

BOOK REVIEWS

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INSA GÜLZOW & NATALIA GAGARINA (eds), *Frequency effects in language acquisition: Defining the limits of frequency as an explanatory concept*. Berlin: Mouton de Gruyter, 2007. Pp. 422. ISBN 978-3-11-019671-9.

A serious discussion of frequency effects in child language acquisition is long overdue. Thus the editors of this volume are to be congratulated on bringing together contributions from a range of different theoretical viewpoints in order to address this issue. The main value of the book is that it prompts the reader to consider precisely (a) what the main theoretical approaches do and do not predict with regard to frequency effects and (b) how best to test these predictions. In this review, I summarise some of these considerations in the form of ‘Ten Commandments’ of frequency-effect research.

(1) *Test frequency-based predictions of particular theories – at the level of abstraction posited by each theory – as opposed to hunting frequency effects*

Several of the studies look for ‘frequency effects’ (e.g. correlations between the frequency of some item in child and caregiver speech) without reference to a particular theory. This is problematic as different theories predict frequency effects at different levels. For example the frequency-based generativist account of Yang (discussed in more detail later) predicts correlations at a very abstract level: the higher the frequency of morphemes that instantiate TNS in a particular language, the earlier children learning that language will obligatorily mark tense where required. In contrast, lexical-constructivist accounts predict correlations at the lexical level. For example, Pine & Lieven (1997) demonstrated a correlation between the frequency of particular DET+NOUN combinations (e.g. *The + man*) in child and caregiver speech.

In this volume, Bohnacker (for Swedish and German) and Kupisch (French, German and Italian) look for cross-linguistic correlations between rates of determiner omission in children and bare-noun production in adults at the level of the syntactic category (e.g. DET+NOUN). This would constitute a fair test of a theoretical proposal in the spirit of Yang, but not that of Pine and Lieven. But because these studies are not framed as investigations of specific theoretical accounts of determiner omission, the implications of their findings (sporadic frequency effects) are unclear. Similarly, Westergaard and Bentzen investigate Swedish children’s over-generalization of V2 word order into embedded clauses looking at the level

of syntactic constituents. Again, this would be an appropriate test of a theory under which children probabilistically set a V₂ parameter but not a lexical-learning account. However, no theoretical proposal is tested explicitly. Djurkovic analyses the relationship between adult and child proportions of true passives (e.g. *Ulysses was written by Joyce*) versus impersonal ‘passives’ (e.g. lit. *The house is building itself*) at the level of the construction, though – in this case – neither lexical-learning accounts (e.g. Israel, Johnson & Brooks, 2000) nor formal accounts based on A-chain maturation (e.g. Borer & Wexler, 1987) would necessarily predict such a correlation. The situation is similar for the study of Kauschke and Klann-Delius, who look for child–parent correlations in (German) vocabulary development at the level of word categories (e.g. NOUN, VERB), as opposed to individual words (e.g. *cat, dog*).

This question of ‘frequency of WHAT?’ is also raised by the study of Gagarina (see also Freudenthal, Pine, Aguado-Orea & Gobet, 2007). These authors demonstrate that the proportion of so-called root infinitives (e.g. **Jack eat the cake*) in children’s speech is related not to the OVERALL frequency of non-finite forms in adult speech, but to the frequency of such forms IN UTTERANCE FINAL POSITION (e.g. *Let Jack eat the cake*). As Roeper points out in his introductory chapter, the appropriate level at which to look for frequency effects will often not be immediately apparent. The appropriate response, though, is not to dismiss frequency effects a priori (essentially Roeper’s approach), but to test the predictions of theories that posit frequency effects at a particular level.

(2) *Define the product of frequency*

When looking for an effect of X on Y, it is clearly necessary to define not only X (i.e. frequency) but also Y (i.e. what frequency is supposed to have an effect on). Several authors discuss the predictions that the most frequent items in the input are the first to ‘emerge’ or ‘reach target criterion’. The notion of emergence is problematic because there is no non-arbitrary way to decide how many times a particular item must be produced (or in how many different constructions) before it can be said to have emerged. Conventional target criteria (e.g. 90% use in obligatory contexts) are equally problematic as, on the one hand, children could meet this criterion using a small inventory of rote-learned phrases and, on the other, adults often fail to reach this criterion (e.g. see Estigarrribia, in press, on inversion in yes/no questions). A third possible prediction is that the most frequent items will be the first items which the child uses productively, though productivity is arguably even harder to define (for possibly the most sophisticated attempt to date, see Aguado-Orea, 2004). Arguably the strongest frequency-based prediction is that specific lexical frames that are frequent in adult speech

(e.g. *What do you ... ?*) will be associated with LOWER RATES OF ERROR (e.g. **What you do want?*) than less frequent frames (e.g. *What could they ... ?*). Given that many such studies have been conducted using both naturalistic (e.g. Rowland, 2007) and experimental data (Ambridge & Rowland, in press), it is a shame that no studies of this type appear in the present volume (see 10).

(3) *Don't confuse 'frequency' and 'counting'*

Authors who argue against frequency-based approaches sometimes interpret such approaches as saying that the child 'counts' (Roeper, p. 24) or 'computes and matches the frequency of various elements in the input' (Bohnacker, p. 54), or that the target of acquisition is 'knowledge of frequency' (p. 55). This is not an accurate portrayal of frequency-based approaches, either generativist (e.g. Yang) or constructivist (e.g. Tomasello, 2003). Whether the child is using instantiations of abstract categories to probabilistically set a parameter (e.g. +/- TNS) or individual lexical strings to acquire a lexical schema (e.g. *What does [THING] [PROCESS]?*), no 'counting' or 'frequency matching' is involved. Under either type of account, 'frequency effects' are epiphenomenal and, at least in some cases, may reflect nothing more than the mundane fact (denied by Roeper on p. 29) that human memory is fallible, and that a string (such as a person's name) will often not be available for recall until it has been encountered multiple times. Throughout the volume, many authors make the valid point that frequency cannot be an 'explanation' and that what is needed are accounts of learning that incorporate frequency effects. It is unfortunate, then, given that such accounts exist (on both the generativist and constructivist side) that few of the studies in this volume test explicit frequency-based predictions derived from them (see 1).

(4) *Don't imply that frequency-based accounts rule out other factors*

Most authors in the present volume rightly discuss factors other than frequency that contribute to ease of acquisition. These include acoustic/phonological/prosodic salience (Gagarina, Kupisch), position in the utterance (Gagarina), semantic transparency/cognitive complexity (Gagarina, Uziel-Karl & Budwig) homonymy/one-to-one vs. one-to-many form-function mappings (Kupisch, Savic & Andelkovic), illocutionary force (Westergaard & Bentzen), animacy (Bordag), and, perhaps most importantly, the child's communicative goals (Uziel-Karl & Budwig). Whilst these factors are often presented as demonstrations that 'frequency

cannot be the whole story' (Yang, p. 398), no account in which frequency is an important factor (including Yang's own theory, as well as rival constructivist proposals) has ever claimed that it is, or even downplayed the importance of any of these other factors. To be sure, more constructivist studies have investigated frequency than other factors such as semantics or pragmatics (though see Theakston, Lieven, Pine & Rowland, 2004; Cameron-Faulkner, Lieven & Theakston, 2007; Ambridge, Pine, Rowland & Young, 2008) but there are two (to my mind, valid) reasons for this: first, unlike many of the other factors, frequency can be easily and objectively measured; hence the data are amenable to quantitative statistical analysis (see 7). Second, whilst most of the above factors are important under both generativist and constructivist accounts, some particular frequency-related predictions made by constructivist accounts are not shared – indeed, are incompatible with – certain (by no means all) generativist accounts (for specific examples, see 5). Thus frequency is a factor that can potentially be used to adjudicate between the two approaches.

(5) *Don't claim that frequency effects are compatible with theories that rule them out*

Bohnacker (p. 55) writes that 'lexical learning, learned strings that turn into schemata and frequency pattern learning are vital components of language acquisition ... and I would venture to claim that most generative acquisitionists think so'. It seems to me that some generativist acquisitionists increasingly appear to accept evidence of frequency effects at the lexical level, yet maintain theories that are not compatible with such effects.

As a specific example, consider Valian & Casey's (2003) account of children's acquisition of *wh*-questions: '[Children] must learn that ... whatever is in INFL can be moved in front of the subject in questions (to COMP) – so-called subject–AUX inversion' (p. 119). In other words, children are learning a FORMAL RULE for subject–AUX inversion. Even if we grant that children may learn this rule on an AUX-by-AUX basis (which seems rather contradictory to the spirit of the proposal), this account cannot explain why children should invert a particular lexical auxiliary (e.g. *is*) when it appears with some *wh*-words but not others. Such a finding would suggest that children are acquiring not a formal subject–AUX inversion rule, but schemas based around particular *wh*-WORD+AUXILIARY combinations (e.g. *What is [THING] [PROCESS]?*). Valian & Casey (203: 118) cite evidence that one child studied 'inverted best with combinations of *wh*-words and auxiliaries that were frequent in the input (Rowland & Pine, 2000)' yet apparently do not consider this finding to constitute

evidence against their own theory; neither do they discuss how their theory could be modified to include a mechanism that would yield this finding.

Returning to the present volume, it is important to be clear that even generativist accounts that include a role for frequency at the level of abstract lexical or functional categories may be incompatible with frequency effects at the lexical level. Under the probabilistic parameter setting account of Yang, different grammars (e.g. +TNS and –TNS) compete to parse input sentences, with successful grammars rewarded, and unsuccessful ones punished (e.g. if a child hears *I run* the –TNS grammar is rewarded; *I kicked* rewards the +TNS grammar). This theory can explain frequency effects at the macro level; for example, English children produce more ROOT INFINITIVES (e.g. **Jack eat the cake*) than Italian children because they hear more sentences that are not unambiguously marked for tense. They cannot, however, explain frequency effects at the micro level. For example, Freudenthal, Pine & Gobet (submitted) show that one Dutch child produces the verb *bouwen* ('to build') exclusively in non-finite form (e.g. **Ik een toren bouwen*, *'I a tower to build'), with over 95% of the mother's uses of this verb in compound finites (e.g. *Ga je een toren bouwen*, 'Go (modal) you a tower to build'). Conversely, the verb *passen* ('to fit') is used by the child in exclusively finite form, with 87% of the mother's uses finite (e.g. *Dat past niet*, 'That fits not [i.e. doesn't fit]'). Such a detailed pattern of frequency effects is hard to explain on the assumption that children are using input utterances to set a parameter as opposed to learning individual strings.

(6) *Rule out frequency effects to find other factors that influence acquisition*

It is uncontroversial that factors other than frequency affect acquisition (see 4). However, many findings that are claimed to provide support for the existence of these factors do not convincingly do so, as confounding frequency effects have not been eliminated. For example, virtually all authors who propose that acquisition is influenced by the setting of a head-direction parameter assume that the fact that English children produce utterances such as *I want it*, but not **I it want*, constitutes evidence that they are in possession of, and have correctly set, the parameter (e.g. Wexler, 1998). In fact, such utterances can provide evidence for this claim only when a simpler input-frequency based explanation (children hear *I want it* far more frequently than *I it want*) has been ruled out. My point is not to claim that an input-based account is necessarily superior to a parameter-setting account (or vice versa). The point is simply that before we can even begin to evaluate any account of any phenomenon, we must rule out simpler explanations.

(7) Conduct a statistical analysis of the data

More than half of the authors in the present volume do not conduct a statistical analysis of their data (though frequency data is, by its very nature, quantitative). To see why this is a problem, consider the otherwise excellent study of Bordag, who presents the findings of a WUG TEST study designed to elicit nominative plurals from learners of Czech. From looking at the means, it seems that children overgeneralize the most frequent plural marker (-i) rather than following the animacy distinction made by the language. If confirmed statistically, this would be an interesting finding. Without such an analysis, this possibility remains purely speculative, as we cannot tell how often children would 'overgeneralize' the -i marker to inappropriate contexts if they were simply producing one at random (as opposed to the most frequent inflection).

(8) Collect a dense sample

Infrequent sampling of naturalistic data (some authors in this volume sample only at monthly intervals) will cause at least three problems. First, frequency estimates will be unreliable (Rowland & Fletcher, 2006), meaning that even large effects may be missed. Second, if an insufficient number of instances of the target construction (e.g. DET + NOUN combinations) are recorded, then there will not be enough variance between different cases (e.g. individual caregiver-child dyads) for statistically significant correlations to be observed. Attempts to mitigate this problem often lead to a third: if data from long time periods are collapsed together, this will often include periods in which the child is at floor (i.e. almost never produces the relevant construction) or ceiling (i.e. can produce the construction at will, having acquired the relevant rule or schema), masking any correlation with the input that occurs over a shorter period. Many of the studies in this volume conclude that the absence of a frequency effect with regard to some phenomenon is theoretically interesting. Such claims are difficult to evaluate since, given the thin sampling regime, it is often possible that the effect in question would have been observed with a larger dataset.

(9) Don't assume that frequency effects must be (log)linear

The absence of a linear correlation between some item in the input and output data (at whatever level) does not necessarily entail the absence of a frequency effect, as such effects may be subject to thresholds. This possibility is not considered in the present volume, though many other authors have proposed that constructions may reach a 'critical mass' for generalization (e.g. of the -ed past tense pattern in English), beyond which further instances may have little (or less) effect (Marchman & Bates, 1994).

Similarly, though most authors discuss factors other than frequency that may affect acquisition, none of the authors who fail to find frequency effects discuss the possibility that the failure to find such effects may result from the failure to control for these potentially confounding factors.

(10) *Conduct an experimental study*

The majority of the studies reported in this volume are naturalistic corpus-based studies. Although such studies have an important place in language acquisition research, it is important to confirm their findings experimentally, as naturalistic data studies can only ever provide correlational evidence. This means that when a correlation between the frequency of some item in caregiver and child speech is observed, it is difficult to rule out theoretically uninteresting explanations. For example, it could be that the caregiver is adapting the conversation to the child's interests, or that certain items (e.g. *do*) are more useful in the language as a whole than others (e.g. *rate*), and so are used more frequently by both caregiver and child. Although many of the present authors (e.g. Bohnacker, Savic & Anđelković, Uziel-Karl & Budwig) make this point, few (Djurković, Bordag, Westergaard & Bentzen) conduct experiments that have the potential to rule out such confounds.

In conclusion, it is to be hoped that this volume will provide the impetus for future studies that aim not simply to identify frequency effects, but to test frequency-related predictions derived from explicit theoretical accounts of language acquisition.

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J. COLOMBO, P. MCCARDLE & P. FREUND (eds), *Infant pathways to language: Methods, models, and research directions*. Hove: Psychology Press. Pp. 336. ISBN: 978-0-8058-6063-4.

This ambitious and stimulating volume arose from a 2005 conference sponsored by the Merrill Advanced Center at the University of Kansas and the Eunice Kennedy Shriver National Institute of Child Health and Development. A principal aim of the conference was to investigate the relationship between specific early language and cognitive abilities and later language outcomes, with a view to informing our understanding of both typical and atypical language development. As Rice argues in the Foreword, few publications in the secondary research literature have attempted to synthesise recent longitudinal data from a variety of research groups, and in this sense the contribution of this volume is particularly timely. Many of the contributors have vigorously taken up the challenge of identifying factors underlying variation in language acquisition. For example, Rice (Chapter 5) explores genetic risk factors in language delay/impairment, Colombo and Colleagues (Chapter 10) chart the relationship between early assessments of attention and later vocabulary growth, Meltzoff and Brooks (Chapter 11) explore correlations between early gaze following and later vocabulary growth, and Kuhl (Chapter 12) reviews a fascinating series of studies suggesting a strong relationship between early speech perception and variety