932

BOOK REVIEWS

Phonological Development: Models, Research, Implications. Edited by Charles A. Ferguson, Lise Menn, and Carol Stoel-Gammon. Timonium, MD: York Press, 1992. 693 pp, $74.50

Reviewed by
ALICE FABER*
Haskins Laboratories

In 1978 a conference on child phonology was held in Bethesda, MD, sponsored by the National Institute of Child Health and Human Development (NICHD). The proceedings of that conference were published in 1980, in a two-volume set entitled *Child Phonology*, edited by Grace H. Yeni-Komshian, James F. Kavanagh, and Charles A. Ferguson (henceforward, CP). That two-volume set provided researchers and students alike with a convenient summary of the late 1970's / early 1980's "state of the art" in child phonology research. As anticipated by the organizers of the 1978 conference, the field of child phonology has developed rapidly, spurred by theoretical, methodological, and technological innovations. Consequently, a second conference, also sponsored by NICHD, was convened at Stanford University in 1989. The proceedings of the second conference were published in 1992, in the volume under review. In this review, I will first describe some editorial flaws in *Phonological Development*; I will then turn to some issues raised by the content of the volume.

Skimming *Phonological Development* (PD), one is first struck by its unevenness. This unevenness is evident in the length and the depth of the various chapters. The 24 chapters are on average 26 pages long, ranging from nine pages (BRADLEY)[1] to 48 (JUSCZYK). The shorter chapters, especially those by INGRAM and BARTON, are overly general and insufficiently detailed, to the point that their claims are unclear. The longer chapters, in particular those by JUSCZYK and FLEGE, could have done with some judicious shortening. The chapters are arranged in three sections, Models, Research, and Implications, each headed by an Overview by one of the editors. While the tripartite division

---

[1]   Throughout, I use the author's name in small caps to refer to chapters in the volume under review. Where chapter number is relevant, it is included in parentheses after the author's name.

makes *a priori* sense, the classification of individual chapters can be quite arbitrary. For example, it is unclear why WERKER AND PEGG's (9) developmentally-based four factor model of speech perception is Research, while DINNSEN's (6) typological account of developing phonological systems is a Model. Likewise, GRUNWELL's (16) comprehensive overview of theoretical and pragmatic considerations in the diagnosis of phonological disorders and the design of clinical treatment plans is classified as Research rather than as Implications, while LEONARD's (17) evaluation of models of phonological learning against the data of phonological disorders is by contrast in Implications. The volume would be more useful if conceptually related chapters had been grouped together, regardless of how data oriented they are. The groupings in which chapters are discussed in the body of this review constitute but one possible arrangement that would have been preferable.

The unevenness of PD is further evident in its extensive editorial problems. These range from forgivable lapses of copy-editing and proof-reading (for example, Velleman 1988, referred to in footnote 2 (p. 216) of MENN AND MATTHEI, does not appear in their bibliography; and Studdert-Kennedy and Goodell 1990, referred to by VIHMAN (p. 415), is not in her bibliography) to serious problems with some of the chapters that should have been caught by referees and then fixed prior to the chapters' being included in the volume. The inappropriate length described above of some of the chapters is only one of these problems. For example, MACKEN's chapter ends with a footnote which begins: "Other obligations prevented me from completing this chapter" (p. 267), and continues with\a sketch of how she would have completed the chapter; either the footnote or the chapter should have been excluded from the volume. MENN AND MATTHEI's chapter, while admirable in its eschewal of passive voice, contains scattered instances of a writing style more appropriate to e-mail communications between close friends than to scholarly writing. McCUNE's chapter contains extended jargon-filled passages of the sort that give scholarly writing a bad name.[2] The figure that WERKER AND PEGG describe in their discussion of their Figure 1 may well involve the data points that appear in Figure 1, but it clearly has them arranged differently; the statements in the text are true of Figure 1, but it requires some effort on the part of the reader to see that this is in fact the case. In the case of BARTON's contribution, a prior familiarity with the extensive literature on phonological awareness and reading (e.g., Liberman and Shankweiler, 1987; Morais, Bertelson, Cary, and Alegria, 1986; Read, Zhang, Nie, and Ding, 1986; Wagner and Torgesen, 1987; Goswami and Bryant, 1990; and the many references cited in these papers) is needed for the reader to understand the position that Barton is allusively dismissing, without argument or apparent motivation. While some chapters present previously unpublished data in support of their authors' theoretical stance, others, in particular those by INGRAM and MOHANAN, serve

---

[2] For example (p. 318): "Werner and Kaplan (1963) proposed a strong hypothesis that the motive for representation and communicative development was the child's effort to maintain intersubjectivity with the mothering one in the course of self-other differentiation." And on p. 333: "If paths toward language are characterized by strong individual differences in how and when the critical variables express themselves, the general problem of how language is acquired will only be solved by close examination of the systematicity inherent in the relevant variables as they interact developmentally for each child."

primarily as brief commercials for longer expositions published elsewhere and are consequently not self-contained.

As noted by Jenkins in his afterword to CP and reiterated by several of the contributors to PD, child phonology is a quintessentially interdisciplinary field. The range of fields a child phonology researcher should be conversant with includes (but is not restricted to): linguistic theory, speech production and perception, developmental psychology, language evolution, learning theory, and language typology. Like the editors of CP, the editors of PD have included chapters on some of these 'peripheral' areas as well as papers representing research in child phonology itself. It is difficult to quarrel with the editors' choices of what to include (except insofar as specific chapters are overly short, as noted above). It is also difficult to argue that specific researchers should have been included as well; after all, some of the obvious omissions might in fact have been invited to contribute but been unable to do so. That said, however, it is surprising in the extreme that certain research areas are wholly unrepresented. These areas include: syntactic development, lexical development, developmental psychoacoustics, development of signed language (e.g., ASL), development of motor control, language development in individuals with specific developmental disorders (e.g., Down's Syndrome, Williams Syndrome), educational issues relevant to language development, and language break- down in aphasia. These are not research areas in which I claim any expertise but rather areas that *a priori* seem relevant to phonological development. Considerations of size (and, no doubt, expense) would have precluded representing all of the above areas; as it is, the volume is unwieldy. Nonetheless, it is disconcerting that animal communications systems (HAUSER AND MARLER) are seen as more relevant to human phonological development than human syntactic development is. In fact, the only discussion of syntax in PD is in the chapter by ELBERS AND WIJNEN (11). They use the fact that children experience 'developmental' phonological disfluencies around the time that they are first manifesting clear syntactic (as opposed to semantic) structure as evidence for a skill component in phonology. The temporary disfluencies reflect this skill's being disrupted because so much of the child's attention is being directed at syntax.

In their introduction to the 1980 volume, Ferguson and Yeni-Komshian predicted: "A successor conference on child phonology 10 years hence, we can be fairly certain, will regard this one as limited and unsure of itself, but we can hope that many of the solid themes of that conference will have been adumbrated in these chapters" (p. 6). It is a measure of the progress in the field over the past decade that both of these predictions are arguably false. On the one hand, many of the contributions in PD contain gratuitous reference to papers or section overviews in CP, in an apparent attempt to legitimize theoretical contributions by grounding them in a shared canon, much as American social and political reformers ground their proposals through reference to the Declaration of Independence, the Constitution, or preserved speeches by past Presidents. Of course, many other references to chapters in CP are not of this gratuitous nature, but rather constitute legitimate citations of the scholarly literature. Crucially, however, neither sort of reference embodies a covert claim that CP is in any way dated. On the other hand, to the extent that a theme can be discerned in PD, it is not one that was presaged in CP. Rather, a general theme through many of the chapters in PD is *continuity and discontinuity in language development*.

In the past, the surface discontinuity between babbling and early speech (early

speech exhibits at least a rudimentary mapping between sound and meaning, while babbling does not) was recruited, most notably by Jakobson (1941), in support of a more essential discontinuity. For Jakobson, there was no relationship between any characteristic of babbling and any characteristic of early speech; he saw babbling and speech as qualitatively distinct activities. With the benefit of hindsight, OLLER AND LYNCH dismiss this view as "preposterous" (p. 511) and assert that it could not possibly have been held by anyone who has ever actually listened to infant babbling and speech.[3] Nonetheless, despite some cracks in the armor, it was still part of the received wisdom that language development starts with an infant's first words when I took undergraduate Psycholinguistics courses in the early 1970's, perhaps due to the publication of the English translation of Jakobson (1941) in 1968. However, as reviewed by Menn in CP, by the end of the 1970's it was clear that babbling is in crucial ways a precursor to early speech. So, when INGRAM asserts that the view of language development he is sketching in PD is "Neo-Jakobsonian", he is referring not to Jakobson's claim of discontinuity between babbling and speech but rather to his view that phonological inventories increase in size during early language acquisition by virtue of the infant's control over an increasing number of distinctive features. Likewise, when MACKEN advocates a return to the spirit of Jakobson (1941), she is proposing a renewed focus on phonological rather than phonetic acquisition; in her view, phonology is autonomous from phonetics.

Three different approaches to the continuity question are evident in PD. The first approach is to document additional points of continuity between babbling and early speech. The second approach is to investigate continuity more generally throughout language development, including learning a foreign language in adulthood. Finally, the third approach is to develop a theoretical basis for apparently discontinuous output resulting from continuous processes of development.

Two distinct approaches to babbling-speech continuity are exemplified in PD. BOYSSON-BARDIES and her colleagues (12), in a large-scale study of children learning French, Swedish, Japanese, and American English, provide extensive documentation of "babbling drift". Throughout the children's babbling and early words, the segmental inventory of their utterances comes to resemble that of the language that they will

---

[3]   While systematic surveys reliably and repeatedly show that the sounds that predominate in most infants' babbling are those that are typologically the most common (e.g., Locke, 1983), phonetically more unusual babbles (e.g., [kxkxkx], [ŋaŋaŋa]) are more salient and, hence, more likely to be noted in informal observation. Certainly, of all the friends' children that I have been acquainted with while they were acquiring language, the only child whose forms I can recall some years later is Miriam, who, like Jacob (described in MENN AND MATTHEI), had no bilabials in the c. 50 words she had at 18 months. Unlike Jacob, however, she did not avoid words containing bilabials. Rather, she consistently replaced the bilabials with dentals, pronouncing her own name [nijə].

The empirically determined low frequency of babbling types that had previously been thought characteristic of a particular developmental stage is exactly comparable to the finding (Marcus, Pinker, Ullman, Hollander, Rosen, and Xu, 1992) that morphological over-regularization (e.g., *goed, foots*) is likewise statistically infrequent.

eventually learn. Thus, at the beginning of the babbling period, [h] was heavily represented in the French children's babbles, as in those of the other children. However, by the time that they had acquired 25 words, [h] had virtually dropped out of the French children's repertoires, in accord with the absence of /h/ as a segmental phoneme of French. The influence of the phonological inventory of the ambient language on the segmental inventory of children's prelinguistic babbles is but one aspect of the continuity between babbling and early speech. VIHMAN (13) takes a different approach. Using the same subjects as BOYSSON-BARDIES *et al.*, VIHMAN compares individual children's babbling inventories with their first words. Across the four language groups, for 75% of the children the syllables constituting their first words were among the most frequent syllables in their babbling. In other words, the syllables that these children had practiced the most during babbling provided the building blocks for their first words. The 25% of the children who did not manifest this correlation were those who were the slowest (chronologically) in lexical development. VIHMAN attributes their non-reliance for their first words on the syllables they had practiced to their greater cognitive maturity at their entry into the lexicon.

The approach to continuity between babbling and early speech exemplified by BOYSSON-BARDIES *et al.* and VIHMAN is within the tradition of past research. The second approach to continuity, that focusing on a longer time span, is more innovative. STOEL-GAMMON (15) and LOCKE AND PEARSON (3) review an extensive literature which, in the aggregate, suggests that the amount and type of vocalization in the canonical babbling stage (starting at about six months of age) correlate with verbalization measures at 11–15 months, amount of talking at three years, vocabulary level at 27 months and five years; vocal imitation at 14 months correlates with verbal imitation at 20 months and with measures of vocabulary development; and consonant inventory size at 12 months correlates with phonological development at three years. Infants who are prolific babblers tend to do better on language measures in their early school years, while infants who are not prolific babblers have a tendency to have speech, language, and academic problems in their early school years. STOEL-GAMMON proposes two explanations for these linkages. The first is that the early, prolific babblers have "better" language faculties than infants who do not babble early. The second is that babbling *per se* facilitates and/or accelerates phonological development. STOEL-GAMMON does not choose between these alternatives, and, perhaps, there is no empirical difference between them (how could different individuals' language faculties be ranked other than on the basis of their performance?). However, simply assuming that early babbling facilitates subsequent language development begs the question of why some infants babble earlier than others do. In contrast, attributing crucial characteristics of babbling and later language development alike to the operation of a child's language faculty provides a potential link.

In a similar vein, BRADLEY (20) documents a relationship between the ability of prelinguistic children tested at age 4–5 to decompose monosyllabic words into onsets and rhymes (e.g., *cat* /k/ + /æt/) and their reading and spelling performance levels at age 8–9: children who are better at decomposing words at age four are better readers and spellers at age eight. In contrast, there is no correlation between early onset-rhyme decomposition and later mathematical ability. BRADLEY supplemented this study with a training study. She found that explicit training on onset-rhyme decomposition at age 4–5 led to improved performance on verbal measures at age 8–9. In a follow-up study conducted some years

later, 13 year olds who had received the training (nine years previously) outperformed the controls who had received no training.

Synthesizing BRADLEY's results with those summarized by STOEL-GAMMON and LOCKE AND PEARSON leads to a view of language development, broadly defined, in which children's language faculties vary. That is, not all children approach the task of language learning equally endowed. That humans differ in basic language skill should not be surprising. After all, we differ in musical and athletic skills. Nonetheless, BRADLEY's results suggest that the constraints imposed on language development by an individual's language faculty are not particularly stringent, as explicit phonological training has a significant and lasting effect. These results have strong public policy implications that are not really made explicit, although STOEL-GAMMON notes the clinical relevance of early identification of atypical development and consequent intervention. (For further discussion, see Oller, Eilers, Steffens, Lynch, and Urbano, 1994.)

Because of the clinical implications of studies documenting relationships between language measures at various stages, these studies cannot necessarily be assumed to have been motivated by specific theoretical considerations. Nonetheless, they are consistent with the theoretical assumption of a general continuity to language development, from babbling through adult proficiency. This *continuity assumption* is most closely associated with the work of Steven Pinker (e.g., Pinker, 1984). While the output of child and adult grammars is clearly different, Pinker suggests that the grammars themselves are not qualitatively different. Rather the output differences are the result of developmental differences in such factors as knowledge, computational procedures, and working memory. A crucial motivation for Pinker's continuity assumption comes from considerations of learnability. If developing grammars are qualitatively as well as quantitatively different from fully developed grammars, that would mean that a child would in the course of language learning sequentially acquire multiple, distinct grammars. Thus in the absence of compelling, independent evidence, the continuity assumption requires positing developing grammars that are only quantitatively distinct from fully developed grammars.

As MENN AND MATTHEI (7) note, the continuity assumption has achieved the status of dogma in child language research. In its strongest, most dogmatic form (a stronger form than Pinker's, to be sure), this assumption requires that developing grammars necessarily be qualitatively comparable to fully developed grammars. However, MENN AND MATTHEI take the position that adoption of the continuity assumption as a methodological principle imposes on researchers an obligation of psychological responsibility. Anyone who posits an immature system that differs from mature systems must also show how and why the transition takes place. As the discussion in PD of Menn's (1983) "Two Lexicon" Model indicates, such a demonstration can be anything but straightforward.

According to the Two Lexicon model, lexical representations for production and perception are distinct. Among the facts that this model was intended to account for are: (i) receptive vocabulary is, throughout the lifespan, much larger than active (production) vocabulary; (ii) young children's early words are often phonologically quite different from the adult models, which the children nonetheless understand perfectly well; (iii) some children who have difficulty producing particular sounds systematically avoid using words containing these sounds, suggesting that they have some kind of phonological representation incorporating the segments that they nonetheless cannot produce. On the

commonly held assumption that adult lexical entries incorporate a single phonological representation (e.g., Kenstowicz and Kisseberth, 1979; Lahiri and Marslen-Wilson, 1991; cf. Klatt, 1981), STEMBERGER (5) rejects the Two Lexicon model on the basis of the formal discontinuity that it putatively entails. He proposes instead a connectionist model of the lexicon. In this model, all words in active and passive vocabulary are represented in the lexicon, but only the active items have clear mappings from word to sound. In contrast, MENN AND MATTHEI note some facts about adult behavior not commonly incorporated into models of the mature lexicon (e.g., Klatt, 1981; cf. Lahiri and Marslen-Wilson, 1991). First of all, adults as well as children generally understand many more words than they actually use. Secondly, and more importantly, adult speakers can generally after some exposure understand speakers of different dialects than their own, although they are typically not very good at imitating other dialects (even though they may think they can!). As MENN AND MATTHEI note toward the end of chapter, in a section that is not clearly connected with what precedes it, these facts could no doubt be incorporated in a connectionist account like STEMBERGER's. The continuity assumption in any case is not a constraint on developing grammars. Rather, it is a constraint on the relationship between developing and fully developed grammars.

I dwell here on the Two Lexicon model not because it is correct (most of the facts that it is designed to account for can, I believe, better be handled in a model that takes into account children's motor skills; see Faber and Best (1994) for details), but because it nicely exemplifies research strategies dictated by acceptance of the continuity assumption. Looking for continuity between child and adult grammars leads to the identification of problems that must be handled somewhere in linguistic theory. Even if the Two Lexicon model is not appropriate, the difference between receptive and active vocabulary must be handled in an adequate model of the lexicon. Likewise an adequate account of speakers' linguistic competence must account for the extent to which they can understand speech varieties (other dialects, different registers, foreign accented speech, etc.) that they have no active control of.

A similar view of developmental continuity is evident in the chapters concerning the acquisition of phonological categories in one's first and second language, by WERKER AND PEGG (9), FLEGE (21), and WODE (22). While the exposition in these chapters is not explicitly framed in terms of the continuity assumption, the arguments are, in all three cases, consistent with it. The pretheoretic observation is simply this: Except under exceptional circumstances, all human children ultimately become competent users of the language spoken around them, without explicit instruction, while humans who learn a second language in adulthood generally do not achieve full proficiency, despite extensive instruction. That is, the result of first language learning is qualitatively different from the result of second language learning. In the past this discrepancy has been attributed to a so-called critical period, whereby language learning ability atrophies after a certain age (e.g., Lenneberg, 1967). Children who, through abuse or neglect, are not exposed to a first language within this critical period never acquire full linguistic skills. For ethical reasons, the role in language acquisition of exposure to language during a so-called critical period can only be explored through *post-hoc* case studies. There is simply no way of knowing how specific abused children would have developed had they not been abused. Even if it were methodologically appropriate to take the failure of children who are grossly abused in a particular way to learn a first language and to generalize from that to the failure of

normal adults to achieve proficiency in a second language, the fact remains that the critical period approach to language development involves positing a developmental discontinuity: Young children have a mechanism (or a set of mechanisms) that adults lack.

In contrast, the approaches to second language learning advocated by FLEGE and by WODE rely on one crucial difference between first and second language learners: Second language learners have already learned a first language. Their view that 'imperfect' second language learning is the result of interference from the first language depends crucially on findings that first language development takes longer than had previously been thought to be complete and on findings of first language interference on characteristics of a second language at a younger age than such interference had previously been observed at. WERKER AND PEGG begin with a summary of the results of approximately 15 years worth of research conducted both in Werker's lab and elsewhere: Young infants can discriminate nearly every contrast that they are tested on, whether the contrast is used in the ambient language or not; adults are much less likely than young infants to be able to discriminate non-native speech contrasts; the developmental change implicit in the first two points is evident within the infant's first year of life; and the change involves reorganization of perceptual biases rather than loss of initial capacities.

To summarize WERKER AND PEGG's model, infants younger than 10 months of age have available to them broad-based phonetic categories, whereas infants older than 10 months of age are beginning to develop an inventory of phonological categories used in the ambient language.[4] More recent work suggests that at least some language specific vowel categories may be acquired by six months of age; (Kuhl, Williams, Lacerda, Stevens, and Lindblom, 1992; Polka and Werker, 1994.) However, in the study described in this chapter, WERKER AND PEGG found that children younger than 19 months could not discriminate real words from nonsense words differing only in a single segment (e.g., *dog* vs. *bog*)[5], even though these same children could, presumably, discriminate nonsense *ba* from *da*. WERKER AND PEGG's interpretation is that only the older children have contrasting phonological categories. In particular, rather than the whole word representations posited by Ferguson (1986) and others, these results suggest that 19 month old children

---

[4] WERKER AND PEGG refer to the latter as "language specific phonetic categories", reserving the terms phonemic or phonological for phonological contrast. However, what they seem actually to be motivating is a conceptual distinction between acquisition of phonological categories and acquisition of structured, phonological contrast among these categories. See Faber, Di Paolo, and Best (ms) for further discussion.

[5] Many readers will interpret WERKER AND PEGG's nonsense example *bog* as the real word meaning 'marsh' or 'fen'. This word is presumably not in the receptive vocabulary of small children being raised in a relatively urban area. While many speakers of American English and most speakers of British English have different vowel nuclei in *dog* and *bog*, in virtually all varieties of Canadian English there is no phonological contrast between /ɑ/ and /ɔ/ (e.g., Chambers, 1992); thus *bog* and *dog* would indeed have the same vowel nucleus and hence be a legitimate minimal pair in Vancouver, where the study was conducted.

are beginning to construct lexical representations composed of smaller phonological units. However, it is not necessarily the case that they therefore have phonemes /b/ and /d/. Recall that preliterate four and five year old children can divide monosyllabic words into onsets and rhymes, but they cannot subdivide syllable rhymes further into their constituent phonemes (BRADLEY). It would thus be interesting to know whether children who can discriminate *dog* from *bog* can also discriminate *dog* from *dock* or from *dob*.

In any case, if it is (metalinguistic) awareness of phoneme-sized segments that facilitates learning to read, FLEGE programmatically extends this one step further, suggesting that a qualitative shift takes place in children's phonological representations around the time they are learning to read (similarly, Best, 1993), and that therefore children before and after this shift are equally metalinguistically aware of their (different) phonological representations. (WODE, inexplicably, places this shift to segmental representations around age two.) Earlier, their representations are composed of syllable chunks (onsets and rhymes), of demisyllables, or even of whole syllables, while later they are composed of phonemic segments.[6] This shift implies also a qualitative change in phonological categories and their relation to an abstract phonetic space. At the earlier stage, the phonological categories occupy a relatively smaller proportion of the overall phonetic space; and, both their prototypes (Kuhl, 1991) and their boundaries are relatively poorly defined. At the later stage, in contrast, the categories have expanded to fill a larger proportion of the overall phonetic space, and their prototypes and boundaries have become better defined.

Coupled with Flege's (1991) finding that Spanish speaking children who learn English prior to age six or seven end up with native voice onset time in stop consonants while children who learn English later do not (even if they do not have striking foreign accents), this model provides a plausible account of 'incomplete' second language acquisition.[7] In many respects, this model is converging with Best's Perceptual Assimilation Model (Best, McRoberts, and Sithole, 1988; Best, 1993, 1994). Because older children's phonological categories occupy a larger proportion of phonetic space than those of younger children, a second language sound that is in the younger children's

---

[6] A similar conclusion, based on very different evidence, is reached by Nittrouer, Studdert-Kennedy, and McGowan (1989). Based on acoustic analyses of productions of /s ʃ/ followed by different vowels, they found that young children have more coarticulation than adults. They infer from this that the children have a different articulatory organization than the adults do.

[7] WODE cites the case of an English speaking three year old child who after nine months in Germany had achieved German pronunciation remarkably like that of monolingual German children his age, except for some phonological interference from English, notably regarding the realization of German front, rounded vowels. FLEGE, in contrast, investigated the pronunciation of adults who had learned English as a second language in childhood. His early learners may well have had non-native VOTs when they first started learning English, just as the child WODE describes may well have ultimately achieved native German vowel pronunciation.

uncommitted phonetic space may fall within one of the older children's phonological categories (similarly, Polka and Werker, 1994). Thus the younger children are more likely to be able to create a new category. Further, because of the older children's better defined prototypes and boundaries, they are less likely than the younger children to notice if a second language sound is not a particularly good exemplar of a first language phonological category. The net effect here is that the achievements of each developmental stage impose a degree of constraint on what can be perceived or acquired subsequently. Just as an adult's first language phonology limits acquisition of a second language phonology, the infant's acquisition of at least some phonological categories of the ambient language constrains his or her ability to discriminate between some non-native sound pairs.

The picture of language acquisition presented in PD is of an underlying continuity to what appears superficially to be a series of discrete stages. The third general approach to the continuity issue in PD is to attempt to explain how this could be the case. KENT (2) and LOCKE AND PEARSON (3) take a primarily biological point of view. In particular, KENT attributes changes in vocal capacity in the first year of life to anatomical changes in the vocal tract. For instance, infants produce their first syllabic utterances with unambiguous vowel nuclei only after their larynx moves into its mature position, at *c*. six months of age. LOCKE AND PEARSON focus on the neurological substrate of phonological learning. They view phonological learning as the complex product of genetically-based disposition and environmental stimulation. They further correlate the onset of babbling with specific cerebral and neurological events related to lateralization. LINDBLOM (4) and MOHANAN (23) extend this approach. Not only does grammatical complexity in an individual arise from an interaction between that individual's genetic endowment and input from the environment, but language itself is a phylogenetic product of both biological and cultural evolution.

LINDBLOM's contribution is his demonstration that the phonological segments of a mature phonology may *emerge* in the process of lexical development (*emerge* here is a technical term, implying qualitative novelty and increased complexity). Grammatical complexity resides neither in the speaker's genetic endowment nor in the linguistic environment, but rather is the result of the interaction between the two. By way of illustration, LINDBLOM presents the results of a computer simulation (Lindblom, MacNeilage, and Studdert-Kennedy, 1984) operating on a phonetic space and a set of optimization criteria. The (mathematically defined) optimization criteria involve minimizing articulatory complexity and maximizing auditory discriminability. The phonetic space includes seven stop closures at places of articulation ranging from bilabial to uvular and 19 vowel configurations, and thus allows for 133 distinct syllables, or trajectories from consonant to vowel. The computer simulation was set up to select the optimal set of 15 syllables from the 133 possibilities, and was run 133 times, each time starting with a different syllable. The 15 most frequent syllables across all 133 optimal sets were: *bi bɛ ba bo bu; di dɛ da do du; ɟi ɟɛ ga go gɨ*. Even though the consonants *q ɖ ɢ* occur in the input phonetic space, they are unrepresented in the optimum syllable set. Likewise only six of the 19 vowel qualities are represented in the optimum syllable set.

Given the programmatic nature of this simulation, the recurrence of specific vowels and consonants is interesting. One might have expected an optimum 15 syllable inventory in which each consonant occurred twice (with one occurring a third time) and 15 out of the 19 vowel qualities occurred once. The 15 syllables that actually recurred can easily be *described* in terms of a phonological inventory of /b d g i e a o u/ and two phonological

rules, one fronting /g/ before a front vowel, and one unrounding and centralizing /u/ after /g/. They were originally *represented* as a set of trajectories through phonetic space that just happen to have some commonalities. However, reanalysis of the 15-syllable inventory as a phonology of two rules acting on eight segments is in some (perhaps undefinable) sense more economical. Thus as the syllable system 'evolved' the possibility of a phonological analysis arose. While this finding has clear theoretical significance, it is methodologically crucial as well. The fact that a speaker's lexicon can be described in terms of a particular phonological organization (inventory + rules) does not necessarily mean that that speaker's lexicon is in fact organized in that way. This distinction between description and organization is fundamental for child phonology research, since one class of research programs in child phonology involves investigating the form of children's phonological representations at particular stages and possible developmental changes in these representations.

For example, MACKEN (8) illustrates the form of argument necessary to impute segment-based phonological representations to small children. MACKEN argues that the various attempts of a single child to produce initial *Cr* clusters between one year, six months and two years, two months of age reflect inappropriate generalization of a phonological rule changing voiceless stops to *f* before *r*. At the first stage illustrated, the child produced *pr, tr, dr,* and *kr* clusters as voiceless aspirated stops.[8] At the second stage, *tr* and *dr,* but not *pr* or *kr,* were /f/. At the third stage, all four clusters were realized /f/, and at the fourth stage *pr* 'reverted' to [pʰ], but the other three clusters remained /f/. (At no point were *dr, gr,* or plain *r* affected, and *f* was never hypercorrected to [pʰ], or to anything else.)

Since MACKEN's attribution of phonological segments to young children depends crucially on her analysis of these forms as reflecting a particular sort of segmental phonological rule, a more detailed analysis is warranted. The analysis described above presumes that the child's *f* is the reflex of the stops in the clusters *pr, tr, dr, kr,* and MACKEN further assumes without comment that the child produces a fully rhotic *r* in other contexts and that the child's *f* reflecting an adult cluster is phonetically identical with *f* reflecting adult /f/. The latter assumption is, as Macken herself has observed (1980), untenable. In the absence of acoustic analysis of both sorts of *f*, the most that can be said is that the child's attempts at *Cr* clusters are perceived by adult speakers of English as /f/. As for the pronunciation of /r/, it is worth noting that /r/ is notorious for being one of the sounds that children acquire quite late, even after they have entered school. Thus it is unlikely (although not, of course, impossible) that MACKEN's child subject produced *r* in other contexts as [ɪ]. Rather, [ʋ], a voiced bilabial or labiodental approximant, is more likely, in light of the fact that 'misarticulated' /r/ is generally perceived as /w/. If a child who produced /r/ as [ʋ] were to attempt the voiceless [ɹ̥] of a *tr* cluster, the result would probably be [ɸ], a voiceless bilabial or labiodental approximant. Since an approximant constriction is, by definition (Catford, 1977), a constriction that produces turbulence with

---

[8]   MACKEN's Table 3 (p. 258) transcribes *drink* [t ɪŋkʰ], with a space for a (missing) diacritic, at T(ime)1, but her discussion strongly implies that for this child /d/ is, like /t/, voiceless aspirated word-initially.

voiceless phonation but not with voiced phonation, the voiceless approximant would be perceived by adult English-speakers as the fricative /f/.

If perceived /f/ reflects target /r/ in this child's productions, a phonological rule changing stops to /f/ before /r/ is not a possible account of the limited data MACKEN presents. Rather, the child is, like many others at this early stage of language development, simply not attempting initial *Cr* clusters (at least, not when the initial C is voiceless). At some points the child preserves the stop consonant, producing, for example, [tʰ]; at other points, the child preserves the /r/, producing [ɸ]. The child's particular substitution patterns may be unusual, but there is no reason to attribute them to a segmental phonological rule, with all the baggage that entails. If anything, the fact that target [r] and [ɾ] are treated differently argues against attributing to the child an adult-like phonological representation. Even so, the data MACKEN presents are consistent with representations that, like those of adult speakers, contain both the stop and [ɾ].

This case study is the only example MACKEN presents of her child phonology rule type 3 (rules that supposedly require imputing to the child segmental phonological organization), which, she claims, requires attributing to the child an adult-like segmental phonology. As illustrated here, this case can better be analyzed differently, calling into question both the existence of rule type 3 and MACKEN's attribution to the child of adult-like phonological representations. MACKEN further claims that rule type 3 is outside the scope of the type of self-organization LINDBLOM describes. If, as suggested here, rule type 3 does not exist (at least at the stage of language acquisition treated by MACKEN), this problem does not arise. But even if there were data sets that could best be described in terms of rules of type 3, it is not clear whether the putative incompatibility of these rules with principles of self-organization is a problem for the rule-based accounts or a problem for 'self-organizing' models of language acquisition. In any case, LINDBLOM motivates his self-organizing model with reference to phylogenetic, not ontogenetic, development of sound systems.

It is tempting to interpret LINDBLOM's findings as providing support for the proposition that phonological organization is only evident in children's lexicons after they reach a certain size. But this common sense proposition does not *per se* need support. The proposition itself, as stated here, leaves open two crucial questions: What sort of phonological organization? and, What size lexicon is large enough to 'require' the added complexity of phonological organization (of whatever sort)? Despite these caveats, there is no doubt that LINDBLOM's modeling demonstrates that seemingly vague notions like 'ease of articulation' and 'discriminability' can be made explicit in a way that leads to intuitively natural results.

McCUNE (10) illustrates a compatible approach. Implicit in models of dynamic or self-organizing systems is the notion that qualitative shifts (discontinuities) have precursors or triggers. So, a hypothetical extension of LINDBLOM's model to phonological development might see a precursor for the development of phonological organization *sensu stricto* in the prior increase in the size of the child's lexicon. Given a particular apparent discontinuity, it makes sense then to look for possible precursors. Following this logic, McCUNE focuses specifically on the babbling-early speech transition. Traditional research on this transition, as reviewed above, focuses on babbling as a (meaningless) phonetic precursor to speech. But (as it should be unnecessary to add) speech involves a mapping between sound and meaning, and what distinguishes babbling from early speech is precisely the addition of meaning. Use of words *qua* meaningful symbols requires that

the speaker/hearer has stable mental representations of objects and actions. Further, language is a communicative system, but it is not the only way that humans communicate. Facial expressions, posture, and non-linguistic vocalizations are available to speakers as well. Based on this conception of language, McCUNE examined transcripts and videotapes[9] of mother/child dyads for instances of representational play (e.g., pretending to feed a doll) and of communicative grunts, as well as early words and word-like utterances. All of the children studied engaged in complex representational play and used stylized communicative grunts before they achieved flexible word use (using the same word in a number of circumstances). On this basis, McCUNE suggests that communicative grunts serve as a *control parameter* for language. That is, in order to begin constructing the sound-meaning mappings associated with specific words, young children must develop the concept of communicating their intents and internal states, via stylized grunts. This communicative intent, when combined with the stable sound sequences of babbling and the stable representations evident in pretending, leads to language.

That the facts of language acquisition are relevant to linguistic theory is not a new observation. However it is one that bears repeating. In the aggregate, the papers in PD amply illustrate (despite the organizational flaws noted at the beginning of this review) that acquisitionally responsible linguistic theories have a lot of recent findings to consider. That is, if the continuity assumption is a reasonable one—as I think it is—linguists, regardless of research speciality, need to be aware of findings like those reported in PD. Unfortunately, both the cover price and the uneven nature of this specific collection preclude using PD as a textbook or a supplement for graduate level courses in language acquisition, although many of the chapters should undoubtedly appear on reading lists for these courses. Nonetheless, at $74.50 it is not unreasonably priced—by current standards for books of this sort—and thus can be recommended for university libraries and for the personal libraries of budding and established researchers in the field.

(Received May 6, 1994)

REFERENCES

BEST, C. T. (1993). Emergence of language-specific constraints in perception of non-native speech: A window on early phonological development. In B. de Boysson-Bardies, S. de Schonen, P. Jusczyk, P. MacNeilage, and J. Morton (Eds.), *Developmental Neurocognition: Speech and Face Processing in the First Year of Life* (pp. 289 – 304). Dordrecht: Kluwer.

BEST, C. T. (1994). The emergence of native-language phonological influences in infants' speech: A perceptual assimilation model. In J. C. Goodman and H. C. Nusbaum (Eds.), *The Development of Speech Perception: The Transition from Speech Sounds to Spoken Words* (pp. 167 – 224). Cambridge, MA: MIT Press.

---

[9] McCUNE does not specifically mention videotapes. She refers simply to home visits and "monthly tapes" that were phonetically described. However, it is difficult to see how representational play could have been reliably identified other than on the basis of videotapes.