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December 19, 2002

Bernhardt, B. H., & Stemberger, J. P. (1998). *Handbook of phonological development from the perspective of constraint-based non-linear phonology*. San Diego, CA: Academic Press.

Book Review

The 793-page volume from Bernhardt and Stemberger is an important contribution to developing a constraint-based theory of phonological acquisition. The authors' goal seems to be to work through aspects of Optimality Theory (McCarthy and Prince 1993; Prince and Smolensky 1993), which has largely focused on adult grammar alternations, to see to what degree its predictions fit with observed findings in the child language acquisition literature. Where the Optimality Theory literature has seemingly ignored developmental evidence, Bernhardt and Stemberger propose modifications to the theory to correct the gaps. In this review¹ I will point out some of Bernhardt and Stemberger's proposed revisions and their motivations for them.

The authors make clear from the outset that the book is intended for a wide audience, including phonologists and linguists, psychologists, and speech and language pathologists. Chapter 1 serves to frame the discussion for the rest of the book. The authors describe what the current discussions in the acquisition and phonology literature are, such as the continuity vs. discontinuity debate, representational phonological theories such as underspecification and feature geometry, and some basic known facts about phonological development phenomena. Bernhardt and Stemberger make clear that they intend to consider both child and adult data as equally valid in articulating their theory, including data from both normally-developing and disordered children. In their view, adult languages of the world might lack the full range of possible linguistic phenomena, and evidence from early development can fill in some of the typological gaps. One drawback of this assumption is that a continuous perspective on development may need to be assumed in order for early child data to be relevant to the view on constraint alternations².

In Chapter 2, Bernhardt and Stemberger go into further detail about the theoretical assumptions they make. They argue for hierarchical organization of phonological constituents (words are made up of prosodic feet, which are composed of syllables, which in turn are made up of segments, composed of specified features) and assume that sub-segmental features are the fundamental units of analysis (not segments, as assumed in OT), though a detailed discussion of the advantages and disadvantages of this assumption is not given. They adopt a feature geometry and tier analysis in which consonants and vowels lie in different tiers, allowing interactions such as vowel harmony (common in adult language) and consonant harmony (more restricted to child language) to be more easily explained. They understand that underspecification can in fact play a role in a constraint-based theory. Finally, they devote several pages to explaining why they feel using constraints only (as opposed to only processes or a combination of the two) is a

¹ I am aware of two previous reviews of this book (Pater, 1999; Lleó, 2001).

² I will elaborate on this more in the second half of the paper.

potentially fruitful line of research.

The need for interdisciplinary work is also discussed: phonological theorists are often ignorant of the research in language acquisition, and studies in child phonology need to ensure they are addressing crucial questions of theoretical import. The former reason is probably of more interest; many phonologists continue to ignore child data because it is not known how that information would impact research on a full-fledged adult system. The appeal is timely, coming when phonological theorists realize more and more that the question of acquisition is central and cannot be considered as an afterthought when developing a theory of language.

A final point made in Chapter 2 is that the authors do not necessarily consider universal constraints to be innate – the two issues are separable. Because humans develop similar constraints cross-linguistically, they argue, does not entail that these constraints must have existed a priori as part of a linguistic system. Bernhardt and Stemberger's constraint-based approach is functional rather than formal and grounded in articulatory, phonetic, and cognitive constraints that tend to be universal to humans, but are not necessarily innate. As I will discuss later, the initial ranking of constraints reflect phonetic grounding and restrictions on cognitive capacities. This approach makes their treatise more neutral and hence accessible to a wider audience, a wise decision in my opinion. By not making the issues of constraint-based analyses so bound to theory, their views and data on acquisition may receive wider attention.

The third chapter fleshes out in greater detail the relevant details about phonological theory not specific to a constraint-based approach. Of course, their ideas of representation are relevant, as many of their proposed constraints (discussed below) make reference to an articulatory utterance as a hierarchical organization of units, not simply a string of phonemes. After examining their discussion on tiers, adjacency, and timing units, it was clear that Bernhardt and Stemberger will not disregard much of the work done in the past several decades in phonology. Unlike some work done in OT, their approach will be a more unified attempt to integrate all known facts, rather than sacrificing good ideas in condensing everything into a single formal theory.

As the authors articulate the theoretical assumptions and the framework in which they will operate, it becomes clear that this could subtract from the value of the book as a reference of early child acquisition data. This becomes especially clear in Chapter 4 where Bernhardt and Stemberger finally lay out their version of a constraint-based theory. The most startling fact noticed immediately is the new names for constraints. In an attempt to make OT more transparent to non-linguists, the authors' eliminate what they believe to be opaque names such as "R-Cond" and "OCP". Curiously, however, they are replaced by constraints such as "Linked Upwards", the meaning of which is not immediately apparent and seems to be too intricately linked to a theoretical notion.

Bernhardt and Stemberger also feel that constraint names should reflect grounded conditions governing the output (phonetically grounded) or the relation between the input and the output (cognitively grounded). Hence constraints with process-oriented names are to be frowned upon in a non-linear approach. This is a noble goal, but to my knowledge the five years that have elapsed since the publication of this volume have not caused much of stir regarding constraint names. This may in part reflect the fact that while Optimality Theory is the most dominant idea in phonology today, many phenomena continue to be viewed from a process-oriented perspective. Ultimately, the

authors' continued insistence on changing terminology is very taxing on the reader. For example, in chapter 6 when discussing foot structure and its implications for word length requirements, the authors note that they avoid the term "sub-minimal" because it is an oxymoron. To suggest that phonologists do not realize this term to be oxymoronic is unfortunate; I think examples such as these reflect professional and political hurdles that must be overcome to promote the authors' stated goal of conducting more interdisciplinary research. As such, I feel that Bernhardt and Stemberger should have sought to take a more standard approach, using standard terminology, so that the reader could more easily grasp the key ideas of their proposal.

The changes to the constraint set are not just cosmetic, but substantial as well. The family of faithfulness constraints is changed to "Survived" constraints, which stipulate that features appearing in the input must survive in the output form. Survived constraints thus comprise a subset of Faithfulness constraints -- Survived constraints do not require that features appearing in the output be present in the input, which is typically done in OT by using DEP constraints to prevent insertion. Bernhardt and Stemberger believe that insertion is prevented by a family of constraints called Not, which mandate that an element must not appear in the output. However, McCarthy (2002) has argued that markedness constraints must distinguish between "old" and "new" violations. That is, it is relevant whether a marked structure appeared in the input in assessing violations. The authors' attempt to eliminate DEP-style constraints is justified on the grounds that superfluous constraints should not be proposed if other, independently motivated constraints can account for the patterns. However, in my view, this is an example of how many of the proposed modifications to Optimality Theory made by Bernhardt and Stemberger in this book have not been well-thought through. In this case, unfortunately, there is apparently no clear justification from child phonology for this proposed modification, only authors' independent views on the theory.

To their credit, Bernhardt and Stemberger do propose changes to Optimality Theory which seem to be grounded in facts concerning child phonology. McCarthy and Prince (1995) present an analysis of coalescence (or fusion) of two consonants through a segmental correspondence framework. However, as noted by Macken (1987; 1992), coalescence can arise as a apparent form of regression. The example given was that /pr/ was initially produced as [p] but later as [f]. If a child cannot produce consonant clusters, the attempt to produce a cluster might result in the fusion of the two target consonants, resulting in an output that reflects neither of the targets, let alone both of them. Bernhardt and Stemberger view this phenomenon as arising simply through the higher ranked of a constraint such as Survived(+continuant) in the process of development. Arguments and evidence such as these are important for linguists to know about and should have been stressed to a greater degree throughout the book.

The data-oriented analysis half of the book begins with chapter 5, where strategies concerning segmental development are discussed. Bernhardt and Stemberger generally question the traditional notions of Jakobson (1941) that say development proceeds from universally unmarked towards marked structures. Rather, the authors' take the stance that more frequent phones within a language may be early developing. Often it is the case that unmarked segments and frequent segments coincide. When a phoneme is not part of a child's repertoire, a substitution generally occurs, and chapter 5 deals with specific case studies concerning substitutions of place of articulation, manner, vowel

substitutions, and chain shifts³.

A common theme throughout the rest of the book for Bernhardt and Stemberger is the debate about what the initial constraint ranking should be, that is, whether structural/markedness constraints should dominate faithfulness constraints. Their message tends to be contradictory at times, as they seem to in principle agree with the idea that Structure constraints should initially dominate, except for several instances they found in which there is a reason for a faithfulness constraint to be high ranked. This leads to their claim that constraints should be initially “semi-random”. Unfortunately, semi-randomness, like many of the speculations in the book, is not clearly testable.

Chapter 9 is titled “Acquisition of Adult Alternations”, but does not offer any proposals as grand-sounding as its title. Rather the chapter discusses a few case studies (English, Hungarian, etc.) in which the child data were non-alternating, but then the adult system possessed extended allomorphy. The authors discuss what constraints might need to be reranked in order to produce the adult system, but they in the end simply tend to fall back on the idea of initial semi-random states to account for variability.

Chapter 10 summarizes the book and notes several areas that the authors failed to address, or where their analyses or predictions seemed to fall short. They argued that some things that they failed to account for, such as personality traits that could inhibit or promote phonological growth, were outside the breadth of a phonological system. Other problems they encountered, such as chain shifts or accounting for phenomena surrounding the low frequency fricatives, are problems for other theories as well.

However the authors failed to note other areas where they seemed to fall short in this book. To this reviewer, their undertaking was at times too broad and not clearly focused. They tentatively supported some ideas while wavering on others. If they could have condensed the volume down to the key arguments that supported their theory, it would have been helpful. At times, they stated all of their opinions on optimality theory, whether they were focused on child phonology or not. Their focus on large amounts of data is to be admired and followed, but I hope that didn’t sacrifice being able to look at the big picture. While this was in many ways a work in progress, what were the main points of their proposal?

To their credit, the authors take few aspects of optimality theory at face value. While they end up adopting many of the traditional notions of the field, it is not first without considering plausible alternatives (and then finding there are none). In the end, the book falls short because it tries to accomplish too much. On the one hand, it is serving as a reference work on the state of knowledge in child phonology, and on the other hand it tests all known knowledge in a new, unstable framework. These competing goals, combined with many stances that seem at odds with conventional attitudes within optimality theory, make the work seem scattered and somewhat divisive.

Extension to learnability

The question of whether a given phonological theory is learnable has been given more attention in recent years, and in some ways Optimality Theory grew out of linear theories of phonology that did not adequately address concerns of learnability.

³ I will devote greater coverage to these notions in the second half of the paper.

Obviously, since Bernhardt and Stemberger deal with the issue of acquisition, it would seem that learnability would be a central topic. Unfortunately, as learnability is often seen as a computational problem, Bernhardt and Stemberger only sketch details of how they view learning taking place.

The dominant notion of how learning takes place in Optimality Theory was articulated by Tesar and Smolensky (1996; 1998). In their view, markedness constraints are initially higher ranked than faithfulness constraints, which would correlate with the Jakobsonian view that early child productions are universally unmarked. According to Tesar and Smolensky, learning takes place by a process of constraint demotion whereby high ranked markedness constraints are demoted until the appropriate faithfulness constraints become active. By not allowing constraint promotion, Tesar and Smolensky hoped to constrain the range of possibilities in the grammar, allowing a child learner to more quickly hone in on the correct grammar. Through constraint demotion, Tesar and Smolensky are able to give proofs that the maximum number of utterances needed to determine a grammar is somewhere on the order of N^2 , where N is the number of constraints.

Bernhardt and Stemberger essentially agree with the major points of Tesar and Smolensky, but also distance themselves in many ways. They feel that the initial ranking of constraints is generally that markedness dominates faithfulness, but this is not exceptionless. They feel that a quasi-random initial ranking might explain variability in acquisition that seems to be found. However, they do not see any rampant variability, something that might be predicted by Tesar and Smolensky ranking all markedness constraints equally initially (hence the idea of a semi-random initial state). Also, Bernhardt and Stemberger see constraint promotion as being the primary way in which constraints are reranked, though constraint demotion is also a possibility. To allay formalist's fears that such a grammar would hence be unlearnable, they propose that children are able to only rerank the crucial constraints that caused their grammar to produce an incorrect output form. Since children have access to their own internal representations, they are "more intelligent" than a simple algorithm that searches blindly for the correct grammar by demoting (possibly incorrect) constraints. The primary evidence given by Bernhardt and Stemberger for constraint promotion is to account for simple regression phenomena, though they provide little support beyond this example.

A severe criticism in my mind that Tesar and Smolensky fail to address is exactly how a precise underlying representation is acquired. Tesar and Smolensky actually assume that underlying representations are available to the learner, and the process of reranking/demoting constraints is to make production agree with the postulated underlying form. They do not address how this underlying form came to exist, and we cannot simply assume that a two-lexicon model would account for such a precise, accurate representation. Bernhardt and Stemberger begin to address some of these concerns by assuming that the initial underlying representation is the same as the output form. (This then presumably assumes that children have adult-like representations). The underlying representation is constantly being updated in an effort to produce the correct surface form. However, one question that comes to mind – if the grammar is incorrect, is this arising as a result of incorrect constraint rankings, or incorrect underlying representations?

An effect of Tesar and Smolensky's algorithm that Bernhardt and Stemberger find

refreshing is its ability to naturally handle frequency effects in the input. More frequently occurring phones have greater distribution in the language, so early constraint rankings naturally tend to more optimal with respect to these phonemes. This side effect, which Bernhardt and Stemberger claim was not apparent to Tesar and Smolensky, can account for many of the frequency effects that we know are present in the input. For example, the finding that children are sensitive at a very early age to the phonotactic patterns of their native language (Jusczyk, Luce et al. 1994) is not surprising. Bernhardt and Stemberger do have the view that learning of constraint rankings can begin to take place at a very early age. In their opinion, evidence from babbling shows this (DeBoysson-Bardies and Vihman 1991; DeBoysson-Bardies, Vihman et al. 1992). They leave open the question, however, of whether constraint interactions can begin at any earlier stage, such as before birth.

Because of the emergence of OT as a dominant framework for phonology, some of the issues discussed by Kelly and Martin (1994) concerning whether language acquisition is driven by domain-general tasks seem to be no longer relevant. Within Optimality Theory, the idea of strong UG that language development does not use domain general cognitive abilities seems to have few supporters. While some view constraints to be innate, I would think there is little room for arguing that decisions on whether to demote a constraint in the learning process is done by language-specific means. Clearly humans and other creatures are sensitive to probabilities, as we are sensitive to many things in our surroundings. Within OT, however, this apparent sensitivity to probability is modeled simply as the tendency for the ranking of the constraints to be sharper with respect to more frequently occurring phenomenon. This frequency idea, however, may be at odds with Metsala's (1997) view that segmental development is a side effect of a desire to increase vocabulary size and that the initial lexicon is more sparse than a typical adult's. If we are to assume that frequency effects drive the order of acquisition, it would be strange to find that the child's lexicon is much sparser (or in any way much different) than a regular adult's, unless we found that child-directed speech is fundamentally different than normal adult speech with respect to neighborhood density and phonotactics. For example, if the phonotactics of shorter words are fundamentally different than those of longer words, we might be able to resolve the ideas of Metsala using the proposed learning process within a constraint-based framework.

As a footnote to this section on learnability, I want to point out that one traditional debate on learnability, that is, the role of negative and noisy evidence (Bohannon and Stanowicz 1988; Marcus 1993), did not receive any attention in Bernhard and Stemberger. One reason that this might have been the case was because the authors had nothing new to add to this debate. However, within a constraint-based framework the issue of negative evidence is also not quite as relevant as it perhaps once was. If you had previously been of the opinion that negative evidence played no role in acquisition, then you could feel justified that constraint demotion is argued only to be the result of the presentation of forms in the input and it is never done on the basis of lack of input. On the other hand, someone who argued for the existence of indirect negative evidence would find comfort in the fact that constraints do seem to be sensitive to probability – the absence of a particular alternation in the input is a clue that such an alternation does not exist. The learner can gain knowledge from this, assuming there is a constraint that

would be sensitive to this (lack of) information. However, whether noisy evidence could constitute either positive or negative evidence seems to be an open question, and it is a debate beyond the scope of the particulars of the constraint-based phonological framework.

Extension to representations

Bernhardt and Stemberger give an interesting discussion on the issue between a one-lexicon and two-lexicon model. Their argument seems to be sympathetic to the claims of Edwards, Fourakis et al. (1999) who suggest separating articulatory and acoustic representations are necessary. Basically, Bernhardt and Stemberger argue that a two-lexicon model is able to account for lexical variability and is able to account for the fact that on-line processing is not required for each production of an adult form. They see merit in the connectionist idea of a two-lexicon model that uses no underlying representations and assumes that mappings between the lexicons must be learned by adjusting activation levels. However most two-lexicon models, they suggest, are not psycholinguistically plausible. For their OT model they use underlying representations and a single lexicon. To me, their discussion was rather vague, and it seemed as though they wanted to have their cake and eat it too! In the end, they are going to take a single-lexicon approach, but want to argue that their ideas are not incompatible with a dual-lexicon notion. My interpretation of their idea is as follows: a language learner must continually test his or her production lexicon against the current ranking of the constraint set. The output is then compared with the production lexicon to see if there are any discrepancies. In OT, discrepancies are corrected by adjusting the constraint set (or one of the lexical representations?) and in connectionism the discrepancy is changed by altering the weight of a connection between the two representations.

Bernhardt and Stemberger adopt a theory that they term default underspecification, which seems very similar to radical underspecification (Archangeli 1988). Unlike radical underspecification, however, some default features may be specified, but default features cannot be specified in underlying representations. Using principles of underspecification and constraints on co-occurrence, they are able to provide explanation for many of the errors children produce. To me, their approach seems similar to those seen in Dinnsen (1993) and Gierut (1996).

Extension to change

Within the optimality theoretic framework, change in the sense of constraint reranking is generally quite discontinuous. Altering the constraint set produces across-the-board changes. To what degree these ideas interact with notions of non adult-like representations is not entirely clear. Dinnsen and Barlow (1998) give an account of a counter-feeding order chain shift such as /s/ → /ʔ/, /ʔ/ → /f/ that relies heavily on underspecification theory. Using underspecification, properties of lexical diffusion can be modeled so that across-the-board changes are minimized and continuity preserved. Clearly there have to be refinements to the initial representations posited in OT – assuming a detailed representation initially in OT still runs into the problem of explaining various production errors that are only later corrected.

Gierut (1995, 1996) discusses cyclicity in the development between supralaryngeal and laryngeal features and finds that phonological development proceeds in a cyclic process characterized by alternating phases of laryngeal and supralaryngeal elaboration. Gierut found that in treating phonologically delayed children, teaching out of phase resulted in significant expansion of the phonemic inventory. This information is relevant to Bernhardt and Stemberger because of their belief in widespread co-occurrence constraints, which mandate that certain features not be present simultaneously. The authors attempt to discard Gierut's findings by suggesting studies in which cyclicity effects were noticeably absent.

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