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Review of Iggy Roca (ed.) (1997) Derivations and Constraints in Phonology

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Review

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

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In Optimality Theory (OT), a grammar is a language-particular ranking of universal constraints (Prince & Smolensky 1993).* There are two types of constraints: markedness constraints prohibit (or require) certain output configurations; and faithfulness constraints demand identity in input→output mappings. Phonological generalisations are expressed by the interaction, through ranking, of these constraints.

In classical generative phonology (CGP), a grammar is a language-particular ordering of rules (Chomsky & Halle 1968). Though the rules are also language-particular, they are constructed using universal abbreviatory devices subject to an evaluation metric. Phonological generalisations are expressed by the rules and their ordering.

OT and CGP differ profoundly in how they express phonological generalisations. Consider the kind of generalisation that gets the most attention from phonologists, the 'process'. In CGP, a process is directly encoded as a rule of the form A→B/C __ D. This rule describes an input configuration (the structural description CAD) and something to be done to it (the structural change A→B). In OT, however, there is no direct analogue of the CGP rule. The closest we can come is to use ranking to characterise a situation where the /A/→B input→output map is required in the C __ D output context: some markedness constraint *CAD dominates any faithfulness constraint F(A ↛ B) that would block the A→B mapping; no markedness constraint that CBD violates is ranked above *CAD; and for all X ≠ B, F(A ↛ B) is dominated by some faithfulness constraint F(A ↛ X) or some markedness constraint violated by CXD. The central claim of OT is that this complex decomposition of the seemingly primitive notion of a phonological 'process' is necessary to achieve real insight into phonological typology and to resolve vexed questions about conspiracies, blocking and triggering, learning, and other long-recognised problems.

According to the jacket blurb, *Derivations and constraints in phonology* (*DCP*) is intended to answer two questions: what is OT about, and how is OT superior

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to CGP (if at all)? These are reasonable goals, and one would expect them to lead to discussion along the lines of the preceding paragraph. But the essays in *DCP* do not address these questions broadly; instead, the majority focus on the much narrower question of whether OT needs to incorporate something like CGP rule ordering in order to account for phonological opacity. The central claim of OT, the ideas behind it and the results it brings receive only sporadic notice – in fact, 'typology' isn't even in the index. (Full disclosure: I was invited to contribute to this book and to participate in the workshop it is based on, but was unable to.)

The issue that *DCP* actually does focus on was first raised by Prince & Smolensky (1993: 79):

> Universal grammar must also provide a function Gen that admits the candidates to be evaluated. In the discussion above we have entertained two different conceptions of Gen. The first, closer to standard generative theory, is based on serial or derivational processing: some general procedure (Do-α) is allowed to make a certain single modification to the input, producing the candidate set of all possible outcomes of such modification. This is then evaluated; and the process continues with the output so determined... In the second, parallel-processing conception of Gen, all possible ultimate outputs are contemplated at once... Much of the analysis given in this book will be in the parallel mode, and some of the results will absolutely require it. But it is important to keep in mind that the serial/parallel distinction pertains to Gen and not to the issue of harmonic evaluation per se. It is an empirical question of no little interest how Gen is to be construed, and one to which the answer will become clear only as the characteristics of harmonic evaluation emerge in the context of detailed, full-scale, depth-plumbing, scholarly, and responsible analyses.

In other words, should OT be implemented with a serial or a parallel architecture? The basic problem faced by any phonological theory is that many linguistically significant generalisations are not surface-true (*pace* Hooper [Bybee] 1976, Vennemann 1974). CGP has a serial architecture because its rules necessarily express true generalisations, but only at the derivational instant when they apply. In OT, though, a constraint may fail to express a true generalisation because it is dominated. This difference invites, but does not require, a parallel implementation of OT.

The bulk of *DCP* consists of eleven essays that offer careful analyses and detailed argumentation bearing on the serial/parallel question. The essays deal with a wide range of languages and phenomena: Yokuts vowels (Archangeli & Suzuki), quantity in Gilbertese and truncation in Lardil (Blevins), Dutch prosody (Booij), Berber syllables (Clements), Boston *r* (Halle & Idsardi), Hebrew spirantisation (Idsardi), Japanese [g]/[ŋ] allophony (Itô & Mester), stress and syncope in Macushi Carib and Southeastern Tepehuan (Kager), accentuation and syllabification in Attic Greek (Noyer), Gere vowels (Paradis) and voicing in Polish (Rubach). Conceptually, these papers form six loose groupings:

- Booij and Clements propose a kind of fusion of OT and Lexical Phonology (LP), which I will call 'Stratal OT'. The idea is that each iteration of Gen approximates an LP stratum (or cycle), as has also been suggested in various other works (Cohn & McCarthy 1994, Hale & Kissock 1998, Hale *et al.* 1998, Kenstowicz 1995, Kiparsky 1997a, b, 1998, McCarthy & Prince 1993, Potter 1994). This move accommodates a much more limited range of serial interactions than CGP or classical LP, but more than parallel OT.

- Noyer, Paradis and Rubach also discuss Stratal OT, but more sceptically. In the cases they examine, they do not find evidence that the strata required to treat opacity are independently motivated.
- Halle & Idsardi conclude their discussion of Boston *r* with the statement that 'an OT account... encounters a number of non-trivial difficulties, and it remains to be shown that it is possible to construct an OT account... that not only grinds out the facts but also explains the reasons for their existence' (p. 347).
- Blevins presents a version of 'harmonic serialism', the less familiar serial OT implementation described in the first part of the quotation above (also see Black 1993, Prince & Smolensky 1993: 15–20, 79–80, and cf. Goldsmith 1990, 1993a). Blevins goes on to argue for the inclusion of language-particular rules in this system.
- Itô & Mester demonstrate that a parallel OT analysis of Japanese [g]/[ŋ] allophony, incorporating faithfulness constraints between output forms, is conceptually and descriptively superior to the best that can be done in LP. And Kager shows that the apparent derivational opacity of stress-conditioned syncope is illusory, once the underlying phonology is fully grasped.
- Archangeli & Suzuki analyse the well-known and some not so well-known opaque interactions in Yokuts using two-level constraints (cf. Cole & Kisseberth 1995, Karttunen 1993, Koskenniemi 1983, Lakoff 1993, McCarthy 1996, Orgun 1996), while Idsardi's paper criticises a two-level analysis of Hebrew.

There is, then, a diversity of opinion, though relatively little dialogue (as usual in anthologies). The editor has inserted cross-references to highlight areas of conflict.

With important exceptions, most of these papers follow a common expositional strategy: some data are presented, they are analysed with CGP rules and a derivation, and then a translation into parallel OT is attempted but ultimately fails. After a while, this drum-beat cannot help but convey an impression of tendentiousness – especially in a context where there is insufficient discussion of the conceptual basis for and results of OT.

A more serious problem, though, is the frequent lack of follow-through in the arguments and the conclusions based on them. There is little progress or value in rejecting one theory unless some effort is made to glean insights that might be used to improve another. How does CGP address the fundamental questions of phonological theory, such as typology, conspiracies, blocking and triggering, and learning? How should readers interpret silence on these matters? It is as if CGP were a mute and immutable null hypothesis.

Clearly, though, CGP is not immutable. In fact, it was recognised long before OT came along that the rich CGP apparatus of rules and derivations achieves broad descriptive coverage at the expense of explanation. For instance, research in autosegmental and metrical phonology (such as Archangeli & Pulleyblank 1994, Goldsmith 1976 or Prince 1983) has looked at the ways in which a richer theory of representations and constraints on representations can simultaneously produce better explanations and a more restrictive theory of rules and derivations. Again, though, *DCP* does not contain much reflection on these later developments of CGP and their relationship to OT.

Given the prominence of the Stratal OT theme, more discussion of the

broader implications of that proposal would have been welcome also. How do Stratal and Parallel OT differ in their predictions, and what kinds of data would, in principle, decide between them? Adding strata improves descriptive coverage, but what penalty, if any, does this move exact in restrictiveness, typology, learning and other goals of phonological theory? Do the other core ideas of Lexical Phonology – Structure Preservation, Strict Cyclicity, Bracket Erasure and the Elsewhere Condition – fit into this revised theory, or have they been discarded? Obviously, it is not possible to cover all of these issues without making a long book even longer, but it would be good to see questions like these asked, if not answered.

DCP begins with two introductory chapters. Roca's introduction handily lays out the issues that will be addressed and summarises most of the articles, complete with data and tableaux. He does a good job of explaining the opacity problem; although many others have noted that opacity presents difficulties for parallel OT, he is the first to move beyond anecdote to a general delineation of the issue. It is, however, incorrect to say that 'the derivational effects of counterfeeding and counterbleeding relations are mathematically inexpressible in Optimality Theory' (p. 8). The problem is that there are two kinds of counterfeeding relations, depending on whether the focus or context is affected (McCarthy, to appear).

Sherrard's introduction is potentially useful as a technical overview of OT, but it does not give a good sense of the key ideas behind OT or the main insights derived from it. There is a similar problem with Roca's introduction, which simply lists, without explanation, seven putative advantages of OT in the space of half a page. Readers asking how and why OT obtains its principal results will have to hunt around for relevant discussion scattered among some of the papers; those by Clements, Itô & Mester, Kager and Rubach are a good place to start.

Part II of the book, 'Theoretical investigations', contains three chapters on topics other than the serial/parallel question. Two of them are about issues that OT has put back on the agenda after long neglect, naturalness (Myers) and learnability (Pulleyblank & Turkel). The third chapter in Part II, by Bromberger & Halle, consists primarily of an argument that OT and CGP (which they call DT) are 'incommensurable', but it winds up with concerns about the procedure for getting to the optimal candidate. The key question, according to them, is 'what is the sequence of the stages traversed... by a speaker in the course of producing utterances...?' (p. 118). The answer, at least as far as CGP is concerned, is supposed to be self-evident: the derivation is something that really happens in the speaker's brain. OT has nothing comparable to say (though cf. Tesar 1995a, b, 1998), and so, they conclude, 'That is a good reason for pursuing DT rather than OT. And since a good reason for pursuing DT rather than OT is also a good reason for accepting the presumptions of DT over the presumptions of OT, it is a good reason for deciding the debate between them immediately in favor of DT without waiting to see which one is likely to accumulate the largest number of generalizations' (p. 119).

The rhetoric of this passage seems a bit overblown. Worse yet, there is considerable doubt that CGP derivations could possibly happen on-line in speakers' brains (cf. Goldsmith 1993b), and there is a further problem, which Chomsky has often inveighed against:

> To avoid what has been a continuing misunderstanding, it is perhaps worth while to reiterate that a generative grammar is not a model for a speaker or a hearer... When we say that a sentence has a certain derivation with respect to a particular generative grammar, we say nothing about how the speaker or hearer might proceed, in some practical or efficient way, to construct such a derivation.
>
> (Chomsky 1965: 9)

A grammar is a function from, say, underlying to surface representations; it is not a procedure for computing that function nor is it a description of how speakers actually go about computing that function.

Part III, 'Empirical studies', is the heart of *DCP*, containing the eleven essays described above, plus two others that are about the lexicon. Inkelas *et al.* argue that lexical exceptions should be treated by making a [+F]/[−F]/[∅F] distinction in underlying representation rather than with diacritic rule features or imaginable OT equivalents. They then recruit this result in support of a further argument that OT is superior to CGP because only OT can distinguish feature-changing from feature-filling processes. Hammond takes the position that there are no underlying representations, only constraints descriptive of the phonological properties of individual morphemes.

Some further issues arose during my reading of Part III; here are the most important:

- In Sherrard's introduction, the text on p. 47 might be read as implying that undominated constraints have a special status as 'inviolable'. Though undominated constraints are usually unviolated, they are in principle violable and will be violated in fact if no obedient candidate is supplied by Gen. The text goes on to say that 'parallelism lies at the heart of OT's distinctiveness and affords it much of its strength' (p. 48). This statement should be qualified. Parallelism in the sense used by Prince & Smolensky in the quotation above – that is, a one-step derivation – is not intrinsic to the theory. But parallelism in a weaker sense – all constraints acting together to evaluate candidates – is indeed distinctive to OT.
- Archangeli & Suzuki tangentially remark, citing a suggestion by Kevin Russell, that since Correspondence Theory (CT – McCarthy & Prince 1995, 1999) 'allows restrictions on the output, on input–output relations, and on output–input relations... why not allow them on the input as well?' (p. 207). CT is really a theory of faithfulness relations (the 'correspondence relation'), so it has nothing to say about whether there are constraints on inputs or outputs. There are, of course, constraints on outputs (the markedness constraints), but standardly in OT, restrictions on inputs are useless because there is no way to satisfy them, since (except in learning) there are no input 'candidates' to choose from.
- Halle & Idsardi's proposed modification of the Elsewhere Condition does not work for the Boston *r* case that they discuss, once all the data are considered. The argument is too complex to summarise here, but it is developed in an appendix to this review which can be downloaded from http://www-unix.oit.umass.edu/~jjmccart/appendix.pdf.
- Noyer writes that 'any OT analysis incorporating intermediate levels, if it is to be of any interest, should acknowledge that intermediate levels have a privileged status in UG' (p. 501). By this he means that Stratal OT has a claim on our attention, but harmonic serialism (as in Blevins' contribution) does not. I do not understand the basis for this *a priori* judgement.

- Paradis (pp. 541, 545) expresses scepticism about the idea of distinguishing separate PARSE constraints for segments and for tones. But this distinction seems uncontroversial, in light of autosegmental theory; moreover, the distinction was already made by Prince & Smolensky (1993: 212) and it is a commonplace of CT, which has superseded the PARSE/FILL model of early OT work.
- Rubach discusses an analysis of Polish in which the output of one stratum includes prosodically unparsed segments that get parsed in the next stratum. He too uses the PARSE/FILL model, arguing that CT is incompatible with this analysis because CT entails that 'unparsed segments do not exist, because all unparsed information is immediately deleted' (p. 574). In fact, CT says nothing about whether there can or cannot be prosodically unparsed segments in outputs. In CT, non-parsing is decoupled from deletion, actually permitting the kind of analysis it is here said to reject.

To sum up, *Derivations and constraints in phonology* contains much of value on the relationship between phonological opacity and serial *vs.* parallel derivation. But, despite its stated goals, it is less successful as a resource for learning about OT, and it does not compare OT and classical generative phonology across the full range of issues in phonological theory.

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