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Synthesizing commentaries and responses

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Bimodal bilingualism is at the same time a fascinatingly different phenomenon from unimodal bilingualism and just more of the same. When bilinguals produce and comprehend a spoken language and a sign language, they are producing and comprehending languages, and like other bilinguals, they must be using mental faculties that contain information about each language, maintaining them separately in appropriate contexts, yet keeping each language 'on' even when it is not being used, and intermingling aspects of the two languages. In this respect, bimodal bilinguals are no different from unimodal bilinguals. Yet, bimodals have a unique potential to combine their languages in a new way, since the articulatory constraints of a spoken language are largely separate from those of a sign language. Understanding the consequences of this difference can, we think, make a valuable contribution towards furthering our understanding of the mental faculties involved in bilingual development and interaction.

In our target article, we presented the Language Synthesis model, following from and building on our previous work (Koulidobrova, 2012, 2016; Lillo-Martin et al., 2012; Quadros et al., to appear; and other works). We have been thinking about and developing this model for some time, but it is still in its early stages, still more of an ideal than a conclusion. We are continuing to test and refine this model, and if our work inspires others to test and refine it – or even to reject it as inadequate – we will be very glad. We are grateful to the commentators for providing much food for thought, including possible challenges to our current assumptions, and an opportunity here to clarify some points.

In this response, we will amplify our discussion around three major areas raised by the commentaries. First, we will expand on the overall current goals of our proposal, and touch on how Language Synthesis might interact with other aspects of bilinguals' linguistic lives. Second, we will discuss aspects of the data we have considered in developing this model, and other potentially relevant data.

Finally, we will discuss in more detail some of the proposed pieces of the model and their theoretical implications.

1. Modest goals

Language is used by people in infinite ways and contexts. Contexts, speaker preferences and experiences, goals and pressures, as well as many other factors all influence the ways that language is used. Knowing this, we still hold fast to the idea that it is useful to study linguistic *competence*, an abstract conception of the grammatical rules that generate the sentences that are used in different contexts and under different situations. Focusing on competence overlooks multiple relevant factors that are involved in the actual production of any particular linguistic form. Yet, even though it is magnificently oversimplified, understanding competence in detail is still immensely difficult. We have chosen to start by grappling with an architecture for representing the competence of an ‘ideal’ bimodal bilingual. We are trying to develop a set of procedures for generating an abstract representation of a potential utterance. Certainly, it is insufficient to explain all – or even much – of what an actual person might do. Yet, we hope this is a step toward a fuller understanding.

In this connection, we acknowledge that our proposal does not directly take into consideration factors that might affect the particular output of different (bimodal) bilinguals, among which a prominent one may be language dominance. Dominance itself is of course a complex concept (see, for example, Silva-Corvalan & Treffers-Daller, 2016), but we are writing here about having grammatical rules more or less like the target language, as well as different degrees of proficiency in implementing these rules. Different language dominance patterns will play a role in the particular types of output patterns a person produces, such that whole classes of potential output (according to the model) might never occur for a person with one dominance pattern versus another.

For example, as pointed out by **van den Bogaerde and Hulk**, the languages of a bilingual are rarely in balance, as their input changes over time and their demands also change. We recognize that the children (and adults) we study are not static in this regard, and one characterization that captures part of this reality is that their sign language is a heritage language, used at home and with family but not in the wider (hearing) community. We are starting to incorporate this characterization into our research and hope that it will help our very abstract, competence-based approach to take into consideration some of the aspects of real life (Chen Pichler et al., in press; Palmer, 2015; Quadros et al., 2016a; Reynolds, 2016a; Reynolds & Palmer, 2014).

Likewise, **van Hell**'s charge that our approach assumes a symmetrical contribution of two languages is just only insofar as we *permit* symmetrical contribution, but the model can equally generate structures that have little or no influence from Language_x on Language_y. We do not here attempt to account for when, or why influence will or will not happen, only where (structurally) it *could* happen.

In this way, as **Putnam, Legendre and Smolensky** point out, we are in danger of both over-generation and under-generation. We expect that linguistic constraints will rule out some examples of synthesis (Lillo-Martin et al., in prep), but also rely on the "traditional [if] increasingly questionable independence of competence and performance" to account for many of the limitations on what is actually produced. As for under-generation, we agree with Putnam et al. and **Baker** that even infrequent examples should be considered, and we do mention a proposal for generating the verb doubling example discussed (Lillo-Martin et al., 2010). We were unaware of the English+Tamil or English+Korean data referred to in their commentary and will be very interested to follow up on those examples.

The contribution by **Liceras** offers mechanisms that we will consider incorporating to make the model more appropriately constrained. Learners (and adults with incomplete acquisition) might well have different grammatical specifications on the (possibly null) functional elements that dictate grammatical structure for their language(s), in comparison to fluent users. This may be one way that dominance is reflected within the computational system: by the presence of non-target elements in the non-dominant language. Likewise, performance factors such as a high degree of automaticity with highly familiar elements can lead to an imbalance in the influence of the dominant language functional categories. We will gratefully consider some of the suggestions made by Liceras in our on-going work on the use of determiners by bimodal bilinguals (Goodwin et al., 2016; Petroj, 2016).

We are also eager to consider further the possible role of priming in examples of synthesis (or the role of synthesis in priming), as suggested by **Serratrice**. Koulidobrova (2012, 2016), in work that has contributed significantly to the development of the Synthesis model, showed how influence through abstract syntactic features may lead to sub-optimal overt or null pronoun usage, just as Serratrice expected. Koulidobrova also suggests ways to account for the perhaps unexpected differences between bimodal and unimodal bilinguals in this domain.

2. Data

In our target article, we summarized some of our research findings involving data from very young bimodal bilinguals (for other studies from this project, see <http://www.bibibi.uconn.edu>). We would like to assure **Morgan** that we have considered a

much wider range of data (from many thousands of utterances) than is represented in the target article. For example, we know that the children are not simply acquiring Sign Supported Speech from the ways they differentiate sign and speech according to their interlocutor (Lillo-Martin et al., 2014), that they use sign in a way that is different from the way that hearing children use gesture (Gökgöz et. al, submitted), and that some of the non-target spoken utterances they produce are much more likely the result of sign influence than simple developmental errors, since non-sign-exposed children never produce such structures (Lillo-Martin et al. 2012, in prep).

Nonetheless, we agree with many of the commentators that more data, of more varied types, is important and could contribute toward needed refinements of our model. We have restricted our attention to the case of hearing children of Deaf parents so that possible differences between this group and other bilinguals, both unimodal and bimodal, would not be obscured. But the model is indeed intended to capture any kind of bilingualism, and as such, the following types of data should also be considered.

Signing. It has long been acknowledged that there are varieties of signing with a range of influence from spoken languages (e.g., Lucas & Valli, 1989). We note wide variations, however, in the degree or type of influence across different contexts. For example, speech-influenced signing is much less common in Brazil, where there is no invented system for representing speech on the hands for educational purposes, as there is in the U.S. (see Quadros, 1997). As **Crasborn** points out, the commonality of speech-influenced signing makes identifying the ‘pure’ sign language grammar difficult, so it might not be clear what the starting point of a bilingual derivation should be. Rather than worry about drawing firm lines between the grammars of Language_x and Language_y, we adopt the view that language mixing can occur and will be prevalent in some contexts due to pervasive diglossia (Stokoe, 1969). A similar approach can be found in Kuntze (2000), who discusses examples he calls ‘code-switching’ where English structures appear in signing (without speech).

Mouthing. As we pointed out in the target article, we follow Emmorey et al. (2008) rather than van den Bogaerde and Baker (2005, 2008) in excluding mouthing from our (current) consideration of code-blending. As **Crasborn** and **Steinbach** point out, mouthing of words (or parts of words) from a spoken language is employed in many sign languages, and it may well constitute a form of synthesis that has become grammaticized. Although we have not yet focused on mouthing, we have found that whispering while signing has both similar and different properties to full-voiced blending (Petroj et al., 2014, Petroj, 2016), so this is a topic that we will continue to consider.

Written language. We also mentioned bilingual effects in the reading/writing of Deaf signers, another possible source of data mentioned by **van Hell**. Again, in

principle, we would expect areas of synthesis for bilingual readers/writers, including Deaf signers accessing a spoken language through print. As van den **Bogaerde and Hulk** said with respect to our effort to collapse cross-linguistic influence of the sort seen in child bilinguals and transfer effects as observed in adult L2 learners into synthesis, it remains to be seen whether the details line up in the same way, but in general terms, we can consider all cases involving combinations of two languages as underlyingly derived by synthesis, with potentially differentiating performance factors to be explored.

Monolingual contexts. While our focus has been on exploring bilingualism effects, our starting point was a model of monolingual linguistic competence. It might not be surprising, then, that some commentators were interested in examining other linguistic phenomena that require extending the typical monolingual competence view, discussed in the following paragraphs.

Demonstration. Sign languages frequently employ mechanisms to *demonstrate* meaning using highly motivated components within linguistic structures, discussed by **Morford and Wilcox**. Following a long line of research that appeals to some combination of linguistic and gestural analyses, Davidson (2015) proposes that such structures in sign share semantic properties with quotation. As **Steinbach** suggests, we have profitably adopted this proposal to help understand derivation under the Synthesis model of code-blended utterances involving demonstrations (Quadros et al., 2016b).

Speech errors. **Morford and Wilcox** also provide examples of the ways in which signers and others combine pieces of even a single language both artfully and painfully. **Pfau** presents word blends as the monolingual equivalent of code-blending, in which two spoken words are chosen and since they compete for the same articulators, they combine. Bilingual equivalents seem also to exist, and further collection of such examples would be appreciated.

3. The devil is in the details

In the remainder of this response, we focus on aspects of commentaries addressing the two primary assumptions of our Synthesis model: (a) Distributed Morphology (DM) vs. lexicalist approaches to morpho-syntactic derivations; and (b) one vs. two simultaneous derivations in code-blending. As noted by **Baker**, more detail about specific derivations of a variety of structures under our approach is indeed needed, and such work is currently in progress. We need to make sure that the model is both sufficiently productive and sufficiently constrained.

Some of the commentators were sympathetic to our choice of DM, while others argued for a lexicalist approach. While we would not say that this difference

is merely one of preference (we hope that it is an empirical issue), neither do we expect to win converts on the basis of code-blending data alone. Nevertheless, we are gratified that some scholars who have thought considerably more than we have about the workings of DM see it as a plausible account of the types of data we have presented.

Pierantozzi, whose earlier work we built on in the target article, provided further evidence of code-switching where, she argues, a DM approach (and the Synthesis model) makes the correct predictions regarding mixed agreement. The phenomenon of mixed agreement received mention from several of the commentators (including **Liceras** and **Pfau**), with 'mixed agreement' regarding some aspects of its analysis. Code-switched utterances involving mixed agreement will form an important part of future proposals, and we agree that language pairs such as Spanish-German, where the gender systems have significant differences, will be particularly informative (e.g., den Dikken, 2011).

One of the questions that needs to be addressed under the DM approach concerns the nature of roots and the role they play in a derivation. This issue was raised by **Alexiadou** and by **Pfau**. Only briefly alluded to in the target article, this is one of the major issues we plan to address in ongoing research. **Pfau's** work on the specification of roots using data from speech errors has been influential in our thinking, but the range of code-blending we have encountered so far challenges us to pursue this topic much more deeply. With **Alexiadou**, we are hopeful that bilingual data will prove instrumental in addressing questions about the nature of roots in DM. **Alexiadou** also raised a very interesting proposal about the locus of code-switching. We hope to see more analyses of code-switching in a wide variety of language pairs to build up the empirical base for evaluating her (and our) proposals.

Lohndal raised some interesting points in comparing our DM-based proposal with a potential lexicalist alternative. He found our argument for underspecification convincing, and suggested that given the types of data we reported, a lexicalist alternative would require uneconomical dual entries of lexical items from Language 1 with features of Language 2. Recognizing the extent to which our proposal relies on existing theories, he nonetheless appreciated that by invoking a new name, we meant only to draw attention to our attempt to integrate these previous proposals and make explicit the bimodal interface.

The DM approach was not, of course, uniformly accepted by the commentators. **MacSwan** was very concerned about whether the DM approach we advocated would be able to adequately address the issues that previous late-insertion models could not handle without the use of a language 'tag' or its equivalent. One such example was the use of the words of one language in the phrase structure appropriate to the other (e.g., 'the house white' by a Spanish-English bilingual). In fact, cases

like this are found in Coda-Talk, when, for example, bimodal bilinguals in the U.S. produce spoken English words following ASL structure as a particular way of speaking in highly bilingual in-group settings (Bishop & Hicks, 2005). Bilinguals can, of course, stick to one or the other language, and they may restrict high levels of mixing (such as Coda-Talk) to very specific contexts. The extent to which such extremely mixed utterances should be generated by a model is an empirical question that we agree should be vigorously pursued. We take it as an open question whether the specific ways that DM implements grammatical differences through features on functional elements will be restrictive enough to generate only the right category of mixed sentences.

Arguments against both the DM approach and the proposal of one derivation were presented by **Donati and Branchini**, and also by **Quer**. Donati and Branchini pointed out that their late linearization solution to Italian-LIS examples with two word orders, which we summarized in the target article, is rejected in their own more recent work, after observing that when bimodal bilinguals follow two separate word orders in sign and speech, they also adopt the appropriate morphological and prosodic patterns for each language. Quer also raised the possibility that separate appropriate prosodic patterns indicates a need for separate derivations. In principle, the late application of morphological and phonological rules under a DM approach would be able to generate such patterns, since, at some point after spell-out, we assume multiple paths to the articulatory interfaces. In our own current work with adult Coda data (Quadros et al., 2016a), we find examples of multiple types, some showing incongruent word order matched with language-appropriate morphology and prosody, but other cases displaying other combinations. Since the examples we have observed so far are few, and we want to know the full extent of possible combinations, extensive elicitation of potentially incongruent structures will form a substantial component of our future research.

4. Conclusion

We reiterate our gratitude for being able to present a proposed model while it is still early in its development. The comments and feedback provided have made us consider new possibilities, and have deepened our determination to continue to pursue answers to the challenges that face us. We are especially eager for new types of evidence from other language pairs, both bimodal and unimodal, and from other domains in which Synthesis may be observed. We are pleased that we can join those studying this unique data type, and make our own contribution toward a fuller understanding of the theoretical implications of the existence and structure of bimodal bilingualism.

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