

COMMENTARIES

Vocabulary does not equal language, but neither does morphosyntax

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The study of bilingual development has been, and must be, an interdisciplinary endeavor; Carroll (Carroll) presents us with a perspective from within one particular discipline, that of generative linguistics. From this vantage point, she provides us perhaps most importantly with the reminder that LANGUAGE is not a unitary construct, and cautions against extrapolating from findings on the learning of one particular aspect of language, such as vocabulary, to LANGUAGE acquisition more broadly. I wholeheartedly agree (for a similar point, see Paradis & Grüter, 2014). I do not agree, however, with Carroll's implication that such unwarranted extrapolation is characteristic of current research on input and bilingual development. A number of recent studies have looked specifically at the differential relation between input (in a wide sense) and bilingual children's acquisition of different linguistic phenomena. Unsworth (2014), for example, reported different effects of input variation on Dutch–English bilingual children's acquisition of grammatical gender versus indefinite object scrambling. Paradis, Tremblay and Crago (2014) compared French–English bilinguals' acquisition of object clitics and definite articles, and also found that the effect of input variation differed by the linguistic property under investigation. While both of these studies approached bilingual development from primarily linguistic perspectives, recent work from the fields of psychology and communication disorders has also included multiple outcome measures, with the aim to differentiate between children's lexical and grammatical abilities at a more coarse-grained, but clinically and educationally relevant level (e.g., Bedore, Peña, Summers, Boerger, Resendiz, Greene, Bohman & Gillam, 2012; Hoff, Core, Place, Rumiche, Señor & Parra, 2012). These studies underscore the importance of Carroll's reminder not to overgeneralize from the study of a single linguistic phenomenon to LANGUAGE learning more broadly. Contrary to Carroll's rather pessimistic

assessment of the current literature, I believe the field has already made significant progress in this respect.

In another cautionary note, Carroll warns against aggregating over simultaneous and sequential learners and treating them collectively as BILINGUALS. This also is an important reminder. The particular argument Carroll adduces in support of it, however, fails to convince, and may be subject to the same criticism Carroll levels at others, namely that of overgeneralizing from observations of a narrow phenomenon. Carroll's argument against grouping simultaneous and sequential bilinguals together rests on work by Meisel – see numerous citations in Carroll (Carroll) – who hypothesizes that neural maturation around the age of 3 to 4 years radically alters how language is acquired. Closer inspection of Meisel's work shows not only a disconcerting absence of supporting evidence from neuroscientific research in the 21st century, but also an almost exclusive focus on the development of verbal morphosyntax in spontaneous production data from a small set of children acquiring a Romance and a Germanic language. My goal here is not to diminish the importance of Meisel's work, which was instrumental for the advancement of generative approaches in the study of bilingual language acquisition. Yet relying on this work, which focuses on a narrow aspect of language and a narrow set of languages, to draw broad conclusions about the need to separate bilinguals into subgroups based on a specific age of onset seems not only a bad idea, but wrong. In fact, it seems that even Meisel would agree: Meisel (2013, p. 76) writes that “it is not ‘language’ which is affected by maturational changes but *certain domains of grammar* [Meisel's emphasis]. The acquisition of lexical knowledge, for example, is not concerned at all.” While Meisel's point nicely reaffirms Carroll's earlier reminder about not overgeneralizing from a linguistic subdomain to all of LANGUAGE, it undermines her criticism of studies whose focus is exclusively on lexical development (e.g.,

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Pearson, Fernández, Lewedeg & Oller, 1997) for not separating learners into subgroups by age of onset.

Carroll's keynote serves to remind us to think carefully about the theoretical constructs involved in the investigation of input in bilingual development. This is a useful and welcome reminder, as it is all too easy to lose sight of these constructs when one is engaged in the logistical and social challenges of conducting empirical research with bilingual children and their families. There is little reflection of these challenges in Carroll's paper. Yet they are a reality not only for researchers working with bilingual children, but for language professionals who (we hope) draw on our research to inform educational, clinical and social practices in their day-to-day work with bilingual families. While Carroll laments, perhaps rightly, that "bilingual exposure research has been operating largely in a theoretical vacuum," her paper provides little guidance on how the theoretical constructs she discusses relate to the educational and clinical concerns that have motivated much of the existing research in the field of bilingual development. Thereby she may have missed the opportunity to lay out to the broader field how the perspective from linguistic theory can make a concrete contribution to the interdisciplinary endeavor of research on bilingual development.

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