

## AUTHOR'S RESPONSE

# Explaining bilingual learning outcomes in terms of exposure and input

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I had several goals in writing my keynote “Exposure and input in bilingual development”. The first was to emphasize that there are two components to the study of environmental effects on language learning. The first is the stuff ‘out there’ (EXPOSURE) that we want to observe and count and whose effects we want to assess; the second is the internal, mentally represented stuff (my INPUT) that is logically related to a particular learning problem. Both exposure and input are indissociable from assumptions about what language acquisition mechanisms do and the nature of linguistic cognition. Accordingly, for example, a decision to count ‘words’ in child-directed speech (CDS) or via a parental questionnaire is not an innocent one. Not only can one find radically different views on what a ‘word’ is (Krause, Bosch & Clahsen, 2015), one can find work that questions the need to postulate such a unit at all (see discussion in MacWhinney, 2000). It follows that adopting a clear position, about which abstract mentally represented elements are crucial CUES to learning some phenomenon, is an essential step in deciding what to count in CDS.

A second goal was to ask: are authors’ conclusions in particular studies, as stated, valid? This required, of course, that I consider research methods. De Houwer (De Houwer) and Pérez-Leroux (Pérez-Leroux) concur in my scepticism about certain studies; they add additional methodological criticisms of their own. Given the readership of *Bilingualism: Language & Cognition*, I adopted a cognitive perspective. The fact that researchers might have other perspectives in mind related to educational policy issues, and/or parental counselling in countries where the monolingual habitus is the dominant ideology (Gathercole; Weisleder), is not a lesson I needed to be taught; it was simply beside the point of my keynote to discuss it.

A third goal was to plead for more descriptive research on CDS. De Houwer (De Houwer) and Mougeon and Rehner (Mougeon & Rehner) agree

with me that good descriptive research on language use in bilingual families and language classrooms is needed, in particular, to provide data on languages other than English and social contexts in places other than Canada, the USA or the United Kingdom. I emphasized the complexity of studying exposure and input from a purely cognitive perspective, a point made equally by Gathercole (Gathercole), Grüter (Grüter) and MacWhinney (MacWhinney). I also expressed doubts that this perspective will suffice. Mougeon and Beniak’s (1991) large-scale study of minority language French speakers’ linguistic knowledge and language use has convinced me that our understanding of exposure and input effects on language learning will be enhanced if we can access ethnographic, sociolinguistic and/or social psychological research that examines attitudes and values about the target ‘languages’ both in young children and in families. I pleaded, in particular, for research on the understanding of young bilinguals as to what language labels like ‘English’ or ‘Dutch’ mean to them.

In what follows, I will try to deal with misunderstandings.

## What it means to tell a causal story about exposure to environment factors

If we claim that exposure is having an effect on language learning outcomes we are attempting to tell a causal story about it. Pinker (1984, pp. 28–31) emphasizes that to claim that specific linguistic stimuli are causing particular learning outcomes and are not merely coincident with learner behaviours, we have to accomplish three different tasks: (i) show that the stimuli are indeed present in the child’s environment (in CDS or speech the child might overhear); (ii) demonstrate that such stimuli have been perceived and processed or used by the child, meaning that we can show that the child is indeed sensitive to the stimuli on some behavioural task; and (iii) show

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that the stimuli make an essential difference in changing the learner's linguistic cognition. Each of these steps is essential to making the argument. If one is missing, then conclusions that particular environmental factors are causing particular learning outcomes are premature, at best, or invalid, at worst. I fear the complexity of the causal story-telling has not been fully appreciated.

Paradis (Paradis) reviews studies using parental reports that show consistent correlations between the reports and children's subsequent knowledge. The correlations are reliable; the parental questionnaires are measuring something. Are we to conclude that the parental language the children are exposed to is causing the patterns in the child data? Paradis apparently believes we should. However, correlational studies do not license the inference that specific properties of the child's environment cause the learning outcomes to be what they are. The point of my keynote was to ask: are the design and methods that have been used appropriate to drawing causal conclusions about exposure? Correlational studies of vocabulary knowledge using parental questionnaires do address Pinker's point (i). Paradis' (Paradis) main point appears to be that correlational studies address Pinker's point (iii); this is just wrong.

What about parental reports in general? Paradis (Paradis) is correct to note the reliable correlations found. I have no objections per se to anyone using parental reports in order to gather data about a child's vocabulary knowledge (Pinker's point (ii)), any more than I would object to diary studies, child language elicitation instruments, or any other commonly used instrument. Language acquisition researchers are always faced with difficult choices regarding the reliability and feasibility of their data-gathering instruments. See De Houwer (De Houwer) and Pérez-Leroux (Pérez-Leroux). The real question is: what do these reports tell us? If I have understood correctly, they tell us that a child who knows both *tooth* and *teeth* has been exposed to those forms. Is it conceivable that this finding could not be true? There certainly are more interesting questions to explore. We know that children need to be exposed to *wug* to use that form but they do not need to be exposed to *wugs* to use the plural. It is not obvious to me that parental reports will reveal such a finding. Similarly, it would not be a trivial outcome if we were to discover that a bilingual child needs exposure in each language to every verb expressing motion concepts to cognize that the verbs in question are, say, intransitive and co-occur with prepositional or post-positional phrases (PPs) expressing place, path or direction concepts. This would demonstrate a particular form of conservatism with respect to input. It would also be a remarkable finding if this kind of knowledge were found to be acquired in one language and 'carries over', without explicit exposure being necessary, to the child's acquisition of motion verbs and spatial PPs in the other

language. Again, I fail to see how parental reports can address such questions.

I explicitly acknowledged that we might expect certain aspects of word learning to reflect frequency of exposure. Given the claim I made that exposure and input studies need to explicitly consider the relationship between language processing and language acquisition, the finding of a correlation between amounts of exposure to each of the bilingual's languages and speed of lexical processing (Hurtado, Grüter, Marchman & Fernald, 2014) is reassuring. On my limited understanding of models of lexical processing, I believe that they predict that as a learner processes a word, the activation threshold levels of the word will change. Alternatively, there might be competition among the lexical resources of the child (MacWhinney). The correlation begs for further research that strictly controls amount of exposure. At precisely this point, researchers will need to be quite clear about what is meant by a 'word' and 'frequency of exposure' and adopt appropriate methods that will allow them to accurately measure the consequences of different frequencies of exposure.

Bernardini (Bernardini) asks if simultaneous bilinguals with different exposure profiles ('strong' versus 'weak' languages) have different kinds of knowledge. She cites recent studies that are perfectly fine examples of research showing that bilinguals do not constitute a single population (one of the points I made in my keynote). However, none of these studies licenses the conclusion that the differences observed are caused by differences in exposure patterns, which is precisely the inference she appears to want to draw.

Weisleder (Weisleder) writes about the various kinds of prejudices against bilingualism that can serve as a backdrop to research that serves to educate parents, policy-makers and the general public (see also Gathercole). I need no lessons on such matters. Even in a country with an official policy of multiculturalism and widespread support for individual bilingualism, many parents fear that it comes with a cost. See my keynote for specific examples, whose import was apparently not understood.

### **Formalization is a tool; it is not a theory of language**

I was quite surprised to discover that by framing my discussion in learnability terms, I was thought to be advancing a "generative linguistic perspective" (Arnorn-Lotem; Grüter; MacWhinney). While it might be true that various models of generative grammar are committed to formalization, it is certainly not true that all formalized theories of grammar are generative.

Formalization is a tool that ought to be part of every psycholinguist's toolkit. It helps to provide clarity about the cognitive distinctions we hypothesize

that learners must acquire. Once learning problems are formally defined, we can better grasp the relationship between acquisition mechanisms and mental representations. Across a large body of research, MacWhinney has explored with remarkable clarity the complex relations between language acquisition, language processing and assumptions about linguistic cognition. He has an explicitly stated, coherent vision of what learning mechanisms do, of what kinds of mental representations they construct, and what kinds of input they need, supported by formalization and computational implementations (MacWhinney, 1987, 2000, 2004; MacWhinney & Bates, 1989; MacWhinney & Leinbach, 1991; McWhinney, Leinbach, Taraban & McDonald, 1989). Of course, so does Pinker (1984, 1989, 1999). The visions are quite different and this has important consequences for conducting exposure and input research. For example, we will not find ‘empty categories’ in CDS so we cannot investigate the effects of frequency of empty categories in CDS. I assume this is just one reason why MacWhinney prefers a vision of grammar in which empty categories do not exist.

### **On abstract versus concrete views of linguistic cognition**

The speech signal and video-recordings of a learning context provide us with necessary evidence of what learners have been exposed to. Speech consists of bursts of noise and bits of silence that we can measure out against temporal dimensions. Knowledge of language is, however, abstract. Accordingly, if we want to talk about anything else (phonemes, morphemes, words, gender, the passive construction, binding relations, parasitic gaps...), we have to impute cognitive reality to these constructs and hypothesize that speech and language processors can compute them somehow out of the bursts of noise and bits of silence. MacWhinney (MacWhinney) correctly points out that people differ in their views of how abstract language is; all the more reason for clarity on this point in individual studies. I happen to believe that we need a slightly more abstract theory of language than the one MacWhinney works with, but I am not in the business of telling others what epistemological and ontological commitments they should adopt (contra Grüter’s demand that I should, cf. Grüter). For the record, my own theoretical commitments fall somewhere in-between MacWhinney and Pérez-Leroux but my views on this matter were irrelevant to the goals of my keynote.

Nonetheless, Armon-Lotem’s (Armon-Lotem) report that some aspects of syntactic processing are less susceptible to length of exposure and age of onset than lexical measures (Thordardottir & Brandeker, 2013)

is welcome. Contra MacWhinney (MacWhinney) and usage-based accounts of linguistic cognition, she argues that not all aspects of syntactic processing can be reduced to lexical knowledge and the distribution of words in sentences (see also Pérez-Leroux & Kahnemuyipour, 2014). Accordingly, just those aspects of syntactic processing ought not to exhibit the incremental and exposure-dependent kind of learning typical of word learning. This is the kind of nuanced prediction that arises when one starts with an explicit picture of what a particular learning problem consists of. The problem has been: how can we actually differentiate the two types of knowledge? Research on LITMUS-SRep (Marinis & Arnom-Lotem, 2015) is quite promising in that it appears to be able to investigate effects of syntactic processing without also investigating lexical access. If not all syntactic knowledge can be attributed to knowledge of words or morphemes, usage-based explanations of grammar acquisition will have to be adjusted. At the very least, MacWhinney (MacWhinney) and Arnom-Lotem (Arnom-Lotem) show the need to move discussion away from traditional – and misleading – views of language as consisting of ‘vocabulary’ versus ‘grammar’. See also Grüter (Grüter).

### **Studying individual differences and variation**

Pérez-Leroux (Pérez-Leroux) correctly points out that I did not emphasize the role of individual variation and its internal causes in my keynote. My focus was on environmental effects. Internal factors are a topic of central concern to many developmental psychologists, particularly those who investigate developmental delays (see also, Armon-Lotem). My view is that this adds yet another layer of complexity to bilingual research on exposure and input. Similarly, appealing to competition to explain individual variation or why frequency sometimes matters and sometimes does not (MacWhinney) may ultimately be the right thing to do, but it makes it harder (not easier) to study exposure effects on learning outcomes.

Mougeon and his collaborators have conducted rich studies of learners learning French under radically different conditions of exposure. Their focus, as sociolinguists, is on variation; that is to say, on the optional use of forms that are grammatical in the target language but differentially used by speakers with different sociolinguistic profiles. Mougeon and Beniak (1991) studied learning outcomes in bilinguals who are much older than is typical of much bilingual acquisition research. This is helpful, and I hope drawing this large-scale and meticulously conducted study to the attention of bilingualism researchers will have served a useful purpose. Mougeon and Rehner (Mougeon & Rehner) bring us up to date on research comparing

English-speaking children who learn French in immersion schools who were the same age as their original population of Franco-Ontarian adolescents and young adults learning French in university classes. This allows for a contrast between classrooms where French is the vehicle through which other content is taught and those where grammar, vocabulary and pronunciation are the explicit focus of instruction. They document that some students have exposure to non-standard variants in the community (meeting Pinker's point (i)). These students exhibit knowledge of those variants (meeting Pinker's point (ii)). It looks as if the learners may have more exposure to different forms used by teachers in the classroom (if true, this would address Pinker's point (iii)). Mougeon and Rehner (Mougeon & Rehner) discuss how frequency effects in classroom discourse are tempered by differences in structural complexity, pressure from the bilingual's other language, and the learner's belief systems. Nothing prevents us from modelling attitudinal factors through stochastic learning models like those mentioned by MacWhinney (MacWhinney) but presumably the interaction of frequency counts and beliefs will not be easy to conceptualize; let alone measure, and explain.

### Conclusion

The study of exposure and input is not a peripheral topic in language acquisition. On the contrary, it is indissociable from our assumptions about perception, inference, speech and language processing, memory, and language acquisition mechanisms. We know that learning language-specific properties requires exposure to relevant speech in learning environments that support interpretation and social interaction. That is not news. The interesting questions turn on how the environment aids or hinders specific learning outcomes. Finding answers will not be easy. Bilingual learning contexts are diverse and complex, and specific learning outcomes are not inevitable. Moreover, we ought to anticipate cross-linguistic effects for at least some acquisition phenomena. There is a great need for more descriptive research that will tell us how language use varies in different kinds of bilingual families and classroom contexts. However, to know what to observe and count, we also need to unpack the complexity of specific learning problems. This is essential as well to get a handle on how different variables may compete in processing and learning since we already know that simple stories about frequency of exposure are not going to be explanatory. Finally, in addition to learning from the rich literatures on monolingual language exposure and input, sociolinguistic and social psychological research on bilingual attitudes and social identity already suggest interesting avenues of exploration for those of us interested in exposure and input.

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