The linking problem is a special case of a general problem none of us has solved

Commentary on Ambridge, Pine, and Lieven

Beekhuizen, B.; Bod, R.; Verhagen, A.

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PERSPECTIVES

The linking problem is a special case of a general problem none of us has solved: Commentary on Ambridge, Pine, and Lieven

Barend Beekhuizen — *Leiden University*
Rens Bod — *University of Amsterdam*
Arie Verhagen — *Leiden University*

In this commentary, we focus on the linking problem Ambridge, Pine, and Lieven identify. Instead of taking a stance on the issue of universal grammar itself, we adopt an epistemological and methodological perspective on language acquisition research. We argue that the problem, linking the input to preexisting representations, constitutes just a small part of a larger methodological problem, namely how to link the input a learner receives, through a set of learning mechanisms and possibly innate representations, to the behavior the learner is producing, whether in spontaneous production or in laboratory experiments. Currently, none of the existing proposals provide an evaluated, or even a testable, account of the full process, that is, an input-output model of linguistic ontogeny. Although the focus on phenomena in isolation, probably an effect of the prevalence of the experimental method, allows the researcher some degree of control, it also distracts from the understanding of how the different mechanisms behave and interact. Our proposed solution is a more HOLISTIC approach to the problem in which the learning mechanisms and their interaction are made fully EXPLICIT, and in which the predicted behavior of the learner is (more) GLOBALLY evaluated. Computational modeling provides exactly the tools appropriate for this task, thereby furthering more precise and testable theories of the learning mechanisms involved.*

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1. The linking problem is everyone's problem. In their article, Ambridge, Pine and Lieven (2014; AP&L) pose the question of whether existing proposals of UNIVERSAL GRAMMAR (UG) are helpful in acquiring certain aspects of syntax, and they identify three problems with existing accounts: (i) there is no successful proposal for how the learner can link innate representations to the input language, (ii) the representations would yield incorrect empirical predictions, and (iii) the innate representations provide no help over known general learning mechanisms (p. e54–e55). In this commentary, we zoom in on the first problem and argue that it forms just the tip of a methodological iceberg in the field of language acquisition as a whole.

The linking problem, as AP&L define it, involves three components: input data, mechanisms that process the input data, and substantive innate representations. Given the assumption that certain aspects of syntax would be unlearnable without these latter representations, AP&L argue that this imposes a burden of proof on those assuming it to show that the actual proposals for these representations do make the allegedly unlearnable aspects of syntax learnable. This requires a linking of the outcomes of a set of assumed cognitive processing operations on the input data, on the one hand, to the hypothesized innate representations, on the other. It is for this linking operation that AP&L observe a lack of proposals showing how it takes place. They furthermore observe that the output of the processing mechanisms on the input itself may suffice as a representation (p. e57), since the mere processing mechanisms for inducing certain categories lead to empirically adequate clusters that may form sufficient representations themselves.[1]

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[1] Strikingly, the very same linking problem applies to the substantive conceptual representations (such as 'being an object' or 'extendedness') found in many usage-based approaches. It is thought that these basic rep-

The linking problem AP&L discuss seems to be a special case of a more general problem, namely that of connecting two or more components of a theoretical explanation by providing empirical evidence for their joint functionality. When we acknowledge this general problem, it is not just a UG-driven approach that suffers from it: any current theory has gaps where components should be connected.

The million-dollar question is what mechanisms and representations (potentially unlearned, potentially domain-specific) exist so that the learner, given the input, gradually comes to behave like an adult language user. Providing an explanation in which the joint functionality of the cognitive mechanisms (and possibly innate representations) is empirically supported requires several things. First, a high degree of explicitness about the mechanisms is needed, more than is typically provided. Second, in many current accounts, the focus is strongly on the mechanisms and representations INDIVIDUALLY, as evaluated against ISOLATED cases of developmental phenomena, but little is said about how the system as a whole operates on the full set of input data, so that effectively it is hard to determine the exact role of all individual hypothesized mechanisms. Summarizing, we could say that in much current work on language acquisition, underspecified individual mechanisms are evaluated against individual empirical cases.

When we say 'explicit', we mean, ideally, the verbal operationalization of a theoretical concept to such an extent that it is testable and that all aspects of the mechanism are described in such a way that one could mechanistically apply it to input data. An evaluation of the whole of a proposed system of mechanisms should then show how the mechanisms in interaction apply more or less blindly to the input data. That is to say: the mechanisms cannot be invoked in an ad hoc manner to explain certain phenomena. If a learner has a certain mechanism at its disposal, it initially does not know when and where to apply it, so that it should in principle apply to all input.

Under these strict requirements, we have to conclude that we currently have precious little knowledge about what cognitive mechanisms and representations drive language acquisition. Daunting as the global question may seem, we believe that we can arrive at a more comprehensive understanding of language acquisition only if we consider the system as a whole: that is, no mechanism operates in full isolation from the other mechanisms (and hence cannot be fully evaluated as such), and evaluation cannot be done only on isolated grammatical phenomena—global evaluation methods, as used in computational linguistics, should become part and parcel of the acquisitional research enterprise.

2. THE ISSUE OF HOLISM: THE INTERACTION OF THE SYSTEM. As we stated earlier, the study of a mechanistically applied set of processing operations in interaction is at this point underdeveloped. Take a usage-based perspective on the first two cases presented by AP&L: learning word classes and learning syntactic roles. As AP&L point out, both can be learned using domain-general distributional mechanisms. However, the learner will have to learn both at the same time. Moreover, 'domain-general distributional mechanisms' is not explicit enough to be properly evaluated: What exactly are the mechanisms? How do they algorithmically work? Are the same mechanisms involved in determining the paradigmatic nature of word classes and the syntagmatic nature of

resentations form the building blocks of the 'conceptual archetypes' later obtained through experience, and that these basic substantive representations are '[p]resumably innate' (Langacker 2008:34). Here, we find a parallel linking problem to the one AP&L pose for UG: if the basic representations are thought to help in developing a system of conceptual archetypes, it has to be shown how these representations and the input are linked in such a way that the conceptual archetypes emerge. We believe that this parallel case underscores our point that the linking problem really is everyone's problem.

notions like subject and object? Do the mechanisms for finding syntagms and paradigms operate simultaneously, or does one come in later? That is, usage-based theorists assume that two aspects of syntax can be learned with (roughly) the same mechanism, but (yet) fail to show how the mechanism can be applied blindly to the input data such that both paradigmatic categories and syntagmatic roles emerge.

A third component can be added to the equation: constructionist approaches attribute the emergent productivity of a learner to mechanisms of schematization and analogy (Tomasello 2003:122–26). The idea is that learners will proceed from fully fixed, chunk-like holophrases, through phases of semiproductive item-based constructions, to an adult state where many constructions have open-ended possibilities, while others remain of a more limited scope. Here, no explicit account exists for the way the now-three mechanisms are jointly applied to the input data so that the observed pattern of increasing abstraction emerges. Using analytical computational models, we can find out THAT there must be increasingly abstract grammars behind the child's productions over ontogenetic time (e.g. Bannard et al. 2009, Borensztajn et al. 2009), but HOW these grammars come about is not answered. Exactly the same questions as the ones above can be asked, but now with two pairwise interactions.

Complicating matters even further: How does the set of interacting mechanisms yield the kind of productions we find for young children? How does the learner process language in such a way that it will not produce all arguments of an argument structure construction? Is this a function of processing power, or are the underlying representations simply incomplete from an adult perspective? Understanding the causes of the child's typical early productions requires integrating the mechanisms for acquiring form-meaning pairings, creating syntagmatic and paradigmatic links, forming abstractions, and (potentially) processing capacity limitations. To the best of our knowledge, few studies incorporate anything approaching this full range.

With these questions and analyses of gaps in the theory, we do not mean to be dismissive of the scientific program of usage-based linguistics. However, the bigger linking problem—to show that the mechanisms do not just explain phenomena by themselves, but also function in interaction to yield the correct predictions on the behavioral side (in comprehension and production, in experimental settings and 'in the wild')—remains unsolved.

Nor do we want to suggest that questions like these are properly addressed by nativist/generativist accounts. As AP&L show, linking the input data to an independently existing set of representations is highly problematic. Methodologically, the appropriate rebuttal to this should not be that the representations AP&L present (NOUN, VERB, subject, and object, to name a few) are not the ones anyone works with anymore. Rather, it should be to provide an explicit, and hence empirically testable, account of how mechanisms in interaction can connect the information in the input data with the representation, answering the same questions as for the usage-based paradigm.

Importantly, nativist approaches are often already far more explicit about the interaction between components, possibly due to the assumptions concerning the ontological modularity of the different components, which makes the question of the interaction of modules more salient. Gleitman (1990, Gleitman et al. 2005), for instance, is quite precise in describing the mechanisms that account for word learning. The learner starts with some form of cross-situational, or distributional, learning, from which it can bootstrap the parameters of basic syntax, which are subsequently used to bootstrap meanings that cannot be (easily) learned from cross-situational observation. Problems with the account and matters of evaluation notwithstanding, it IS an explicit account of how the mechanisms interact. For the phenomena of early language production, precise descriptions of what causes the child to produce the typical truncated utterances of early production are made and evaluated (Pinker 1984, Boster 1997, Berk & Lillo-Martin 2012; for an explicit nonnativist proposal, see Schlesinger 1971).

In line with Bod's (2009a:130) criticism, we believe the global goal of a theory of the acquisition of a grammar should be, in the first place, '[a] description of an input-output procedure [or] a process-based model that tells us how new utterances are produced on the basis of previous utterances'. As Bod notes (p. 131), 'almost any current linguistic theory … has given up on the construction of a precise, testable model of language use and language acquisition'. Explicitly describing the algorithmic nature of the cognitive processes, as well as their interaction, is paramount for arriving at that goal.

3. The issue of evaluation: the global perspective. The linking problem also extends in another direction. As we emphasized, the explicit, mechanistic description of a set of interacting cognitive processes is required to obtain a more comprehensive understanding of the acquisition of grammar. A corollary of this is that all input data have to go through this 'sieve', and hence that the same set of processes has to be active in explaining all grammatical phenomena. This means that if some processing mechanism applies only to a certain phenomenon, or does not apply only to that phenomenon, or applies differently to that phenomenon (modulo the nature of the input), further explication is necessary for the theorizing not to be ad hoc.

What we see, however, is that most studies on the acquisition of grammar work on a case-by-case basis. In discovering the interesting phenomena, and in order to gain some level of depth of understanding, this is necessary. It also seems a natural consequence of the prevalent experimental method: the degree of experimental control can only be obtained by pitting, for instance, two closely related syntactic phenomena as minimally different conditions against each other, or by only using one syntactic phenomenon and varying some other dimension. However, what these kinds of studies do not test is how the mechanisms fare when applied to the full breadth of input. We propose that an evaluation of the global performance of a system is a necessary addition to the case-driven work in solving the bigger linking problem.

We should emphasize here that for some domains we do find global evaluations. Word-class learning has been well studied, as AP&L note, with different computational algorithms that, through some sort of distributional analysis, arrive at clusters that can be globally evaluated. The use of gradual schematization, from a usage-based account, has been circumstantially shown to apply on the full breadth of a corpus of child-directed speech (cf. Bannard et al. 2009). Note that the model that was used was not a mechanistic description of the cognitive processes necessary to arrive at an ever more abstract set of constructions, but an analytical model that tested how likely different hypotheses about the grammar generating the data were. Work on parameter setting exactly lacks this kind of breadth: when evaluating only a handful of parameters, as in Legate & Yang 2007, the applicability of a complete parametric system on the full set of data is not shown.

Obviously, explaining the full set of phenomena with a single set of mechanisms is not a feasible scientific goal. However, if we believe that the model starts with one set of processing mechanisms, content representations, and structure representations, it should at least be a goal to show that that very same set yields the desired behavioral outcome for as many different sorts of phenomena as possible. Perhaps the concept of evaluating on the full breadth of the data with a single comprehensive set of mechanisms is an ideal that we will never reach, but that nonetheless should guide our methodology and theory development. But we definitely do not want to suggest that a more global evaluation should REPLACE case-oriented research: specific cases can be very insightful in the study of some mechanism—for example, the study of long-distance WH-questions and their peaky type/token distribution sheds light on the radial category nature of syntactic constructions in general within a usage-based framework (Verhagen 2005, Dąbrowska et al. 2013).

4. COMPUTATIONAL MODELING AS THEORY FORMATION. A method that can help in resolving aspects of the bigger linking problem is computational modeling, and especially modeling that explicates aspects of the processing side of human cognition (that is, models operating on an algorithmic level in terms of Marr 1982). In a sense, one could argue that these kinds of computational models explicate theory formation to such an extent that a computer could carry it out. This means that they satisfy the explicitness requirement.

Moreover, using modeling techniques, one can show how different processing operations behave in an interacting way on the data. A good example is the MOSAIC model, which works from first principles on memory, and in which chunking operations and generalization mechanisms interact in an incremental way on the memory representations and input data, predicting the occurrence and lack thereof of optional infinitives (see e.g. Freudenthal et al. 2010). In a different vein, of interest is the combination of U-DOP (Bod 2009b) and the constructionist assumption that grammatical structures are meaningful too, which, although not evaluated on naturalistic data, shows how one can, with a single mechanism (viz. assuming all possible substructures and letting the statistics decide which are the most likely), show how words (simple form-meaning pairings), fully productive syntactic patterns, and semiproductive patterns can emerge (Beekhuizen & Bod 2014, Beekhuizen et al. 2014).

Finally, computational modeling makes the global prediction of a model visible. Global evaluation is already a central method in many computational linguistic approaches, and the methods of evaluation are often extendable to cognitive computational models. Bod (2009b), for instance, describes U-DOP, a model building on exemplar-based principles of information processing and a simple form of analogical reasoning, and shows how the analyses U-DOP makes of sentences are very much in line with how human annotators parsed the data. Moreover, Bod shows how auxiliary fronting, a parade case of an allegedly unlearnable syntactic pattern, can be learned and parsed correctly with this model. For an example of a global model that is not a cognitive model, Bannard and colleagues (2009) showed how later productions of children are more likely generated by more abstract grammars. To this end, they tested the model on the full range of data, showing that the gradual abstraction account applies across the board.

5. CONCLUSION. We already know quite a lot about the acquisition of grammar, but this knowledge is rather piecemeal: for some cases we know how one specific mechanism applies to the full array of empirical phenomena, for others how multiple mechanisms conspire to predict one phenomenon, but how multiple mechanisms interact in blindly processing the input such that the wide array of developmental phenomena can be explained is currently mostly beyond our understanding. What AP&L call the linking problem for UG-driven approaches is but the tip of a methodological iceberg: we simply do not know how to link most cognitive subsystems to each other or to the full breadth of the input data. Daunting as it may seem, we do believe that this should be an ideal for research on language acquisition, and that computational cognitive modeling provides an indispensable tool. Without the extended theorizing, more global means of evaluation, and a potential for evaluating interacting cognitive processes that apply as a whole to the input data, the bigger linking problem will remain.

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[b.f.beekhuizen@hum.leidenuniv.nl]
[rens.bod@gmail.com]
[a.verhagen@hum.leidenuniv.nl]

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