ACQUIRING PHONOLOGY

Children often mispronounce words when learning their first language. Is it because they cannot perceive the differences that adults make or is it because they can’t produce the sounds involved? Neither hypothesis is sufficient on its own to explain the facts. On the basis of detailed analyses of his son’s and grandson’s development, Neil Smith explains the everyday miracle of one aspect of first language acquisition. Mispronunciations are now attributed to performance rather than to competence, and he argues at length that children’s productions are not mentally represented. The study also highlights the constructs of current linguistic theory, arguing for distinctive features and the notion ‘onset’ and against some of the claims of Optimality Theory and usage-based accounts. Smith provides an important and engaging update to his previous work, The Acquisition of Phonology, building on ideas previously developed and drawing new conclusions with the aid of fresh data.

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ACQUIRING PHONOLOGY

A Cross-Generational Case-Study

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Preface

Long ago I published a book (Smith, N.V., The Acquisition of Phonology: A Case Study, Cambridge University Press, 1973), documenting the linguistic development of my elder son Amahl (A). More recently I have been documenting the linguistic development of his elder son Zachary (Z), and have observed interesting similarities and differences between the two cases. More importantly, the advantages of hindsight in combination with advances in the field have enabled me to revise or reinterpret some of my earlier conclusions. The results of this “Acquisition of phonology from A to Z” follow.

I should start by admitting that there are several possible limitations of the study. Interacting with a grandchild is not the same as living with one’s own offspring, and my contact with Z was relatively sporadic compared to my continuous contact with A. However, in the three years or so during which I collected data, I had over 150 sessions with him and rapport was excellent, so I don’t think I have missed much of significance in the areas I concentrated on. This leads directly to the second limitation: the focus of the study is largely restricted to segmental phonology, with no systematic discussion of prosody. This is partly a reflection of my competence, partly a function of time and equipment. I am aware that research into first language acquisition is more sophisticated and hi-tech than it used to be (see e.g. Chun, 2007), but I still think there is room for the kind of detailed diary study that I carried out before. I am therefore not entirely convinced by Snyder’s (2007: 51) use of the past tense when he writes “diary studies were valuable as a source of impressionistic data, before modern recording technologies became available”. I confess that, apart from a tape-recorder, I made no use of the kind of instrumentation that would have made various perceptual and other tests possible. In general I do not think this has seriously impaired the analysis but it does mean, for example, that I was unable systematically to investigate for the existence of ‘covert contrasts’ in the

1 If McMahon (2007:181) is right that “prosodic and segmental phonology are separate components neurologically [and] acquisitionally” this limitation is venial.
sense of Scobbie et al. (2000; cf. Macken & Barton, 1980). Finally, and most obviously, phonological theory has moved on since 1970 and I may not have kept up with it as much as is desirable. None the less, there is value in using a descriptive framework that is largely traditional or theory-neutral, and I have included critical discussion of issues arising in current theories, especially Optimality Theory (OT). I am comforted by the thought that dinosaurs can be interesting.

To offset these limitations, there are some positive aspects of the study. To the best of my knowledge it is the first cross-generational study of any aspect of language acquisition. Too much should not be read into this fact: there are no genetic, epigenetic or environmental claims lurking in such a minute sample, but direct comparability (same author, same family) with the earlier study should ensure that any generalisations are genuine. There is also the fact that the current study started earlier (with babbling) so the nature of the early stages should be clearer, and I have deliberately synchronised some of the stages of analysis of the two subjects to highlight parallels and differences between A and Z (A’s stage 1 is chronologically the same as Z’s stage 4). At a minimum, I hope I will have provided more useful grist for the reanalysis mill. A major feature of the 1973 study (hereinafter ‘APh’) was the set of linguistically analysed longitudinal data – data which have been widely exploited ever since. Fikkert (2007: 538) made a plea for more databases, and this book includes another one. I would be delighted if it elicited even a fraction of the reactions that its predecessor did.

The structure of the book is as follows. Chapter 1 provides an overview of my main theoretical presuppositions, chapter 2 summarises the findings of APh, and chapter 3 discusses some of the subsequent developments in our understanding of the acquisition of phonology, in large part on the basis of reactions to APh. Chapter 4 presents an analysis of Z’s developing phonology from the first babbling to his mastery of the (segmental aspects of the) adult language. This leads in to chapter 5, a discussion of the nature of the acquisition of phonology, which is followed by a diachronic lexicon, a variety of appendices and the usual list of references.

Although the major developments of Z’s phonology are given in both prose and formal rules, I have reduced the plethora of formalisation that characterised the earlier monograph, so this one should be a little more user-friendly. There is inevitably a large amount of data which have necessitated the pervasive use of phonetic transcription. For representations of the adult language – basically the ‘Received Pronunciation’ (‘RP’) of British English – I use the system of Wells (1990), with representations in oblique strokes /x y z/; for the children’s
pronunciation I use the alphabet of the International Phonetic Association (IPA) with representations in square brackets \([x \ y \ z]\); for intermediate stages in derivations involving the children’s putative own system, I use elements of the IPA with representations in pipes \(|x \ y \ z|\). Where the context makes it obvious what kind of representation is involved (e.g. in the diachronic lexicon) I have omitted the distinguishing brackets. It should be noted that these conventions mean that an example such as adult /pen/ (pen) might be produced ‘correctly’ as [pɛn] by the child even though this correctness is disguised by the transcriptional difference between ‘e’ and ‘ɛ’.

There are, of course, many differences between the monographs and their implications. The most significant is the claim that the major determinants of the children’s productional divergences from the forms of the adult language (their mispronunciations) are a matter of performance rather than of competence. Justifying this conclusion forms part of a discussion of the nature of representation and metarepresentation, and leads to the suggestion that the children’s output is not in fact ‘represented’ at all. For those who are happy to take the data and their analysis on trust the main conclusions can be found in chapter 5. Most will be more sceptical.

Finally, a note on pronominal usage: I have used ‘he’ rather than ‘she’ or ‘he or she’ to refer to the generic child acquiring his phonology. My excuse is that I have sons and grandsons but no female descendants. No one is meant to feel excluded.
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Zachary and Amahl at the ages of 2 years 3 months and 36 years respectively