nativist-empiricist debate. At its core is the simple idea that there is a relationship between syntactic constituents (by which I mean constituents in the loosest sense, as in a subpart of a whole) and prosodic ones, and that infants are sensitive to these prosodic characteristics. Similarly, the Christophe et al. 2008 article they reference is not discussing innate grammar. AP&L acknowledge this but suggest that UG is hidden in the margins with the use of terms like 'spontaneously perceived'. I would argue that, regardless of the specific positions of any of the authors with respect to UG more broadly, this is reading too far in the margins with respect to the article itself. The only kind of UG that is explicitly present in the article is the notion that infants are learning a language that has some kind of constituent structure that can be marked by prosodic regularities. Christophe and colleagues cite findings suggesting that such structural regularities may be universal, and at this point do allude to some form of UG in stating that '[PBH] is a universal procedure that is used in all languages' (2008:66), but this is not a necessary component of their argument. This may seem nitpicky, but it speaks to an issue of where the 'assumptions' are in a theory, which I address in the next section.

AP&L's larger point regarding the PBH, however, is that (even if it is itself theory-neutral) it may be used by nativists as a means of helping with the 'linking problem' between underlying structure and surface input. It is therefore germane to consider the validity of the PBH. AP&L rightly raise the concern about the extent to which there really is a relationship between prosodic and syntactic structure. A solid discussion of this issue by Fernald and McRoberts (1996) (and others related to PBH) is available in Morgan and Demuth's *Signal to syntax* (1996). In it, Fernald and McRoberts argue that proponents of PBH need to pay better attention to cue reliability—how often a prosodic

* Thank you to Erin Conwell for her comments on an earlier draft of this commentary.

cue, when present in the input, leads to a correct generalization about the presence of a syntactic boundary. AP&L focus on one source of cue unreliability, the case of pronoun sentences. They rightly explain that the prosodic boundary in sentences with pronoun subjects is typically not coincident with the subject-verb boundary, unlike sentences with full noun phrase subjects. Since the great majority of child-directed utterances contain pronoun subjects (Fisher & Tokura 1996, Soderstrom et al. 2008), this poses a potential problem for the PBH. However, the situation with respect to cue reliability is more complicated than this suggests.

One important question is what happens when there is a cue mismatch. Infants' use of prosody to mark the subject-verb boundary becomes more plausible if infants have a way of knowing that pronoun phrases should be treated differently. At least one study suggests that this might be the case (Gerken et al. 1994). Note that pronoun sentences, although highly frequent in child-directed input, contain certain properties that make them unique: they are relatively uniform compared with the much larger variability of full noun phrase subjects, they are very short, and they are acoustically reduced. Relatedly, as Fisher and Tokura (1996) point out, child-directed speech also contains a large number of sentence fragments that are structurally complete noun phrases and verb phrases, demarcated by utterance- or clause-level prosody (Fisher & Tokura 1996, Soderstrom et al. 2008). Therefore the PBH may provide means of detecting phrase-level constituents even if the phrase boundaries themselves are not sufficiently cue reliable. Another possibility is that yes-no questions, which are also highly frequent in child input and contain unique prosodic features that make them easily identifiable, may play a unique role in providing clues to the subject-verb boundary (Soderstrom et al. 2008).

There is an additional weakness to the current state of the PBH account that is not even mentioned by AP&L, namely, that the relationship between prosody and syntax varies significantly from language to language. To date, our understanding of how prosody might inform syntax for the language learner is largely limited to English, French, and Japanese (e.g. Christophe et al. 2008, Fisher & Tokura 1996, Seidl & Cristià 2008, Soderstrom et al. 2003). This and the above concerns, however, are weaknesses that are addressable with additional research.

In sum, the PBH as a theory is distinct from, and neutral with respect to, the debate on UG. AP&L point to some well-understood weaknesses in the PBH, but none of these weaknesses rises to the level of suggesting that PBH is implausible, but rather suggest simply that it is incomplete.

2. THE CHALLENGE. AP&L's larger point captures an insight that seems to fall out naturally from any serious contemplation of the earliest stages of grammatical development from the INFANT's perspective—namely, that in order to link the input with underlying UG, a great deal of computational work on the input must take place. Their assertion that the extent of this computational work overrides any benefit of UG (at least as it is currently articulated in much of the mainstream language development literature) is a serious challenge to nativists to show how a hypothesized component of UG actually solves the problem it purports to address. This reframing of the innateness question can only benefit the theoretical development of the field.

Of course, this challenge must be leveled equally against both nativist and empiricist accounts of any given component of the acquisition problem. The flip side of the obligation for nativists to resolve the linking problem (of which I would argue 'data coverage' and 'redundancy' are important subcomponents, rather than separate ideas) is the

obligation of empiricists to fully articulate how language regularities are resolved developmentally into clearly definable structures that are recognizable to linguists. I have argued elsewhere (Soderstrom et al. 2009) that empiricists must progress beyond 'proofs in principle'. Computational models need to be scaled up to the level of real, messy data, and these models must also be explicit about the assumptions they are making about how the input is represented and processed, in the same way that AP&L argue is incumbent upon nativists. Such models must necessarily include some level of 'innate' or at least 'prelearned' knowledge, unless the model were to take the raw acoustic stream and generate all the phonology, the lexicon, and the syntax in one go—which we are obviously a long way from.

AP&L put forward the claim that distributional and semantic regularities in the language account for the successes that UG accounts claim as their own. It is equally incumbent on models that generate such categories with only distributional and semantic regularities to demonstrate that their success is not dependent on assumptions about how the input is represented or the nature of linguistic knowledge that sneak innate knowledge in the back door. One important way in which such 'backdoor' UG might emerge in these models is in the relationship between the graded outcomes of distributional analysis and the discrete forms that constitute syntactic categories. Models that use the one to generate the other must necessarily make decisions about how similar is similar enough—how categories are defined from graded input. More importantly, the knowledge that the task is to generate such categories is itself an assumption that lies squarely within UG. This assumption, and the larger-scale assumption that language is structured into distinct domains (syntax, phonology, the lexicon), is not trivially part of UG either, in the sense of a general bias toward learning language, but rather is a fundamental part of linguistic knowledge. AP&L's suggestion otherwise (p. e59) notwithstanding, computational models have yet to demonstrate that these classic building blocks emerge from broadscale distributional analysis.

Of course, it is important to differentiate between knowledge that syntactic categories exist, and knowledge of the presence of particular categories. AP&L's main point is to address specifically the claim that UG includes knowledge of PARTICULAR categories. They argue that crosslinguistic differences in what categories exist make this unlikely, and further that even if a limited number of such universal categories do exist, this does not solve the linking problem to the other categories. One important question not posed by AP&L is whether linking to one category, or a small number of categories, can help bootstrap to linking other categories. Even if this would only push the system forward a small amount, it would be a contribution worth noting, as distributional analyses have not solved the linking problem either. No existing distributional model can generate *de novo* a fully differentiated set of syntactic categories that accurately describes the complexity of a human language. This is not to say for certain that such a system cannot exist. But if intellectual rigor is demanded of nativists, the same level of rigor needs to be applied to distributional models. We need to move beyond the 'proof in principle' stage. AP&L dismiss concerns over level of analysis (i.e. how the infant knows to perform the specific kinds of distributional analyses necessary at the appropriate levels of phoneme, word, etc.), but this problem is at the crux of the matter. We do not yet know what types or extent of constraints is necessary to generate the right kinds of generalizations based on distributional analysis. Until that time, empiricists cannot claim victory any more than nativists can.

An alternative approach to resolving the linking problem is to dispense with the notion of syntactic categories altogether, innate OR learned (e.g. Bybee & McClelland

2005, Langacker 1987), and thereby dispense with the need for linking between input and representation. It is important to keep in mind that dispensing with the notion of syntactic categories essentially means dispensing with large swaths of basic linguistic theory over the last century. Syntactic categories as a concept emerged because they captured important insights about the nature of relationships between words that can be divorced in important ways from semantics AND simple distributional relationships (Chomsky's (1957) *Colorless green ideas sleep furiously*). If this notion of syntactic categories is to be lost, it needs to be replaced with something of equal theoretical value—a difficult task, but one worth pursuing.

3. CONCLUSION. AP&L's article might be viewed as the latest round in a long, tedious dispute that stems back at least as far as Chomsky and Skinner if not back to the outlawing of discussion of the origins of language in the mid-nineteenth century by the Société de Linguistique de Paris. Luckily, this dispute has yielded enormous fruit over the last half-century or more. Chomsky's original attack on the behaviorist approach to language learning has ultimately resulted in much more robust general-purpose learning mechanisms, particularly within the last few decades. These models are slowly picking away at nativist assumptions about what is and is not learnable in language. At the same time, those championing computational models of language acquisition are also increasingly sensitive to the depth of the challenge facing them. AP&L commend nativist approaches for 'captur[ing] aspects of the acquisition problem that might otherwise have been overlooked and identify[ing] cues and mechanisms that are likely to form part of the solution' (p. e81), but suggest that if UG cannot fully account for a phenomenon, it must be discarded. They are right in one sense, in that we are reaching a point where we must begin to progress beyond the simple explanations and toy problems. But this should be leveled equally to both sides of the argument. More importantly, if we have learned anything, it is that language acquisition is a difficult problem. If we are going to crack the mystery, it is going to involve an 'all hands on deck' approach. The solution MUST involve general-purpose learning mechanisms, some form of UG, and neutral-ground ideas like prosodic bootstrapping.

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[Received 23 January 2014;
accepted 28 April 2014]