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REVIEW ARTICLE

Gita Martohardjono & Suzanne Flynn (eds.). *Language in Development: A Crosslinguistic Perspective*. Cambridge: The MIT Press, 2021. Pp. xi + 346.

Q1 Reviewed by Daniil M. Ozernyi, Northwestern University

Language in Development: A Crosslinguistic Perspective (LD) is a volume honouring Barbara Lust, whose work spans the subfields of first and second language acquisition, research methodology, work with the hearing impaired, aphasia etc. Scholarly work like Lust's, which is truly cross-linguistic *sensu* incorporation of diverse intra-linguistic but cross-subfield perspectives, is exigent in modern linguistics, where the subfields grow increasingly oblivious to the existence of other subfields. As such, second language acquisitionists will benefit from attending to what is going on with merge and phases; conversely, syntacticians will find that acquisition might be helpful in their debates on multidominance and antilocality. In such an environment, where a nascent science like linguistics faces crossroads, it is vital to sustain the value of convergence and a holistic approach. LD celebrates just that. In what follows, I overview the book and comment on select papers in the edited volume. Out of 13 chapters, omitted are those by Virginia Valian, Christina Dye & Claire Foley and Maria Blume.

D. Terence Langendoen's chapter is a bracing excursion into disjunctive coordinate constructions. The inadequacy of the calculus of individuals (CI, Q3 Leonard & Goodman 1940) for disjunctive connectors is shown, and an extended CI ordering (ECI, *I**) is proposed and then recursively defined. Langendoen's theorising is interesting and sound, yet the empirical value of it is not immediately clear. Eschewing much of the technical discussion for the sake of accessibility, let me only say that the topic of disjunction and basic Booleans generally is, while fundamental, not well studied for natural language. One example is languages that disobey De Morgan's law(s). Langendoen's conclusion is that there is much complexity to disjunctive constructions in terms of logic and that children seemingly choose from hundreds of potential options when interpreting disjunction. I add that there is likely even more complexity should we attempt empirical investigations.

Q4 *Hard Words*, by the late Lila R. Gleitman and colleagues (LRG), delves into child language acquisition of vocabulary. The chapter is a summary of well-known work, so I will only raise one question which challenges the 'hard' versus 'easy' dichotomy. Before moving to 'hard' words, LRG deal with 'easy' ones, stipulating that for nouns like CAT, 'all the learner has to do is to match the real-world environment (recurrent cat situations) with the sounds of the word (recurrent phonetic sequences) in the exposure language’ (33). To my view, this seeming straightforwardness is questionable. The learners are able to identify something as a cat even if they have never been faced with, say, a white cat, while all they have seen in life are brown and black cats. Surely, a salient property of cat-ness as acquired by matching is missing from the white cat, yet the cat *is* identified as a cat, not as an unknown object. These kinds of generalisations are tricky to account for, and the seeming ‘easiness’ of acquisition of concrete nouns is, to my view, misleading. Hence, perhaps it is concrete nouns that are ‘hard’ and abstract nouns/verbs, etc., that are ‘easy’ — since, for the latter, we do have LRG’s theory of acquisition, and for the former, we don’t?

Austin et al. report on a longitudinal study on five child heritage bilingual (Spanish/English) speakers and attrition of gender and number agreement. While Q5 the conclusions drawn by the author concern the possible onset of morphological attrition and question the role of language dominance, I agree with the remarks of Q6 Martohardjono (122f), who thinks some amount of nontarget production (which the conclusions are based on) can be accounted for by exposure of the five participants to different varieties of Spanish. Indeed, I am willing to go further and say that it is extremely hard to interpret the results of a study where the participants come from strikingly heterogeneous language backgrounds when it is coupled with such a small number of participants. Indeed, the authors do not overview differences between Spanish dialects of Peru, Honduras and Ecuador, where parents of the participants of the study come from. Yet, such differences are sure to influence children’s acquisition, hence, attrition processes. Homogeneity — the degree of which can be debated — of participants’ backgrounds in any study is a requisite methodological element aiding meaningful interpretation of the study.

Q7 The chapter by Kedar is an interesting account of gestures (not signing) in child language within a longitudinal (3;0 - 3;8) case study on a Hebrew-English bilingual. While we need to be cautious interpreting the results of case studies, I heartily support the view that studies on externalisation to diverse modalities bring an interesting perspective to the field of language acquisition, and — perhaps — are Q8 even able to contribute to syntactic studies investigating syntax-PF mapping. Kedar’s study, however, faces some challenges. The broad framework is that bilinguals will gesture more while acquiring the second native language per the temporary deficits in lexicon and grammar of the target language. Yet, I contend that before empirical investigation of such conjecture is attempted, it is imperative to develop the conjecture into a model and show the precise mechanisms of the gestures’ alleged compensatory function — hopefully, in a formally sufficient and developmentally adequate way. Until then, I am not entirely sure whether Kedar’s subject gestured more because she was compensating, because she was excited to speak to Kedar or for some other reason.

Reiko Mazuka’s valuable contribution on infant-directed speech (IDS) draws some very fine distinctions which previous research in the field has not addressed. IDS is said to be conventionally slower than adult-directed speech, and this slowness (hence, overarticulation) has been a hallmark of some of the research in first language acquisition. Mazuko argues that the perceived ‘slowness’ could be a result of ‘averaging the syllable (or mora) duration without removing the contribution of phrase-final lengthening’ (195). Such averaging ‘has an effect of overestimating the syllable (or mora) duration’ (195). As Mazuka rightfully points out, this calls for re-evaluation of some of the research in the field. The findings could easily be extended to second language speech where similar methodological pitfalls apply. And even though I do not think that most domain-general models of L_1/L_2 Q9 acquisition are going to come under further scrutiny, Mazuka’s distinction is likely to be valuable for domain-specific models of acquisition (of phonology).

Q10 The chapter by Santelmann looks at discontinuous dependent morphemes (DDMs) in English and German. Not surprisingly (as she herself remarks on (221)), Santelmann finds differences in parental input in these two languages: the distance between constituents separating connected morphemes and the kind of constituents are different in English and in German. I only aim to question Santelmann’s conclusions: she writes that ‘children cannot use a universal strategy for tracking morphosyntactic relationships within sentences’ (222). The data in the chapter point to German children identifying constituents, but Santelmann claims that English children need not do that since they can just ‘focus on individual lexical Q11 items’. However, even if English children do not appear to use their cognisance of constituency, it does not mean they CANNOT use it. Moreover, even the conclusion that English children ‘need not focus’ on constituency is not entirely warranted: why not? Santelmann ties it to unnecessary ‘processing resources’, but I think additional studies are due to tease apart what really is processively burdensome, why and in what manner. Only after those findings are available can the language-specific preferences for DDMs be scrutinized.

Q12 The chapter by Yao and Packard (YP) asks whether L_2 learners parse ‘Verb NP_1 Q13 DE NP_2’ constructions in Chinese as relative clauses (RC; PRO_I NP_1 de $NP_{2\ I}$) or in a verb-object manner, along the lines of $[_{VP}$ V $[_{NP}$ $[_{PossP}$ N Poss] N]]. YP overview previous studies and conduct their own experiments to show that native Chinese speakers prefer the RC-interpretation, while non-native speakers from both head-initial and head-final languages do not analyse DE-constructions as RCs, but as VOs. YP provide thorough statistical analysis and awareness of pitfalls of ANOVA while using it with a Likert scale and the likes, thus providing alternative analyses. I concur with the conclusion drawn that there is no full or partial transfer. However, YP might benefit from looking also at the pro-drop parameter, since RC above requires a PRO to establish a dependency. Transfer actually might be detected there. Further, I vehemently disagree with the overall conclusion of the study that parsing strategies may be ‘qualitatively different’ (250) for L_1 and L_2. Yes, L_2 learners do not parse DE-RCs in the same way as native speakers do, but that only indicates that they have not achieved ultimate attainment; not that there are fundamental differences in L_1 and L_2 processing. A qualitative/fundamental difference would be presented by a different parsing mechanism, that is left-corner parser with top-Q14 down prediction component versus ‘active’ bottom-up parsers (see work by Masaya Q15 Yoshida). The authors do give a nod to the fact that ‘proficiency [could have] contributed to the nonnative parsing decisions' (150). As proficiency generally tends to account for all things non-native, this should be the null hypothesis and just a nod does not resolve the potential objection. Further, the information about the background of the L_2 participants and testing which was performed is very sparse. A score of 65 out of 100 on some Chinese language test potentially lacking in validity or/and reliability and describing the learners as 'advanced' says exactly nothing about the proficiency of the learners. 'Advanced', 'intermediate' are just labels which when not supplied by ample descriptors are empty. Yet, obliviousness to Q16 requisite validity of language assessment is a more than endemic problem in SLA research, far from being peculiar to this study.

Q17 Martohardjono, Valian and Klein (MVK) take up the deficit and transfer accounts (d/t) of L_2 acquisition in their chapter, while looking at acquisition of tense. Personally, I would only say that d/t's claims (about Universal Grammar, for example) presuppose that what is acquired is a non-human language so they are investigating something entirely different from the faculty of language in the Chomskyan sense. MVK, however, are willing to go much further and engage with the d/t in a meaningful manner. They provide a careful overview of studies up to date and show that d/ts are not compatible with observed evidence for young Q18 learners, not only for advanced or 'near-native' learners (which has been a hallmark of d/t accounts). I applaud MVK's crusade while treading very gingerly regarding their reliance on frequency and saliency — those constructs are not yet entirely (well)defined (saliency, in particular) or linked to formally sufficient accounts (frequency), so we need to exercise extreme caution while making use of them.

Q19 Q20 Phillips et al. (IP) look at heritage Spanish speakers and late ESP-ENG bilinguals' differences in processing. They provide a comprehensive account of existing literature and get at the right questions, some of which I highlight below. First, I laud them for drawing attention to the fact that there is no significant difference in susceptibility to L_2 influence between heritage speakers and late bilinguals (310). More importantly, heritage speakers who acquired L_1 and L_2 in similar environments and perform similarly on proficiency tests do not necessarily constitute a homogeneous sample. I stressed the vitally requisite homogeneity elsewhere in the review, and IP are consonant with that. Lastly, as with most groups under psycho-Q21 linguistic spotlight, 'heritage speakers' are defined very loosely and research on different populations are being grouped under the same label, pointing to only superficially contradicting results. More care with terminology and who the terminology is exercised upon is exigent, and IP offer evidence to support that.

Lastly, Sherman and Flynn (S&F) overview the available linguistic research on early language changes in Alzheimer's disease and offer an important methodological point to bear in mind for clinical research: the former can inform the latter in valuable ways. The methodologies used in first language acquisition studies are entirely applicable for studies of subjects with mild cognitive impairment, to which their experiments investigating free relatives, ellipsis in coordinate sentences, etc., richly attest. The cross-discipline applications that open the horizons of new insight are inspiring, and S&F's ingenuity in driving them is admirable.

Despite the critiques above, the volume achieves what it sets out to achieve: under a common theme of language development, studies from different areas of linguistics are combined and made mutually relevant. Semantics, syntax, processing, L_1/L_2/heritage/bilingual acquisition, methodology, clinical linguistics all come together in an overall successful attempt at a cross-linguistic volume in the spirit of Barbara Lust! I stand staunchly with Martohardjono and Flynn in advocating for cross-field cooperation, and I see enormous benefits in future work, the objective of which is to point out different, bracing, unconventional, not immediately observable perspectives on — for some old, and for some new — phenomena. In fact, I think this is the only way forward.

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