Laboratory phonology 8 (review)

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Laboratory phonology 8. Ed. by LOUIS GOLDSTEIN, D. H. WHALEN, and CATHERINE T. BEST. (Phonology and phonetics 4-2.) Berlin: Mouton de Gruyter, 2006. Pp. xvi, 675. ISBN 9783110176780. $192 (Hb).

Reviewed by JAYE PADGETT, *University of California, Santa Cruz*

This collection of papers is the end-product of the eighth Conference on Laboratory Phonology (LabPhon), held in New Haven, Connecticut, in June 2002, and hosted by Yale University and Haskins Laboratories. The volume is dedicated to the memory of Catherine P. Browman. If the LabPhon conferences and volumes were a bit renegade when they began, they are now more of an institution. It was still unusual in the 1980s to combine phonological theorizing with experimental methods and with theories drawn from phonetics and psycholinguistics, but to do so now seems more the norm. Out of thirteen papers published in the 1989 issue of *Phonology*, three incorporate experimental methodologies. (I construe 'experimental' broadly to include, for example, gestural or neural network modeling and formal learning theory.) In 2009 it was nine out of thirteen. (Six of these were from a special issue called 'Phonological models and experimental data'; the reader can decide whether this strengthens or weakens the point.) The early 'labphon' movement can take credit for much of this change. This year we should see the inaugural publication of a laboratory phonology journal to replace the published volumes. In my view, this shift to a regular, peer-reviewed, and more accessible forum is very welcome.

The book contains twenty-six contributions (including four commentary pieces) and an introduction. It is divided into three sections (two of them further subdivided): 'Qualitative and variable faces of phonological competence', 'Sources of variation and their role in the acquisition of phonological competence', and 'Knowledge of language-specific organization of speech gestures'. I found these groupings to be nebulous; what comes through much more clearly is a second theme, on sign languages and comparisons between spoken and sign language. The deployment of laboratory methods and 'philosophy' in exploring sign languages is an exciting development. Other leitmotifs in the book draw on gestural phonology, exemplar modeling, acquisition, and the roles of abstract and categorical vs. concrete and gradient notions in representation and usage. Some of the papers are probably longer and less clearly written than they might be, but this is a minor complaint about a very interesting collection of works. Given space limitations here, I could not do justice to all twenty-six contributions; instead I focus on highlighting a few of them.

MIRJAM ERNESTUS and HARALD BAAYEN, in 'The functionality of incomplete neutralization in Dutch: The case of past-tense formation' (27–49), replicate, for one speaker, the finding in Warner et al. 2004, 2006 of incomplete neutralization (IN) of final devoicing in Dutch, based on a reading task involving nonce verb forms. (Unlike in Warner et al., the forms were not presented as minimal pairs.) Particularly interesting are the results of their perception experiments using the speaker's productions as stimuli. Ernestus and Baayen show that subjects not only detected IN but also used it to choose the appropriate past-tense ending (-*te* or -*de*) for the nonce verb stimulus forms, a task that requires the listener to infer the underlying voicing of the stem-final obstruent. The authors argue that IN, as well as their perception results, are due to the storage of lexical paradigms, among other things. Consider for example the form [vɛrvɛit] 'widen' and its infinitival form [vɛrvɛidⁿn]. Even if the former is stored in its surface form (contrary to the assumption of

most generative phonologists), both the production and the perception of its final consonant will be influenced by activation of the associated form [vɛrvɛid n] (see also Bybee 2001). IN has posed a serious problem for the traditional understanding of the phonology-phonetics relation, in which discrete phonology is transduced into continuous phonetics, because if /vɛrvɛid/ is categorically devoiced to [vɛrvɛit] by phonology, then phonetic implementation has no means of recovering underlying voicing in order to produce IN. The storage and use of entire paradigms circumvents this problem. Since not all words with underlyingly voiced obstruents will be equally supported by related forms of a paradigm (e.g. because they do not exist or are less frequent), this proposal about how IN occurs should be testably different from one that relies on access to orthography or the underlying representation.

In a comment article 'Dynamics in grammar: Comment on Ladd and Ernestus & Baayen' (51–79), ADAMANTIOS GAFOS compares two general views of the phonetics-phonology relation: the traditional view in which phonology precedes phonetics (as assumed by D. Robert Ladd in his contribution and by most phonologists) and one in which they exist simultaneously. Gafos favors the latter, and proposes an understanding of it in terms of nonlinear dynamical systems, which can elegantly combine qualitative and quantitative variation. (Gafos cites the work of Browman and Goldstein as an important precedent for this general idea.) To model IN of [voice], Gafos combines a 'grammar dynamics' that contributes a strong attractor (a familiar notion in nonlinear dynamics) to a [–voice] value (in the appropriate context) with an 'intentional dynamics' attractor that models intent to express a [+voice] value. (Gafos suggests that this intent might come from orthography, from lexical effects, or other factors.) The relative strength of these attractors depends on the settings of a continuous parameter. The hypothesis is that these attractors combine in a way that can model IN. Gafos notes an interesting property of the model: as the continuous parameter controlling the strength of the intentional dynamics is varied, the system shifts rather categorically from one of incomplete neutralization to one in which voicing is largely realized. (The latter corresponds to a state in which speakers make a point of pronouncing the spelling.) Generative phonologists have not frequently questioned the assumption that phonology and phonetics are built out of fundamentally different matter, since the difference between categorical and gradient behavior is so intrinsically compelling. But the idea that both quantitative and qualitative behavior can emerge from one system (because it has nonlinear properties) is a commonplace in the hard sciences. Gafos (57) seems to offer the dynamical systems approach to IN (and other facts) as a better alternative to exemplar-based approaches, because it can incorporate larger speech context (e.g. speaker intentions). Exemplar-based approaches, however, may be equally capable of doing this, for example, if exemplars are indexed according to the larger context in which they were produced or perceived.

A contribution by MATTHEW W. G. DYE and SHUI-I SHIH, 'Phonological priming in British Sign Language' (241–64), discusses results of a primed lexical-decision experiment that involves native and nonnative deaf signers of British Sign Language. The authors frame their work as an attempt to test predictions of the cohort theory of spoken word recognition (Marslen-Wilson & Welsh 1978) in the realm of sign language. One prediction is that lexical decisions will be facilitated by primes sharing phonological properties with the target word. Among other interesting outcomes, Dye and Shih found priming effects in the reaction times of native speakers, but only when prime and target shared both location and movement. (Handshape had no effect.) This result jibes with others suggesting that location and movement play a primary role in word recognition in sign language. Nonnative signers did not show this priming effect, and differed from native signers in other ways that lead Dye and Shih to suggest that they 'use a qualitatively different process' of lexical access (259). Equally interesting, the priming effect occurred only when both prime and target were real words. This shows, Dye and Shih suggest, that the effect occurs in the lexicon.

JAMES M. SCOBBIE, in 'Flexibility in the face of incompatible English VOT systems' (367–92), presents an interesting study of the distribution of voice onset time (VOT) in the /p/–/b/ contrast in Shetland Isles English speakers. Scobbie explores three classes of subjects according to the geographical origins of their parents: Shetland, (other) Scotland, and England. This is interesting because the contrast is described as prevoiced/short-lag (b/p) for vernacular Shetland but short-

lag/long-lag (p/pʰ) for the other relevant dialects. This means that learners in this community can be faced with highly variable and seemingly incompatible evidence about the VOT of /p/ and /b/, with short-lag [p] being ambiguous. Indeed, some of Scobbie's subjects seem to use primarily prevoiced /b/, others short-lag /b/, and some even seem to employ either in categorical variation. Values for /p/ do not seem to reflect a universal target; rather, speakers fall along a VOT continuum from short to long lag for this category. Scobbie finds evidence that the VOT target for /p/ depends on the rate of prevoicing of /b/ in a way that suggests maintenance of contrast. Reflecting on all of his results, Scobbie suggests that exemplar theory may provide a better means of modeling the data than innate categorical features: speakers share abstract phonological categories, but can differ gradiently and systematically in how they implement them. Since results such as these are potentially interesting for phonological theory, Scobbie argues that we should not always seek to minimize interspeaker variation in experiments.

RAJKA SMILJANI, in 'Early vs. late focus: Pitch-peak alignment in two dialects of Serbian and Croatian' (495–518), examines effects of narrow focus and boundary-tone crowding on the realization of pitch accent in Belgrade Serbian (BS) and Zagreb Croatian (ZC). BS has a contrast between L*+H 'rising' and L+H* 'falling' accents on initially stressed words; the difference is one of alignment only. In falling accent the rise is completed in the first syllable (as traditionally described, though Smiljani's data show that this need not be true), and pitch falls afterward. In rising accent the peak is reached in the posttonic syllable. ZC has a L+H accent but lacks the above contrast (at least for some speakers). Smiljani is interested in how the existence of the contrast in BS, as opposed to its absence in ZC, constrains the former's reponse to pressures exerted by narrow focus and competing boundary tones. Indeed, while sentence-initial narrow focus causes a leftward shift of pitch accents in ZC, the two BS accents get more different from each other in alignment. Both dialects avoid accent realization posttonically in final position, due to competition with a boundary tone; this leads to compression but not elimination of the difference between the pitch accents in BS. Smiljani takes her detailed results to support a distinction between tonal association and alignment as proposed by Ladd (1996).

Using electromagnetic midsagittal articulography (EMA) data, TAEHONG CHO, in 'Manifestation of prosodic structure in articulatory variation: Evidence from lip kinematics in English' (519–48), explores the effect of prosodic factors—H* pitch accent, boundary (word, intermediate phrase, intonational phrase), and position (post- vs. preboundary)—on lip movements in English. Putting aside some differences between lip opening and closing gestures, Cho finds, for example, that lip gestures are faster, longer, and spatially larger when accented. Both initial and final higher boundary positions generally lead to longer gestures. Discussing these results, Cho questions whether varying any single parameter of articulatory (gestural) phonology (AP), such as the stiffness parameter, target location, or intergestural timing, can account for the changes that occur under accent or at boundaries. Cho therefore raises the question whether AP is adequately equipped to model effects of prosody on kinematics.

Over the years the LabPhon volumes have upheld a standard of excellent quality in edited paper collections, due to the hard work of LabPhon conference organizers and editors, and perhaps also due to the lack of a journal devoted specifically to laboratory phonology. With the important set of papers that the volume contains, *Laboratory phonology 8* meets this standard.

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Language evolution and syntactic theory. By ANNA R. KINSELLA. Cambridge: Cambridge University Press, 2009. Pp. 222. ISBN 9780521895309. $99 (Hb).

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Language evolution and syntactic theory (*LEST*) is a welcome addition to the growing literature on the biological evolution of grammars. *LEST* is a revision of Kinsella's 2006 University of Edinburgh Ph.D. thesis, which was written under her former name of Anna R. Parker. Despite the title of the book, only one syntactic theory is given much attention—namely the MINIMALIST PROGRAM (MP). *LEST* argues that the MP, as developed by Noam Chomsky and his co-thinkers, violates numerous properties that any biological faculty needs in order to be evolvable. But in a sense that turns out not to matter, since K argues that the MP, when the full set of its devices is taken into account, is far less 'minimalist' than its supporters envisage. On the whole, I feel that K is successful in accomplishing her goals, though, as I note below, one might argue that she somewhat overstates her case.

Ch. 1, 'Constraining our theory of language' (1–38), assumes a basic knowledge of both evolutionary theory and syntactic theory. It begins with a quick overview of the MP and points out that this approach differs from its predecessor nativist theories in reducing (or appearing to reduce) the amount of innate knowledge. After outlining different classes of evolutionary accounts that might be applied to language evolution (adaptationist theories, exaptationist theories, spandrel theories, saltational theories, and self-organization theories), K observes that '[a] saltational account fits well with the minimalist style of argument for language, as a more minimal, more economical language faculty leaves less for evolution to have to explain, a single genetic mutation looking more reasonable as a consequence' (14). The remainder of the chapter traces the roots of the MP in prior 'Chomskyan' approaches and discusses the dynamics of an MP derivation, at least insofar as they were understood in the first few years of the twenty-first century.

I found Ch. 2, 'Language as a perfect system' (39–69), to be the most interesting in *LEST*. The defining feature of the MP research program is to be 'concerned with … determining the answers to … the question "How 'perfect' is language" ' (Chomsky 1995:221). K remarks on the difficulty of determining what Chomsky might have in mind by 'perfection', since there are various (incompatible) ways that the mapping between phonetic and logical form might be carried out in the most economical or efficient way possible. But whatever alternative is chosen:

> [t]he minimalist view of language is interesting from an evolutionary perspective for a number of reasons. It is unclear how a perfect system might arise in the course of evolution, or indeed, how a system might begin as imperfect and be fashioned in the course of evolution to become perfect. The perfection of the minimalist language faculty makes it appear unlike other biological systems, which are typically not considered perfect in any sense. (41)

But as K emphasizes here and later in *LEST*, the more economical character of the MP with respect to antecedent models is more apparent than real: 'In the MP, all of the considerations which fall outside of issues of economy are simply reformulated and appear as a complex inventory of features to be checked' (49). And even if an MP theory were perfect, optimal, or economical (K highlights the differences between the three), how could 'any of the three characteristics … possibly emerge on an evolutionary timescale through gradual adaptive processes. Does adaptive evolution give us perfect systems, optimal systems, or economic systems?' (58). K, of course, an-