

## PERSPECTIVES

### News, somewhat exaggerated: Commentary on Ambridge, Pine, and Lieven

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Ambridge, Pine, and Lieven (AP&L) claim that the knowledge attributed to children by the proponents of UG does not account for language acquisition, bringing evidence from several domains. In this response, we take issue with their claims with respect to two domains. In the case of categories, where distributional learning plays an important role, we argue that AP&L fail to recognize recent analyses showing that abstract representations yield better quantitative models for early child data. In the case of subadjacency, we provide several empirical arguments against their claim that it can be reduced to some general discourse-pragmatic principles.\*

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Ambridge, Pine, and Lieven (2014; AP&L) state that, contrary to the claim by the proponents of UNIVERSAL GRAMMAR (UG), the innate knowledge attributed to child learners does not contribute to language acquisition. They review the domains of categories, basic morphosyntax, structure dependence, subadjacency, and binding principles and conclude that these domains present no learnability problem. UG does not help language learning, we are told. It is dead, or should be. Those across the theoretical divide might feel like Mark Twain upon reading his obituary, that the reports of this death have been exaggerated.

Debates in the behavioral sciences often take the form not of conversations, but of dogs barking up different trees. The two leading paradigms in acquisition, generative and usage-based approaches, focus respectively on representation and process. Generative or grammar-based approaches focus on the formal properties of children's language. Usage-based approaches emphasize interactions and the gradual nature of learning. Is there something to be learned across paradigms?

Let us start by acknowledging what seems most useful in AP&L's presentation: the questions it highlights. What does it mean to say that a given category is innately available to children? Can we say that a given category (say, determiners) is given by UG, if not all languages have it? How are the learned elements mapped into the semantic and formal space for each category, given the range in variation that we see? These and several other questions that their article highlights are important, by anyone's account. To address them, we concentrate on claims around two of the domains discussed in their article: syntactic categories and conditions on long-distance extraction, also known as subadjacency. As we show in our discussion, these two domains appear to be at the two extremes of the learning continuum, with categories involving much parametrization and hence requiring substantive learning, and subadjacency representing largely invariant formal universals for which there is little evidence of learned behavior.

According to AP&L, the notion of innate syntactic categories suffers from three core problems. The first concerns how distributionally extracted information gets linked to categories. The proposed mechanisms are deemed insufficient, because they do not extend to other categories beyond nouns and verbs. Second, according to AP&L, UG approaches have a data-coverage (a.k.a. empirical adequacy) problem, because some

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categories do not exist in all languages. Finally, they claim that the UG approach suffers from redundancy, since learning categories requires distributional learning (also needed to succeed at the segmentation task). Distributional learning, coupled with recognizing the semantic similarities in the given class, should be enough to yield the category. Ergo, innate categories are unnecessary: 'Learners will acquire whatever syntactic categories are present in the particular language they are learning, making use of both distributional (e.g. Mintz 2003) and semantic similarities (e.g. Pinker 1984) between category members' (p. e59).

AP&L concede that proposed approaches for the learning of syntactic categories are 'largely along the right lines' (p. e59). So, what is the problem? Their objection to UG-based accounts goes along the following lines: '[UG proponents] ... attempt to squeeze fine-grained language-specific categories, defined by distribution and semantics (and possibly also function and prosody), into a rigid framework of putative innate universal categories' (p. e60).

The reason, in their eyes, that there are presumably no proposed linking mechanisms for categories such as determiners and complementizers is that 'there ARE no good candidates for innate universal syntactic categories other than NOUN' (p. e57). Let us first contextualize this problem. AP&L overestimate the authority of the claim by authors such as Evans and Levinson (2009) that there are 'no viable candidates for crosslinguistic syntactic categories' (p. e57), a topic for which there is no consensus.<sup>1</sup> The essence of the UG-based approach is to identify universal and parameterized components, and attribute a different learnability status to them. The method is rather simple: (i) identify general and specific properties of language, (ii) assume that all language variation is learned, and (iii) assume that learning has to be circumscribed to observable properties of utterances. Any other property results from innate characteristics of the learner, by default. Where proponents of universals differ from their critics is that they hold variation to occur within a limited, rather than unbounded, formal space. As some see it, this debate is not about whether there are any universals, but about what is the right degree of abstraction in language analysis (Baker 2009).

While making general claims about a data-coverage problem, AP&L make no reference to the learnability-based literature specifically devoted to the typology of nominal reference (Chierchia 1998, Guasti et al. 2008). This literature explicitly explores a parameter that separates three types of languages: languages with no determiners and generalized bare nouns, languages with generalized determiners and restricted bare nouns, and mixed languages like English.

Since languages vary in the presence/absence of a determiner category in their functional inventory, what is the UG claim about the abstract category 'determiner'? In the words of Valian (2009), children 'do not construct the category'. Rather, they map the determiner words learned into the abstract category. The UG claim is not that children automatically assume an English-like category D, with the semantic and formal distribution of English determiners. The claim is that children map into abstract representations all results from their distributional learning (Marcus et al. 1999). In facing input, children may fail to segment a given determiner-noun sequence, and, in fact, they often do (e.g. Parent: *Who wants some mango for dessert?* Child: *What's a semmango?* (Bernstein Ratner 1996)). Once segmentation succeeds so that a distributional class is identified, it will be treated as an abstract category (of the right type, and made up of the

<sup>1</sup> A review of responses to Evans & Levinson 2009 suggests that the argument against universals does not represent a majority view in the field (Harbour 2009, Pesetsky 2009, Matthewson 2013, Legate et al. 2014).

right features). There is an active discussion, and a body of data, bearing precisely on this issue.

Degree of syntactic diversity in early word combinations is considered the crucial data for comparing grammar-based and usage-based approaches (Ninio 2011). Some in the field see children's initial production as primarily made up of 'frozen' idiomatic phrases, and conclude that children do not possess abstract syntax (Tomasello 2000). In the case of the determiner category, Pine and Lieven (1997) point out that children acquiring English seem to have little overlap in the lexical items with which they use individual determiners. According to the authors, this shows that young children do not possess an abstract representation of the category determiner. Instead, the argument goes, children rely on lexically narrow representations. These are limited-scope formulae that are neither abstract nor associated with a productive combinatorial system.

This interpretation is not without criticism. The initial finding of low lexical overlap across determiners is, according to Valian, Solt, and Stewart (2009), a sampling artifact. These authors point out that children show the same degree of variation in the use of determiners as their mothers, relative to their language production. Yang (2013) has proposed that statistical modeling can test the presence/absence of category-based grammatical rules. An abstract rule for the generation of noun phrases ( $NP = D + N$ ) makes reference to two categories of words (determiners and nouns), independently sampled from the sets of determiners and nouns. The probability of finding a specific combination of a given determiner and a given noun is the product of the marginal probabilities of the two. Frequencies for individual words are predicted on the basis of Zipf's law, which states that the frequency of a word is inversely proportional to its frequency rank. A usage-based model would have to rely on sampling from a set of jointly formed (lexically stored)  $D + N$  combinations, extracted from the input. The fit of these two models is examined against CHILDES data for nine samples of young children learning American English at the two-word stage. The statistical comparison reveals that the lexically linked (single-bin) combinations severely underpredict the diversity of combinations found in the corpora. The dual-category model is a far better predictor of the data. Furthermore, the analysis of samples of early child speech shows considerable diversity when compared to adult corpora. In Yang's words, 'The previous literature is mistaken to interpret the value of combinatorial diversity as a reflection of grammatical productivity' (2013:6325). The distribution of determiners at the first stage of language production seems best explained on the basis of separate formal categories, rather than on lexically stored combinations of  $D + N$  strings. This, of course, is not proof that innate categories exist. What the evidence shows is that we do not find a point in development at which children behave as if they did not have them. This is an important result. One cannot but wonder why AP&L fail to address it in their piece, or why they a priori exclude comparisons between approaches (p. e53).

So what is in the category? Here we acknowledge that the landscape of the field has changed in recent years. Categories are a manifestation of properties that cluster around a distributionally recognizable class. Current thinking within the MINIMALIST PROGRAM holds that syntactic categories are made of combinations of features. The distributional objects (lexical items) are not themselves universal, but their ingredients are.

The assumption that lexical items are unstructured sets of features, which are just memorized by the language learner, raises a further question: how does the learner decide on the features? Assume the learner has a conceptual space that she must ascertain the grammar of (say the space of pronouns). The conceptual structure of human thought provides her with a range of possible analyses, in terms of semantically motivated notions such as number, participant in the speech act, etc. Some subset of these will be available to reify grammatically as a set of features. (Adger 2006:507)

Viewed this way, a UG theory of features is but a theory of which ‘semantic [and formal] similarities’ children will attend to when mapping function words to a category. To the extent that there is variation, a specific grammar may contain only a subset of all available features. In morphosyntax, as in phonology, children learn by strengthening a subset of predetermined contrasts supported by the input, to the detriment of other potential contrasts that are not. Features are what enable children to filter the input in a meaningful way.

A consequence of this approach is a set of potential predictions about the types of mapping errors made by children. Children learning English exhibit overextensions of definite determiners in comprehension. In a context where Bert’s necklace has been discussed, and Ernie puts on a different necklace, children but not adults interpret a definite determiner in statements such as *Look at Ernie. Did Ernie wear **the** necklace?* to refer to the new (not yet mentioned) necklace. Matthewson, Bryant, and Roeper (2001) suggest that this form of overextension is congruent with the semantics of determiners in languages where the determiner system does not reference familiarity in the common ground of discourse. Other studies, such as Ramos 1999, Baauw 2000, Pérez-Leroux, Munn, Schmitt, & Delrish 2004, Pérez-Leroux, Schmitt, & Munn 2004, have shown overextensions of definites in generic contexts (*The zebras have stripes* = ‘zebras’) and inalienable possession (*the boy lifted the ear* = ‘his ear’). These overextensions parallel uses of the definites in the Romance languages. In our view, an approach that can yield generalizations encompassing developmental errors and typological variation is clearly to be preferred to one that does not.

Seen this way, solutions to the linking problem of categories must be sought within a theory of formal features capable of covering the crosslinguistic facts and capable of supporting the development of mapping hypotheses. The alternative is reliance on ad hoc proposals of what counts as relevant for the learner, or on some unspecified semantic generalizations. Determiners and other functional words are part of what Gleitman and colleagues call ‘hard words’ (Gleitman et al. 2005). These are verbs and other relational terms whose semantic mapping cannot be solved with the simple strategies of linking label to object, or even label to situation. Instead, relational terms require strategies that link sets of utterances to situations. This is, in a nutshell, the syntactic bootstrapping hypothesis (Gillette et al. 1999, Gleitman et al. 2005). In the case of determiners, a theory of formal features includes anaphoric reference, which constitutes the basis for the core meaning of the indefinite/definite determiner contrast in languages like English. Sneed German (2008) has shown that a simple algorithm tracking previous discourse mention of a noun will serve for bootstrapping the basic determiner meanings (definite/indefinite). Augmenting the algorithm with a basic lexical distinction (stage-level/individual-level predicates) and Diesing’s MAPPING HYPOTHESIS (Diesing 1992) is sufficient to solve the much more complex mapping problem of which types of noun phrases, and in which syntactic contexts, can receive generic reference (Gelman 2003, Gelman & Raman 2003).

As a preliminary summary, the problem of universality and variation is an important one, and AP&L are right to highlight it. In focusing on determiners, we can show that the existing UG acquisition literature contains explicit proposals that (i) cover the range of existing languages, (ii) work out a mapping algorithm for determiners, and (iii) provide better predictions for the earliest output of distributional learning. To speak to this problem, however, we find that we have strayed beyond the boundaries set by AP&L by discussing both current thinking about features in minimalism and competing approaches in acquisition.

We are still left with the problem of redundancy. Does a learner need a theory of potential features, or is it enough simply to posit a Saussurean instinct for detecting contrasts, and for structuring into a combinatorial system? Even to those committed to abstraction in grammar, it is now an open question whether the content of features is part of a larger lexical inventory as in Chomsky 2001, or part of a third factor, external to language (Chomsky 2005, O'Grady 2012). In either case, raw experience will not do. We need either a well-developed theory of what (conceptual) components of experience children are predisposed to attend to, or a theory of linguistic features.

In §5, AP&L claim that subadjacency can be accounted for using some general discourse-pragmatic principles, so that an innate specification of island constraints is redundant. According to them, 'island constraints can be explained by discourse-pragmatic principles that apply to all sentence types, and hence that will have to be learned anyway' (p. e72).

Their critique of the learnability problem has two angles. One is to suggest that verb-complement islands are to a certain extent a gradable lexical phenomenon and must be learned. The other is to say that the route to learning subadjacency domains follows discourse and information structure. Surprisingly absent from their exposition is the crosslinguistic literature on the acquisition of the WH-island, which deals with familiar examples such as 1 and 2, taken from work by de Villiers, Roeper, and Vainikka (1990). The former is ambiguous: *when* may apply to the embedded clause (the 'long distance' interpretation of the question) or to the matrix (the 'short distance' interpretation). The sentence involving a WH-island in 2, almost identical to 1, is not ambiguous. It can only mean *when-tell*.

- (1) When did the boy tell his father \_\_ that he hurt himself \_\_ ?
- (2) When did the boy tell his father how he hurt himself?

The question of categories is crucial here since WH-islands depend on the differentiation of complementizers and WH-pronouns: both are functional elements that occur in the left periphery of clauses, but have different meanings. In an argument for connectionist approaches, Ellis (1996) points out that cluster analysis produces a clear separation of word classes in general but classes WH-words and complementizers together (Ellis 1996:365). Since the distributional profiles of WH-pronouns and complementizers groups them together, it is not clear how the lexical learning route will address the problem of WH-islands. Children must rely on the fact that WH-pronouns and complementizers have different semantic properties in order to treat them differently in extraction contexts.

AP&L's approach to learning about islands takes a different route. According to them, complex NPs, subjects, relative clauses, and adjuncts are islands because they lie outside what they call the 'potential focus domain' of the sentence (p. e72). Learners are thought to be 'sensitive to the pragmatic principle that one cannot extract elements of an utterance that are not asserted, but constitute background information' (p. e68). The pivot point of their account lies on this notion of potential focus domain, which, coupled with an approach to learning them, should take care of islands.

AP&L use a simple declarative sentence like *Bill bought a book* to introduce their notion of focus. In this sentence, they state that the topic is the subject and the potential focus domain is the predicate. They argue that while the 'actual focus' under the default interpretation is equal to the whole predicate, with the use of focal stress, actual focus can fall anywhere within the predicate. In 3, following AP&L's conventions, bold shows **potential focus domain** and underlining indicates actual focus.

- (3) a. Bill **bought a book**. (He didn't run a marathon.)  
 b. Bill **bought a book**. (He didn't steal or borrow one.)  
 c. Bill **bought a book**. (He didn't buy a newspaper.)

AP&L's conceptualization of notions such as potential focus domain is problematic. Focus is often defined as the nonpresupposed part of the sentence, where the presupposed part of the sentence is determined by the speaker's and hearer's assumptions at the point of the utterance (see e.g. Chomsky 1971, 1976, Jackendoff 1972). Typically, WH-questions (known as context questions) are used as a shorthand way of showing the context in which the particular sentence is uttered. Under this view, any part of the sentence has the potential to be focused. The focus of the sentence (or what AP&L refer to as actual focus) is the part that provides the answer to the context question. Hence, the context questions corresponding to the sentences in 3 are given in 4, in the same order.

- (4) a. What did Bill do?  
 b. What did Bill do to a book?  
 c. What did Bill buy?

AP&L assert that one cannot place the focus of the question in the part of the sentence that is presupposed. This much is correct, in the sense that you cannot focus the presupposed part of the sentence. In other words, using the terminology introduced by the authors, a mismatch between the focus of the sentence and the context question will result in ungrammaticality (or infelicity), as shown in 5.

- (5) a. Q: What did Bill buy?  
 A: \*Bill bought a book.  
 b. Q: Who bought a book?  
 A: \*Bill bought a book.

Note that the ungrammaticality judgments in 5 are the result of a mismatch between the ACTUAL focus and the context, and not a predetermined notion of potential focus domain. According to AP&L, islands constitute inherently presupposed domains, and it is not possible to focus inside them. Their analysis is shown for complex NPs in 6, where their potential focus domain is shown in bold, and the island in angled brackets (p. e74).

- (6) Bill **heard the rumor** <that Sue stole the files>.

They propose a negation-based test for determining what is a potential focus domain. As per that test, you can deny the root clause, but not an embedded clause (p. e74).

- (7) Bill **bought a book**. → No, he didn't.  
 (8) Bill **heard the rumor** <that Sue stole the files>. → No, he didn't/\*she didn't.  
 (9) Bill **heard** <that Sue stole the files>. → No, he didn't/\*she didn't.

This approach classifies complex NPs (which are islands), as in 8, alongside with sentential complements (which are not), as in 9. Their response to this empirical problem is that intuitions of extractability (from sentential complements) are gradable, subject to lexical variation, and that the intuitions on the negation test apparently correlate with the intuitions on WH-extraction. There is no indication of how lexical variation would account for the case of WH-islands, which can appear with the same lexical verb as non-islands. There is an additional problem for the lexical solution, to which we return later.

Considering the case of complex NPs, we can show that AP&L's claims about focus and islands are misguided on several grounds. First, the complex NP can be part of the focus of the sentence, as shown in 10.

- (10) Q: What did Bill hear?  
 A: Bill heard the rumor that Sue stole the files.

More importantly, you can in fact contrastively focus on elements within the complex NP, as shown in 11, with focus underlined. If the explanation for the island constraints is the inability to focus elements within these islands, as claimed by AP&L, these sentences should be ungrammatical.

- (11) a. Bill heard the rumor that Sue stole the files.  
 b. Bill heard the rumor that Sue stole the files.  
 c. Bill heard the rumor that Sue stole the files.

Here we note that their discussion conflates two distinct types of focus (É. Kiss 1998, Büring 2013). Their point of departure is information focus, also known as question focus. However, their test of potential focus domain is negation, which involves contrastive focus. An element inside an island can be contrastively focused, so that 11c is a perfect rebuttal for a situation such as 12, where it is true that Bill heard a rumor about Sue stealing something, but not true that the thing stolen was the files.

(12) Q: Did Bill hear the rumor that Sue stole the donations?

A: No. He heard a rumor that Sue stole the files.

In fact, AP&L's proposed negation test does not seem to tap conditions on focus, but conditions on ellipsis: 'There is a simple independent test for whether a particular constituent falls within the *potential focus domain*: whether it can be denied (without recasting the entire phrase)' (p. e74). Why is recasting a problem at all? In their account, this is a stipulation needed to make their test differentiate between matrix and embedded clauses. The analysis in 13–14 shows that the problem is not focus, but that ellipsis in general preserves the root, and the island status of the embedded clause is irrelevant. Example 15 illustrates a perfectly viable response for the situation in 12. The ellipsis is ambiguous; only the context or further expansion can make it clear that the focus inside the elided component is the direct object of the embedded clause. So, contrastive focus can apply inside a complex NP island, whereas an overt movement configuration is blocked.

(13) Sentential complement (nonisland)

Q: Did Bill tell you that Mary stole the files?

A: No, he didn't ~~tell me that Mary stole the files~~.

\*No, ~~he told me that~~ she didn't?

(14) Complex NP (island)

Q: Did Bill hear the rumor that Sue stole the files?

A: No, he didn't ~~hear the rumor that Sue stole the files~~.

\*No, ~~he heard the rumor that~~ she didn't?

(15) Q: Did Bill hear the rumor that Sue stole the donations?

A: No, he didn't ~~hear the rumor that Sue stole the donations~~. He heard a rumor that Sue stole the files.

The source of the ungrammaticality of extracting from inside a complex NP cannot be focus but rather a ban on extraction from these islands, which is what subjacency (or its more modern counterparts such as barriers or phases) is trying to account for.

To show that the problem is not with focus or even WH-questions but rather constraints on extraction, we can also consider multiple WH-questions in English. Here, we are really in the domain of information focus, but as we show below, the problems persist. As we know, in English multiple WH-questions, only one WH-phrase fronts and the other one(s) stay(s) in situ. Imagine a context in which a number of individuals heard rumors that a number of other individuals stole the files, where each individual heard a particular person stole the files. In this context the following questions and pair-list an-

swers are grammatical. Crucially, the in-situ WH-words, while still constituting information focus, do not lead to ungrammaticality.

(16) A: Who heard a rumor that who stole the files?

B: Bill heard a rumor that Sue stole the files.

John heard a rumor that Mary stole the files.

Jane heard a rumor that Tom stole the files.

Such cases show clearly that the problem is not pragmatic. In fact, it is not difficult to construct a pragmatically sound context for the ungrammatical question that violates the complex-NP island. Yet, the resulting question is impossible to formulate (compare grammatical B with ungrammatical B').

(17) A: Bill heard a rumor that Sue stole something.

B: Really? What?

B': \*What did Bill hear a rumor that Sue stole?

The objections raised above can easily be extended to the other cases of islands AP&L present. To summarize, even before we evaluate whether the alternative AP&L have put forth can account for THE LEARNING OF island phenomena, we have concluded that their analysis suffers from various conceptual and empirical problems. The evidence, however, seems to support a structural explanation for these facts. But it should be added that it is not enough to say that (information) focus and WH-islands are similar learning domains. What is needed is to specify a learning strategy that can distinguish islands from nonisland domains.

AP&L do not actually articulate such a learning strategy. Their contribution is limited to pointing out that children are highly sensitive to information focus. They assume that, from this, it follows that children should be very good at learning about potential extraction domains. We agree with their assessment of the growing literature on children's sensitivity to information structure. This literature has yielded excellent evidence that children are sensitive from the outset to information (question) focus in the root. Clear data has been gathered from the analysis of spontaneous speech (Allen 2000, Serfati 2005) and from careful experimentation (de Cat 2009, Salomo et al. 2010). However, this literature has nothing to say yet about how children deal with information structure in the context of complex clauses, which is where subadjacency is relevant.

There is another dimension of development that any theory of learning should attend to. The experimental literature on WH-islands suggests substantive uniformity from early stages of development. Children show very low rates of errors with islands, at the same time that they demonstrate willingness to give long-distance interpretations to comparable nonislands. Between the ages of three and six, children give long-distance interpretations to questions like 1 (repeated below) roughly 50% of the time, but only around 6% of the time for questions like 2 (de Villiers et al. 1990, Pérez-Leroux 1993). This suggests a surprising degree of uniformity in intuitions about extractability from island domains.

(1) When did the boy tell his father \_\_\_ that he hurt himself \_\_\_?

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

An independent challenge for any theory of language acquisition is to describe which grammatical properties are easily or automatically accessible to children and which are not. A joke used to circulate in linguistics departments: 'Question: How many generative syntacticians does it take to change a lightbulb? Answer: One. He goes to the closet, takes out a lightbulb, and everything else happens from principles.' As a theory of learning, UG might sound like a joke. But in the case of islands, it does not fare too badly. To the ex-



tent that we find clear uniformity in language behavior at the outset of development, in domains where the input to model the behavior is opaque and/or infrequent, we are right to suspect that the story might involve something other than learning.

We also need to examine when children do not behave uniformly. De Villiers (2005) notes that children seem to violate the factive island. This island is derived from an underlying complex NP syntax for what on the surface looks like a simple complex complement, on the basis of the meaning of the given verbs.

(18) Bill knows ~~the fact~~ <that Sue stole the files>.

Only when children understand the meaning of *know* will they attribute the more complex structure to these utterances. Lexical learning is sensitive to experience, so we should expect initial variability for such types, and we find it.

In the developmental field, children are said to only engage in communication when they demonstrate intersecondary subjectivity, that is, joint attention behavior that takes place between two agents in reference to a separate object. By ignoring the intricate nature of linguistic representation, as AP&L do, or by trivializing learning, as sometimes is done in this corner of the yard, we are all refusing to meet this important developmental milestone. This is a good time for discussing general or specific nativism, but we set as a prerequisite condition that the discussion should take place within viable frameworks for describing language. It is not useful to simulate a conversation on language acquisition when we do not share the premises of what language is. Perhaps the field is not yet ready for such a conversation. Maturation works, so, when this conversation finally happens, it will be an exciting one. Perhaps it will lead to models that can tell us when we will identify early learning success, and when to anticipate a protracted learning curve. Perhaps it will lead to models that can describe all that is possible in language, and that can predict what will be more likely. While we are at it, why not hope for models that can tease apart the contribution of probabilistic information from that of discourse interactions? We close this commentary with the tantalizing words of Susan Oyama: ‘Nature and nurture are not alternative sources of development, competing explanations, or even complementary ones, but product and process of development’ (Oyama 2013).

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