PERSPECTIVES

Focus on facts not fiction: Commentary on Ambridge, Pine, and Lieven

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Ambridge, Pine, and Lieven could have provided a stronger argument for their conclusion, which postulated that innate universal-grammar-specified knowledge does not simplify the language learning task, had they not paid so much attention to the Chomskyan paradigm. I argue that poverty-of-the-stimulus arguments do not take into account that children are opportunistic learners employing multiple strategies, that they do not accomplish individual tasks sequentially but acquire (partial) knowledge about multiple domains simultaneously, and that they do not acquire perfect knowledge of language. Furthermore, work in formal linguistics suggests that the Chomskyan paradigm is internally incoherent and that the formalism of the Chomskyan framework lacks mathematical precision, making it difficult to evaluate its predictions. Given that linguistics ought to provide crucial input for language acquisition research, more attention needs to be paid to non-Chomskyan work in linguistics.*

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1. Introduction. Ambridge, Pine, and Lieven (2014; AP&L) provide compelling evidence supporting their conclusion that 'the innate UG-specified knowledge posited does not, in fact, simplify the task facing the [language] learner' (p. e53) with regard to the domains of identifying syntactic categories, acquiring basic morphosyntax, structure dependence, subjacency, and binding principles. Agreeing with AP&L's interpretation of the evidence and their main conclusion, I argue below that, regrettably, AP&L missed the opportunity to make a much stronger case against the postulation of an innately fixed UG. They give too much credit to proposals that are often only mentioned but not exposed to critical analysis. An unnecessary desire to be conciliatory makes AP&L vulnerable to harsh criticism (as expressed in Hornstein 2013b) and prevents them from taking a firmer stand against UG proposals.

In this commentary I expose a weakness in AP&L's argument and provide suggestions that could put an end to a fruitless debate that has occupied language acquisition research far too long. I first argue that even if one accepts AP&L's arguments on empirical grounds, these arguments are too weak to challenge the UG hypothesis (§2). I then argue that it is a mistake to (implicitly) focus on Chomskyan orthodoxy that postulates one genetically specified mechanism (or set of mechanisms) that is supposed to account for the acquisition of every possible human language (§3). Next, I introduce arguments from formal linguistics that show internal inconsistencies in the UG approach (§4). Finally, I conclude that combining arguments from developmental psychology (of the kind AP&L provide) with arguments from formal linguistics will result in a more powerful criticism of the Chomskyan paradigm than either of those approaches can achieve on its own.

2. The weakness of AP&L. The poverty-of-the-stimulus argument (e.g. Chomsky 1965, 1975, 1977, 1986, 1993, Lightfoot 1989, Musolino et al. 2000, Crain & Pietroski 2001, Legate & Yang 2002, Lidz et al. 2003, Baker 2005, Petitto 2005, Smith 2005) has

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convinced many linguists and psychologists that children rely on innate knowledge when learning their first language. The logic of the argument can be stated as follows.

(i) If one observes the input and output of a cognitive system C, and if the input alone cannot account for the output, then system C needs to contain an innate structure that is sufficiently rich to account for the difference between input and output.
(ii) The input to system C (primary linguistic data available to the language learner) is insufficient to explain the observed output (language performance) (poverty of stimulus).
(iii) System C therefore contains an innate structure that accounts for the difference between input and output (modus ponens from (i) and (ii)).

This is a valid deduction,[1] and if the premises are true, the conclusion follows. The first premise is usually accepted. If there is a difference between input and output, then an internal structure needs to account for the difference. But, many nonnativists have challenged the second premise and attempted to show that children receive sufficient input to account for their linguistic competence (e.g. MacWhinney & Snow 1985, 1990, MacWhinney 1995, Sampson 2002, Tomasello 2003, Behme & Deacon 2008, Behme 2014). AP&L follow a similar strategy in their article. They propose that for several linguistic domains (e.g. identifying syntactic categories, acquiring basic morphosyntax, structure dependence, subjacency, and binding principles) innate knowledge would not simplify the task facing the learner. They claim this is the case because (i) the input is not as impoverished as often assumed, and (ii) existing innate accounts are flawed because they suffer from problems of 'linking, inadequate data coverage, and redundancy' (p. e54).

Assuming for argument's sake that all of AP&L's claims are factually correct,[2] their approach has two weaknesses. First, establishing that the input is rich enough for some data-driven learning strategy to succeed neither establishes that children use this strategy nor rules out that children rely on innate knowledge. For example, AP&L argue that the input contains enough information that learners can 'use distributional learning to form clusters that correspond roughly to syntactic categories' (p. e56) and suggest that innate, prespecified categories to which these clusters would need to be linked are neither needed nor helpful.

A defender of nativism might suggest that AP&L overlook a 'chicken and egg' problem: the primary linguistic data contain the regularities reported by AP&L BECAUSE they have been generated by the kind of innate UG nativists postulate.[3] Given that a generative grammar provides a finite set of rules for combining constituents into larger units and can generate an infinitely large set of sentences (language), one should expect statistical regularities in the primary linguistic data. That some non-domain-specific computational mechanism can exploit these regularities is irrelevant to how children learn language. Given that (on the nativist view) UG facilitates syntactic category ac-

[1] The poverty-of-the-stimulus argument is rarely if ever stated as a deductive argument. I am using this form here because it simplifies the demonstration of the weakness in AP&L's approach. However, I am NOT suggesting that Chomsky or other nativists are committed to a deductive argument.

[2] Making this assumption does commit one to the belief that AP&L's argument could not be challenged on empirical grounds (e.g. it could turn out that full acquisition of the domains AP&L discuss is not possible without innate knowledge). But, given that an empirically weaker AP&L argument would be subject to the same problems I discuss, one can set aside such (potential) challenges.

[3] It is regrettable that AP&L focus mainly on (some of the) weaknesses of selected UG proposals but never address the foundational problems of the UG approach. These are discussed here in §4.

quisition, there must be a solution to the binding problem (and to other problems identified by AP&L for other domains). That the solution is currently not known does not entail that no solution exists. A human engineer might be able to design a general-purpose learner capable of acquiring syntactic categories by using the distributionally defined clusters directly, but children rely on UG. Assuming that UG is a product of messy evolution, one should expect redundancies, not efficiently designed computational solutions. AP&L admit they only 'have shown that none of the categories, learning procedures, principles and parameters proposed under CURRENT UG-based theories aid learning; [they] have not shown that such innate knowledge could not be useful IN PRINCIPLE' (p. e82).

This nativist strategy might considerably weaken the persuasive force of the poverty-of-the-stimulus argument. But, while a strong poverty-of-the-stimulus argument provides strong support for its conclusion, a weak poverty-of-the-stimulus argument does not establish that its conclusion is wrong. Hence, nativists might grant that by itself ' "You can't learn X without innate knowledge" is no argument for innate knowledge' (p. e82), but reject the stronger claim that they should give up nativism unless they can show how X can be learned with innate knowledge. Given that nonnativists do not offer a complete account of how children learn syntactic categories for every human language, it would be premature to give up nativism.

Second, even if it can be shown for SOME domains that the input is not too impoverished for data-driven learning mechanisms to succeed AND that those mechanisms exist, it does not follow that children do not depend on innate knowledge for the acquisition of OTHER domains. The nativist can accept that some domains, previously thought to require innate knowledge, can be learned based on the information contained in the primary linguistic data. However, nonnativists have not shown that ALL linguistic domains can be acquired without innate knowledge. In other words, AP&L's having established that the poverty-of-the-stimulus argument is weak for linguistic domains D_1, D_2, D_3, D_4, D_5 does not provide any support for the assumption that it will be equally weak for domain D_n.

Defenders of the MINIMALIST PROGRAM might even welcome AP&L's demonstration that some domains can be acquired without UG, because minimalists (e.g. Chomsky 1995, Adger 2003, Radford 2004, Boeckx 2006) attempt to reduce the complexity of the postulated innate resources. One 'important reason [for wanting to minimize UG] is that any special mental capacity has to be encoded somehow on the genome, such that the genome builds a brain with special information-processing capability' (Jackendoff 2011:588). But, they would argue that SOME linguistic domains cannot be learned from the input alone. This objection cannot be overcome unless one either (i) offers a complete account of language acquisition that does not depend on innate knowledge or (ii) provides independent arguments that challenge the plausibility of an innate UG. AP&L are in no position to offer (i) and provide no explicit argument for (ii). Hence, even the modest conclusion that 'nothing is gained by positing components of innate knowledge that do not simplify the problem faced by language learners' (p. e82) appears too strong. If one accepts that UG exists, it might be more parsimonious to assume that UG also contributes to domains for which alternative learning strategies are possible. In the next section I propose stronger empirical arguments that challenge UG.

3. EMPIRICAL CHALLENGES TO THE ORTHODOXY. Even though AP&L focus on 'specific proposals for particular components of innate knowledge' (p. e54) by innatists, they (implicitly) accept the Chomskyan orthodoxy that ONE genetically specified mech-

anism (or set of mechanisms) accounts for the acquisition of every possible human language. I argue below that liberating theorizing from the still powerful influence of this orthodoxy allows abandoning the futile search for a one-size-fits-all solution to the language acquisition problem.

Decades of work in developmental psychology (some of which AP&L discuss) have produced results that strongly suggest that children rely simultaneously on several general-purpose mechanisms when they learn language (e.g. Christiansen et al. 1998, MacWhinney 2004, Blanchard et al. 2010, Christiansen et al. 2010, Perruchet & Tillmann 2010, Rytting et al. 2010) and that not all mechanisms work equally well for all languages (e.g. Blanchard et al. 2010, Karaminis & Thomas 2010).

AP&L provide compelling evidence suggesting that children can learn several linguistic domains from primary linguistic data. But they consider each domain in isolation. While this approach might simplify analysis for researchers, it is implausible that children acquire domains in isolation or test multiple mutually exclusive hypotheses. Instead they are opportunistic learners and employ 'whatever works' strategies in the language acquisition task (e.g. MacWhinney 2004). Further, they do not accomplish individual tasks sequentially but acquire (partial) knowledge about multiple domains simultaneously and use this knowledge as stepping stones for more complex tasks (e.g. Maye et al. 2002).

Naturally, the earliest language acquisition tasks would profit most from contributions of an innate UG, and one would have expected AP&L to focus on these tasks (which are often neglected by nativists; for discussion see Behme 2014). Research on these early tasks suggests that they can be accomplished by non-domain-specific mechanisms, and that different languages provide different challenges. One of the earliest language acquisition tasks, word segmentation, can be simulated by computational models that exploit several cues simultaneously (e.g. Christiansen et al. 1998, Blanchard et al. 2010, Monaghan & Christiansen 2010). Recent findings suggest that the strategies leading to successful word segmentation vary depending on the language learned. For example, Blanchard and colleagues' model PHOCUS relies on very basic assumptions about language learning. Beginning with an empty lexicon, it incrementally adds items to the lexicon, based on phonemes that occur together (probabilistic and phonotactic cues). Phonemes that occur within frequent words have high transitional probabilities, while phonemes that cross word boundaries have low transitional probabilities (e.g. Saffran et al. 1996). In addition to these transitional probabilities, which are helpful to detect word boundaries, the Blanchard et al. model could exploit phonotactic cues. Specifically, when the model encountered an unfamiliar word, it could rely on two kinds of phonotactic cues: phoneme combinations, and occurrence of at least one syllabic sound per word (for details see Blanchard et al. 2010:496–501).

Blanchard and colleagues could show that the combination of these two simple cues allowed a performance of 76–81% precision/recall scores for an English test corpus. Unexpectedly, the same model performed substantially worse (19–47% precision/recall scores) on a Sesotho corpus (Blanchard et al. 2010:503). This result 'highlights the importance of testing acquisition models on data from a variety of languages because the results can be so different from what is obtained with English corpora' (ibid., 505). The difference in performance is explained by the fact that the most frequent word in the Sesotho sample is monosyllabic. This results in a very high percentage of oversegmentation errors from which the model cannot recover. Obviously, children learning Sesotho are able to master the word-segmentation task. This indicates that they cannot rely on the same cues as the model used by Blanchard and colleagues. A model that in-

corporates more cues than the Blanchard et al. model simultaneously might succeed in the Sesotho word-segmentation task. What these cues are and how they interact with one another is a matter of ongoing research.

Other researchers have demonstrated that combining models (which had been originally used for independent tasks) can increase the generality of the models across inflection types and grammatical classes, and across languages. Karaminis and Thomas (2010) combined elements of previous connectionist models of morphology to implement a generalized inflectional system. Their 'Multiple Inflectional Generator (MIG) considered three grammatical classes (nouns, verbs, and adjectives) and multiple inflections for each grammatical class (e.g. nouns: base forms, plurals, and possessives)' (Karaminis & Thomas 2010:732). Preliminary results show that MIG can reproduce error patterns and accuracy levels of inflection acquisition.

In both English and Modern Greek, an OPTIONAL INFINITIVE stage was observed, even though the character of that stage is different in each language (unmarked stems vs. third-person singular, respectively). Generalization rates of the past-tense rule were high for novel stems, even for phonotactically illegal stems. MIG captured the order of emergence of different inflection types for different grammatical classes. And it was able to capture developmental patterns for two languages of different morphological complexity (Karaminis & Thomas 2010:734).

These two examples are suggestive. They indicate the problems any innatist account faces. On the one hand, it is implausible that any biologically plausible mechanism could encode all of the specific information required for the acquisition of every possible human language. On the other hand, if this information is not part of the innate endowment but has to be learned from the input, innatism offers no substantial advantage over nonnativist accounts. Some of the discussion of crosslinguistic research by AP&L could have focused on the different challenges posed by different languages. This would have added force to their argument for nonnativist solutions.

Nativists may wish to sidestep the 'crosslinguistic dilemma' by conceding that many or even all early language acquisition tasks can be accomplished by domain-general mechanisms that exploit statistical regularities and other information contained in the input. However, more sophisticated linguistic structures, which the child learns later on, cannot be learned by domain-general mechanisms that have access only to the primary linguistic data. In other words, from the fact that SOME ASPECTS of language can be learned without reliance on innate knowledge it does not follow that no domain of language acquisition requires innate knowledge. Seemingly, this proposal greatly reduces the amount of knowledge that needs to be innately specified. Even under this proposal, however, innate resources for every possible human language need to be specified (because every child can (potentially) learn any human language). Given how little is known currently about many human languages, at the very least it is premature to assume that one genetically fixed mechanism can fully account for the acquisition of domains across the range of all human languages. So far nativists have not revealed how any of the proposed innate constraints are instantiated in human brains. Drawing attention to this fact can add strength to AP&L's arguments. In the next section I discuss arguments from formal linguistics challenging the plausibility of the Chomskyan proposal even for a single language.

4. LESSONS FROM FORMAL LINGUISTICS. Language acquisition researchers will benefit from taking seriously Katz's (1981) proposal that the study of psychological mechanisms that underwrite language acquisition and the study of the (syntactic) structure of

natural language are two distinct fields of inquiry. Chomsky's proposals have conflated the study of knowledge of language (a branch of psychology) and the study of natural language (a branch of formal linguistics). While it is possible to study natural language without paying close attention to findings in experimental psychology, the study of knowledge of language should be informed by the best available linguistic theories. Unfortunately, AP&L have focused too narrowly on linguistic theories proposed under the influence of Chomskyan orthodoxy and, especially, on work that has been done with the aim to refute nonnativist accounts of language acquisition. Such work naturally leaves out vast areas of linguistic research and ignores accounts that offer alternative explanations for linguistic domains that need to be considered by any language acquisition account (e.g. Seuren 1996, de Groote 2001, Muskens 2003, Pollard 2011, Postal 2011, Kubota & Levine 2012, Sampson & Babarczy 2013).

Furthermore, work in formal linguistics shows that the Chomskyan paradigm is internally incoherent (Katz 1981, 1990, 1996, Langendoen & Postal 1984, Postal 2003, 2009, 2012, Behme 2013, 2014, Neef 2014). And, given that most if not all UG proposals are based on Chomsky's notion of a generative grammar, they are equally flawed. One form of the poverty-of-the-stimulus argument attempts to establish that a child could not possibly learn a language from the input because '[t]he child's experience is finite but the capacity eventually attained ranges over an infinite domain and must therefore incorporate some recursive property not demanded by experience' (Lightfoot 1989:322). This form of the argument receives little attention in the literature and is usually accepted without critical evaluation. As I show below, however, accepting this form of the argument commits the defender of UG to an internally incoherent ontology.

The logic of Lightfoot's argument can be stated as follows.

(i) If one observes the input and output of a cognitive system C, and if the input alone cannot account for the output, then system C needs to contain an innate structure that is sufficiently rich to account for the difference between input and output.
(ii) The input to system C (finite experience) is insufficient to explain the observed output (capacity reaching over an infinite domain) (poverty of the stimulus).
(iii) System C therefore contains an innate structure that accounts for the difference between input and output (modus ponens from 1 and 2[4]).

The input any child receives is undoubtedly finite. And, if her linguistic competence ranges over an infinite domain, this competence extends nontrivially over the input. This much is uncontroversial indeed. But, if the child's ability to acquire linguistic competence is based on a grammar that is a component (or state) of his brain, then any capacity attained can only be finite as well. This fact is often obscured because Chomsky habitually conflates knowledge of language (a psychological brain state) and language itself (a formal object consisting of sets of sentences).

Any natural language that is generated by a generative grammar with recursive rules is nonfinite. Assuming a one-to-one relationship between the sentences of such a language (types) and physical copies of these sentences (tokens), which can be stored in or manipulated by human brains, makes it clear that nativism faces the same problem as (nominalist) nonnativism because 'there are too many sentences in a natural language for them to have either concrete acoustic reality or concrete psychological or neural re-

[4] In this case it is unproblematic to propose a deductive argument because it is not merely implausible but impossible for any finite input to account for an output ranging over an infinite domain.

ality' (Katz 1996:270). Chomsky recognized that nominalist grammars (taxonomic descriptions of speech) cannot account for the abstract properties of natural languages. Katz argues persuasively, however, that 'Chomskyan linguistics [fails] to solve the problem of the abstractness of grammatical structure … [because] nothing essential changes when the concreteness of stretches of physical sound is replaced with the concreteness of mental/neural states' (1996:272–77). Like nominalism, the Chomskyan approach falls well short of accounting for all of the possible sentences of a natural language like English:

> [Chomskyan] linguistics requires that there be infinitely many real objects to serve as referents for the linguist's statements about infinitely many sentences of a natural language, while a concrete linguistic reality guarantees that there are none for the infinitely many possible but never-to-be-actual sentences. (Katz 1996:279)

If language is (i) a biological organ (as claimed in Chomsky 1986, 2002, 2012), then it is finite. If language is (ii) a collection of potentially infinitely many sentences or expressions (as claimed in Chomsky 1965, 1968, 1977, 2012), then finite human brains can at best instantiate a very small part of language. And if language is (iii) an abstract object (as claimed in Chomsky 1957, 1961, 1977, 2012), then the nature of the relationship between language and brains needs to be explained. Any view claiming that language is (i), (ii), AND (iii) is internally incoherent and should be rejected for this reason. It is possible to maintain that knowledge of language ((i) and/or (ii)) and language (iii) are distinct. But in this case innatism faces the same Lightfootian poverty-of-the-stimulus problem as (nominalist) nonnativism.

Furthermore, the formalism of the Chomskyan framework (and many UG proposals assuming this framework) lacks mathematical precision, making it difficult to evaluate its predictions. Montague (1970) called Chomskyan grammar mathematically imprecise and unsystematic, and others remarked that 'in some [Chomskyan] works we even find purported theorems being stated without any proof being suggested, or theorems that are given "proofs" that involve no definition of the underlying class of grammars and are thus empty' (Gazdar et al. 1985:14). Chomskyan nativists have failed to specify criteria for the boundary between the grammatical and the ungrammatical utterances of any language, and '[n]o one has ever successfully produced a comprehensive and accurate grammar of any language' (Graddol 2004:1329). These problems made it difficult to evaluate the mathematical implications of the continual recasting of Chomsky's theories. But the foregoing has shown that the problems reach far deeper than AP&L suggest. Not only have Chomsky-inspired nativists failed to specify for any X how one 'can learn X WITH innate knowledge, and here's one way that a child could do so' (p. e82), but to date nativists have also not even specified what it is that the child needs to learn. The attempts to formalize learning algorithms by Chomskyan nativists (even for the examples used to establish the alleged superiority over nonnativism) remain sketchy and are often less plausible than alternatives proposed under competing linguistic frameworks.[5]

[5] A full discussion of this issue goes beyond the scope of this commentary. Interested readers may consult Hornstein (2013a, discussion), which reveals the inability of leading Chomskyan nativists to demonstrate superiority over competing proposals (e.g. analysis of the example *Instinctively eagles that fly can swim* by Adger (based on minimalism) and Behme (based on any category logic with a nondirectional, intuitionistic implication operator —o such as Muskens's LAMBDA GRAMMAR, de Groote's ABSTRACT CG, Pollard's LINEAR CATEGORIAL GRAMMAR, or Levine and Kubota's HYBRID TYPE-LOGICAL GRAMMAR)).

5. Conclusions. I have argued that, in spite of providing strong empirical evidence, AP&L's arguments are too weak to support their conclusions. However, combining AP&L's arguments with additional evidence from developmental psychology (especially crosslinguistic studies) and formal linguistics provides sufficient evidence suggesting that the Chomskyan orthodoxy has outlived its usefulness and that a refocus of language acquisition research is long overdue. AP&L have argued that 'there is no working UG-based account of any of the major phenomena in language acquisition; current accounts of this type explain the data only to the extent that they incorporate mechanisms that make no use of innate grammatical knowledge' (p. e82). Furthermore, the Chomskyan conflation of knowledge of language and language obscures the internal incoherence of Chomskyan innatist accounts. Katz (1996) has argued convincingly that the Chomskyan revolution of the 1950s was never completed, and language acquisition researchers need to be aware that an innate UG cannot overcome the Lightfootian poverty-of-the-stimulus challenge. Hence, the Chomskyan paradigm is not superior to nonnativist accounts. None of the arguments discussed here rule out that innate, language-specific knowledge exists. But, the burden of proof is on proponents of UG. Until they provide internally coherent and neurophysiologically plausible proposals, there is little reason to privilege their proposals over those by other linguists.

REFERENCES

Adger, David. 2003. *Core syntax*. Oxford: Oxford University Press.

Ambridge, Ben; Julian M. Pine; and Elena V. M. Lieven. 2014. Child language acquisition: Why universal grammar doesn't help. *Language* 90.3.e53–e90.

Baker, Mark. 2005. The innate endowment for language. *The innate mind: Structure and contents*, ed. by Stephen Laurence, Peter Carruthers, and Stephen Stich, 156–74. Oxford: Oxford University Press.

Behme, Christina. 2013. Biolinguistic Platonism remains an oxymoron. Nova Scotia: Dalhousie University, ms. Online: http://ling.auf.net/lingbuzz/001765.

Behme, Christina. 2014. *Evaluating Cartesian linguistics: From historic antecedents to computational modeling*. (Potsdam linguistic investigations 12.) Bern: Peter Lang.

Behme, Christina, and S. Hélène Deacon. 2008. Language learning in infancy: Does the empirical evidence support a domain specific language acquisition device? *Philosophical Psychology* 21.641–71.

Blanchard, Daniel; Jeffrey Heinz; and Roberta Golinkoff. 2010. Modeling the contribution of phonotactic cues to the problem of word segmentation. *Journal of Child Language* 37.487–511.

Boeckx, Cedric. 2006. *Linguistic minimalism: Origins, methods, concepts, and aims*. New York: Oxford University Press.

Chomsky, Noam. 1957. *Syntactic structures*. The Hague: Mouton.

Chomsky, Noam. 1961. Some methodological remarks on generative grammar. *Word* 17. 219–39.

Chomsky, Noam. 1965. *Aspects of the theory of syntax*. Cambridge, MA: MIT Press.

Chomsky, Noam. 1968. Linguistics and philosophy. *Language and philosophy*, ed. by Sidney Hook, 51–94. New York: New York University Press.

Chomsky, Noam. 1975. Recent contributions to the theory of innate ideas. *Innate ideas*, ed. by Stephen Stich, 121–31. Berkeley: University of California Press.

Chomsky, Noam. 1977. *Essays on form and interpretation*. New York: North-Holland.

Chomsky, Noam. 1986. *Knowledge of language*. New York: Praeger.

Chomsky, Noam. 1993. *Language and thought*. Wakefield, RI: Moyer Bell.

Chomsky, Noam. 1995. *The minimalist program*. Cambridge, MA: MIT Press.

Chomsky, Noam. 2002. *On nature and language*. Cambridge: Cambridge University Press.

Chomsky, Noam. 2012. *The science of language: Interviews with James McGilvray*. Cambridge: Cambridge University Press.

Christiansen, Morten H.; Joseph Allen; and Mark Seidenberg. 1998. Learning to segment speech using multiple cues: A connectionist model. *Language and Cognitive Processes* 13.221–68.
Christiansen, Morten H.; Rick Dale; and Florencia Reali. 2010. Connectionist explorations of multiple-cue integration in syntax acquisition. *Neoconstructivism: The new science of cognitive development*, ed. by Scott Johnson, 87–108. New York: Oxford University Press.
Crain, Stephen, and Paul M. Pietroski. 2001. Nature, nurture and universal grammar. *Linguistics and Philosophy* 24.139–86.
de Groote, Philippe. 2001. Towards abstract categorical grammars. *Proceedings of 39th annual meeting of the Association for Computational Linguistics (ACL '01)*, 148–55.
Gazdar, Gerald; Ewan Klein; Geoffrey Pullum; and Ivan Sag. 1985. *Generalized phrase structure grammar*. Oxford: Blackwell.
Graddol, David. 2004. The future of language. *Science* 303.1329–31.
Hornstein, Norbert. 2013a. A cute example (and discussion). Faculty of Language, September 8, 2013. Online: http://facultyoflanguage.blogspot.ca/2013/09/a-cute-example.html.
Hornstein, Norbert. 2013b. Mothers know best (and discussion). Faculty of Language, October 21, 2013. Online: http://facultyoflanguage.blogspot.ca/2013/10/mothers-know-best.html.
Jackendoff, Ray. 2011. What is the human language faculty? Two views. *Language* 87.3. 586–624.
Karaminis, Themis, and Michael Thomas. 2010. A cross-linguistic model of the acquisition of inflectional morphology in English and Modern Greek. *Proceedings of the 32nd annual conference of the Cognitive Science Society*, 730–35.
Katz, Jerrold J. 1981. *Language and other abstract objects*. Totowa, NJ: Rowman and Littlefield.
Katz, Jerrold J. 1990. *The metaphysics of meaning*. Cambridge, MA: MIT Press.
Katz, Jerrold J. 1996. The unfinished Chomskyan revolution. *Mind and Language* 11. 270–94.
Kubota, Yusuke, and Robert Levine. 2012. Gapping as like-category coordination. *Logical Aspects of Computational Linguistics: 7th international conference, LACL 2012, Nantes, France, July 2–4, 2012, proceedings*, ed. by Denis Bechet and Alexandre Dikovsky, 135–50. New York: Springer.
Langendoen, D. Terence, and Paul M. Postal. 1984. *The vastness of natural language*. Oxford: Blackwell.
Legate, Julie Anne, and Charles D. Yang. 2002. Empirical re-assessment of stimulus poverty arguments. *The Linguistic Review* 19.151–62.
Lidz, Jeffrey; Sandra R. Waxman; and Jennifer Freedman. 2003. What infants know about syntax but couldn't have learned: Experimental evidence for syntactic structure at 18 months. *Cognition* 89.B65–B73.
Lightfoot, David. 1989. The child's trigger experience: Degree-0 learnability. *Behavioral and Brain Sciences* 12.321–34.
MacWhinney, Brian. 1995. *The CHILDES project: Tools for analyzing talk*. 2nd edn. Hillsdale, NJ: Lawrence Erlbaum.
MacWhinney, Brian. 2004. A multiple process solution to the logical problem of language acquisition. *Journal of Child Language* 31.883–914.
MacWhinney, Brian, and Catherine Snow. 1985. The child language data exchange system. *Journal of Child Language* 12.271–96.
MacWhinney, Brian, and Catherine Snow. 1990. The child language data exchange system: An update. *Journal of Child Language* 17.457–72.
Maye, Jessica; Janet F. Werker; and LouAnn Gerken. 2002. Infant sensitivity to distributional information can affect phonetic discrimination. *Cognition* 82.B101–B111.
Monaghan, Padraic, and Morten H. Christiansen. 2010. Words in puddles of sound: Modelling psycholinguistic effects in speech segmentation. *Journal of Child Language* 37.545–64.
Montague, Richard. 1970. Universal grammar. *Theoria* 36.373–98.

Muskens, Reinhard. 2003. Language, lambdas, and logic. *Resource sensitivity in binding and anaphora*, ed. by Geert-Jan Kruijff and Richard T. Oehrle, 23–54. London: Kluwer.
Musolino, Julien; Stephen Crain; and Rosalind Thornton. 2000. Navigating negative semantic space. *Linguistics* 38.1–32.
Neef, Martin. 2014. Das nächste Paradigma: Realistische Linguistik: Eine Ergänzung zum Beitrag *Wo stehen wir in der Grammatiktheorie?* von Wolfgang Sternefeld und Frank Richter. *Muttersprache* 124.2.105–20.
Perruchet, Pierre, and Barbara Tillmann. 2010. Exploiting multiple sources of information in learning an artificial language: Human data and modeling. *Cognitive Science* 34.255–85.
Petitto, Laura. 2005. How the brain begets language: On the neural tissue underlying human language acquisition. *The Cambridge companion to Chomsky*, ed. by James McGilvray, 84–101. Cambridge: Cambridge University Press.
Pollard, Carl. 2011. Proof-theoretic background for linear grammar. Columbus: The Ohio State University, ms.
Postal, Paul M. 2003. Remarks on the foundations of linguistics. *The Philosophical Forum* 34.233–51.
Postal, Paul M. 2009. The incoherence of Chomsky's 'biolinguistic' ontology. *Biolinguistics* 3.104–23.
Postal, Paul M. 2011. *Edge-based clausal syntax*. Cambridge, MA: MIT Press.
Postal, Paul M. 2012. Chomsky's foundational admission. New York: New York University, ms. Online: http://ling.auf.net/lingbuzz/001569.
Radford, Andrew. 2004. *Minimalist syntax: Exploring the structure of English*. Cambridge: Cambridge University Press.
Rytting, C. Anton; Chris Brew; and Eric Fosler-Lussier. 2010. Segmenting words from natural speech: Subsegmental variation in segmental cues. *Journal of Child Language* 37.513–43.
Saffran, Jenny R.; Richard N. Aslin; and Elissa L. Newport. 1996. Statistical learning by 8-month-old infants. *Science* 274.1926–28.
Sampson, Geoffrey. 2002. Exploring the richness of the stimulus. *The Linguistic Review* 19.73–104.
Sampson, Geoffrey, and Anna Babarczy. 2013. *Grammar without grammaticality*. Berlin: De Gruyter Mouton.
Seuren, Pieter A. M. 1996. *Semantic syntax*. Oxford: Blackwell.
Smith, Neil V. 2005. Chomsky's science of language. *The Cambridge companion to Chomsky*, ed. by James McGilvray, 21–41. Cambridge: Cambridge University Press.
Tomasello, Michael. 2003. *Constructing a language: A usage-based theory of language acquisition*. Cambridge, MA: Harvard University Press.

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