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Iggy Roca (ed.), *Derivations and constraints in phonology*. Oxford: Clarendon Press, 1997. Pp.xii+601.

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Most of the papers in this collection derive from presentations at a workshop at the University of Essex in 1995. Except for Chapter 17, "Non-transparent Constraint Effects in Gere: From Cycles to Derivations" by Carole Paradis, the constraint-based papers assume some version of Optimality Theory (OT). However, in the spirit of the unbridled chaos reigning in the phonological realm, it is possible to find among these eighteen papers enough combinations and permutations of both rules and constraints, and derivational and non-derivational systems to please the most theoretically promiscuous phonologist around. Given the youth of OT at the time of the workshop, one might expect the current volume to be already somewhat obsolete. This is not the case—OT has not solved the opacity problem in an insightful manner, there is still debate over whether separate strata of constraints are required, and the scourge of functionalism continues to stalk the phonological landscape. Several papers in the volume are noteworthy in that they transcend the declared theme of comparing rule-based and constraint based theories by offering theory-independent insights. Rather than discussing all the papers, I have chosen to comment in more detail on a few representative ones.

Iggy Roca's first chapter "Derivations or constraints, or derivations and constraints?" is an evenhanded overview of phonological theory including a thumbnail sketch of OT, rule-ordering and opacity, and how the remaining chapters address these issues. Nicholas Sherrard is not to be envied his job of providing in Chapter 2, "Questions of priorities: an introductory overview of optimality theory in phonology", an introduction to OT, with some of its brief history. This survey is based largely on unpublished (and thus unrefereed) work from the *Rutgers Optimality Archive* (<http://rucss.rutgers.edu/roa.html>). I recommend these first two chapters to anyone trying to get a handle on the field.

Chapter 14, "Correspondence and compositionality: The Ga-gyô variation in Japanese phonology" by Junko Itô & Armin Mester makes bold claims concerning the superiority of non-derivational OT over its competitors. To account for the complex distribution of [g] and [ŋ] in Japanese, the authors invoke a series of devices outside the domain of 'orthodox' OT, in which the grammar contains a fully ranked set of constraints which either demand input/output faithfulness or surface wellformedness. Itô & Mester posit an output-output correspondence constraint, IdentSS, which requires identity between *segments* in morphologically related surface forms. Unfortunately Itô & Mester do not define the segment, and it remains unclear whether both featural and segmental faithfulness constraints are necessary. The importance of developing an explicit account of the semantics of a notational system (e.g. What is a segment?) is discussed by Sylvain Bromberger and Morris Halle in Chapter 3, "The content of phonological signs".

In Itô & Mester's model, the second member of a compound is in a correspondence relation with the free form of the same morpheme, if such a free form exists (implying, thus, that free forms must be derived in parallel with bound forms—this issue is not discussed in the paper). Unfortunately the existing output-output analyses that Itô & Mester build on are fatally flawed. For example, the phase alternations of Rotuman are phonologically triggered, not governed by "syntactico-semantic" conditions as assumed by McCarthy 1996 (see Hale, Kissonock & Reiss, to appear, for critical discussion of the output-output correspondence literature). Furthermore, Itô & Mester's assumptions concerning the structure of the

Japanese lexicon and the use of diacritics to distinguish lexical strata have been shown to be untenable by Rice (1997) and by Sharon Inkelas, Orhan Orgun & Cheryl Zoll in Chapter 13 (see below).

Itô & Mester attempt to model apparent free-variation between [g] and [ŋ] in certain contexts. They do not entertain an extragrammatical explanation (register switching, etc.) but rather claim that the choice is "left open by the grammar" (432). Thus, their view precludes research into what actually does trigger the variation. Note also that by proposing a non-deterministic grammar, they reject without comment a well-established tradition of viewing grammars as functions mapping inputs to outputs, since the mathematical definition of a function requires that a given input have a *unique* output.

In order to allow the grammar its indeterminacy, Itô & Mester call upon two separate devices: multiple inputs and free ranking. The use of multiple inputs (underlying representations) has several problems including rendering obscure the term 'morpheme'. The multiple inputs (some with /g/ and some with /ŋ/) are, lo and behold! able to generate variation between surface [g] and [ŋ]. This position is not unreasonable, and it may even be correct (though it would help to have a theory of what determines the use of one version of the morpheme in a particular case), but the evaluation of competing theories loses credibility when they criticise a rule-based account for relying on the existence of underlying /ŋ/, since, they insist here, "there is no underlying /ŋ/ in Japanese" – the rule responsible for the distribution of [g] and [ŋ] is "(semi-) allophonic" [sic] (446). A coherent comparison to their OT proposal would obviously allow for the possibility of underlying /ŋ/ in a rule-based grammar.

Under free ranking, the constraint set is not fully ranked and outputs derived from any of the undetermined rankings are considered grammatical. Again, in the absence of a theory of how this randomization works, the idea of free ranking is too unconstrained to be interesting—is the variation not dependent on speaker or social context or elicitation method? This paper leaves so many questions unanswered, and so many assumptions unexplored, that it is impossible to accept it as the "streamlined OT analysis" (422) the authors promise.

Probably the most important contribution is Chapter 13, "The implications of lexical exceptions for the nature of grammar" by Inkelas, Orgun & Zoll. The claim that the paper demonstrates the superiority of OT over rule-based phonology is unfounded, but the authors raise a more important question: What is phonology a theory of? This paper might have been entitled "The resurrection of the null hypothesis" because it reminds us that whatever is idiosyncratic and unpredictable in the output of the phonology should be 'prespecified' in underlying representations. The authors argue convincingly against morpheme-specific co-phonologies to account for lexical exceptions to alternations and static patterns. In other words, phonologists should not waste time trying to model in the grammar what should be relegated to the lexicon.

It is interesting to note that this paper and the one by Ito & Mester both tout the superiority of OT, yet they are at odds in some of the most basic parts of their analyses. Ito & Mester disapprove of the use of underspecification, including a ternary distinction of binary features (+/-/Ø) to 'diacritically' prespecify underlying representations so that they 'come out right'. However, they are not averse to dividing the lexicon into various strata. Only OT, they argue, can incorporate these views in an insightful manner, and thus these assumptions lead them to proclaim the superiority of OT. Inkelas, Orgun & Zoll, in contrast, demonstrate the *necessity* of prespecification involving a ternary distinction of binary features, and they categorically reject the type of lexical stratification that Itô & Mester propose. They similarly invoke their results in support of OT.

In Chapter 4, "Expressing phonetic naturalness in phonology," Scott Meyers argues that the fact that so much of phonology can be "explained" in terms of phonetics means that we should derive the

phonological constraints from phonetic facts. Meyers' proposal fails to distinguish the set of computationally possible grammars from the more restricted subset of grammars that could possibly arise diachronically. In other words, Meyers' approach can never lead to a full understanding of phonology as computation. This approach has been criticized for cognitive science in general by Pylyshyn (1984:207): "the observed constraint on [a system's] behavior [may be] due not to its intrinsic capability but to what its states represent". If we are interested in studying the phonology 'computer' then we need to distinguish a *possible* phonological computation from an *impossible* one. The set of attested phonological patterns and their distribution may be somewhat skewed by the accidents of language change, as lucidly discussed by Juliette Blevins in Chapter 7, "Rules in optimality theory: two case studies". Real explanation of the nature of phonological computation requires us to see beyond such epiphenomena as 'markedness tendencies'.

A further problem arises when Meyers attempts to explain sound patterns in language by appealing to the competing forces of 'discriminability' and 'expenditure of effort'. Note that 'effort' is typically not defined in functionalist work, and even if it were, it is not clear that we should believe that humans strive to minimize their expenditure of effort—consider Don Juan, Mother Teresa, and Sir Edmund Hillary. An additional problem with such functionalist models (as well as other non-scientific theories based on a struggle between competing forces like Yin and Yang, Light and Darkness, Good and Evil, etc.) is that we can turn them around to contain principles like **obfuscate** (confuse the listener—instead of increase discriminability) and **no pain-no gain** (put out some effort—instead of minimize effort). We then find that these 'dysfunctionalist' principles generate the same types of grammars as the functionalist ones.

Meyers claims that certain types of behavior, such as better performance at hitting targets in careful speech, provide evidence that "phonological representation[s] are phonetic *targets*" (130). This confuses the competence/performance distinction, and it also entails a glib rejection of the possibility that grammar may be informationally encapsulated, as supposed under the modularity hypothesis. Speakers can do many things to disambiguate their message, even pronounce *knight* as [knaɪt] or point with their fingers, if they think it will help. There is no reason to believe, however, that this reflects access to their grammars. Furthermore, it is clear that phonology cannot be derived from phonetics, since phonetics relies on the logically prior categories of phonology in order to make any generalizations at all (Hammarberg 1976).

Meyers proposes that a "speaker's knowledge of what is difficult to produce or to perceive is directly incorporated into the grammar as criteria for the evaluation of potential phonological representations" (146). Consider an alternative account of how a learner figures out what patterns are present in the target language: s/he listens to it. This brings us to an evaluation of the oft-repeated claim that OT's universal constraint set aids the learner in the acquisition process. What advantage does a child have in learning the target language when innately endowed with a universal constraint like *[-bk, +rd] ('no front round vowels')? Born into an English-speaking environment, such a child will have this knowledge, but never be able to put it to any use at all since the primary linguistic data contains no front, rounded vowels. Born into a French-speaking environment, the child 'knows' again that front, rounded vowels are 'marked', but alas, s/he must be resigned to a life of simultaneous labial and coronal exertion. From the learner's point of view (as well as from the linguist's) a theory without a constraint that is either irrelevant (for English) or misleading (for French) is to be preferred. (I am grateful to Mark Hale, personal communication, for this argument.)

Chapter 5, "Gradient retreat", by Douglas Pulleyblank & William Turkel is committed to acquisition and learnability issues. The paper contains some interesting ideas concerning the modeling of acquisition in OT, but it suffers from fundamental problems. First, in considering a proposal that all Faithfulness constraints must outrank all Wellformedness constraints at the initial state of the grammar, they come to the invalid conclusion that "to assume that all inputs are identical to observed outputs is effectively to not

assume inputs at all" (164). Of course, what input-output identity really means is that there is no phonology, so to speak—no rules, or in OT terms, no Faithfulness constraints dominated by ‘competing’ Wellformedness constraints.

These authors also propagate an error found in (at least some of) their sources. They propose that children, *without learning any lexical items*, begin to distinguish "the acoustic properties that serve to contrast a given language's phonemes" (164). Given the definition of contrastive (e.g. phonemes or features are contrastive if they can signal a difference in meaning) this position is not coherent: "In general, it should be observed that ‘minimal pair’ is not an elementary notion. It cannot be defined in phonetic terms, but only in terms of a completed phonemic analysis" (Chomsky 1964:97; see also Hammarberg 1976).

Blevins’ paper and ‘r, hypercorrection, and the elsewhere condition’ by Morris Halle & William J. Idsardi both discuss McCarthy’s (1993) notorious OT account of [r] insertion in Eastern Massachusetts English. As McCarthy himself notes, "r is demonstrably not the default consonant in English" (189). That is, it is not the maximally unmarked consonant that an OT account predicts would emerge in such a situation. In order to account for the insertion of [r] McCarthy proposes a special *rule* of r-insertion: "a phonologically arbitrary stipulation, one that is outside the system of Optimality" (190). Halle & Idsardi point out that this solution "is equivalent to giving up on the whole enterprise" (337). Blevins is more accommodating. She first provides an excellent discussion of how such phonologically arbitrary processes arise diachronically. Since such phenomena are incompatible with a phonology restricted to a universal constraint set, she develops a version of OT that incorporates learned rules as a ‘periphery’ to the universal constraints at the ‘core’ of the grammar.

Blevins convincingly demonstrates that synchronic phonology needs to be able to construct arbitrary rules due to the accidents of history. However, she does not show that we need OT at all. She states that OT can account for "conspiracies, emergence of the unmarked, and the general property of languages to instantiate certain phonotactic targets again and again by distinct strategies" (228). Note that the first and third of these properties are basically the same—there may appear to be multiple mechanisms leading to a systematic distinction between the shape of underlying representations and the shape of surface forms. Given the vagueness of terms like "again and again" and "conspiracy" (how many rules make a conspiracy?), these two properties cannot be evaluated scientifically. The notion of ‘emergence of the unmarked’, especially in its application to child phonology, derives from a confusion of linguistic competence and performance, as well as from a failure to recognize that children’s early representations must be quite rich (see Hale and Reiss 1998). So a question arises: if we need language-specific rules, and we don’t have any strong motivation for constraints of the OT type (e.g. learnability considerations), and OT cannot (yet) generate opacity in a straightforward manner, then why should we entertain OT at all?

In line with the transcendent goal of delimiting phonological theory, Blevins provides a reanalysis of Prince & Smolensky’s (1993) account of vowel deletion in Lardil nominatives. She rejects their somewhat mysterious proposal that this process is a "slightly altered form of a universal phonological constraint" (228) *viz.*, "Word-final vowels must not be parsed (in the nominative)". Instead she adopts the reasonable view that Lardil has a language-specific morphological process of nominative formation. In addition to showing how this process arose via diachronic rule inversion, she addresses the importance of constraining OT, or at least being explicit in modeling the interaction of phonological and morphological processes/constraints. Note that Itô & Mester dub these central problems "some distracting technical complications" of their analysis (435, fn. 17).

OT has revived interest in phonology and especially in important areas like acquisition and learnability. However, many of the claims of superiority for OT are unfounded, and the comparisons to rule-based

systems are spurious. It is no surprise if a vague, unprincipled theory can "account for" data that a constrained theory cannot (though it is not clear that this is shown by any of the authors in this volume). We should compare explicit, well-constrained theories which use rules to explicit, well-constrained theories which use constraints.

One could argue that the apparent sociological success of OT reflects more a lack of intellectual integrity and rigor on the part of the phonological community than any conceptual or explanatory advantage of the theory itself. Clearly, we need to maintain scientific standards if change is going to be accompanied by progress. Of course, none of this criticism of OT is meant to imply that rule-based phonology is without its flaws—the question is whether OT is the right solution. There is a lot of interesting discussion and good linguistics in this book, some of it in the articles I did not mention, but any reader who finds the arguments for OT to be convincing should read more carefully.

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