

Phonological dialectics

A short history of generative phonology

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1. Introduction

The organization and content of this article is as follows. In Section 2, the largest and main section of this article, I discuss the development of generative phonology. My theme is that the focus of attention has been shifting between representational and derivational issues. I will discuss the derivational debates in the seventies (rule ordering and rule formats), the representational explosion in the eighties (multilinearity), and the derivational denial in the nineties. In Section 3, I conclude that it is now time to turn back to representational concerns.

2. Three decades of generative phonology

2.1 The general form of a phonological theory

Phonological theorizing (like many other human activities) seems to follow a dialectic pattern such that the attention and creativity of those involved shift back and forth between what are the two major aspects of any phonological theory: *representations* and *derivations*:

- (1) F (Input) = output

The function F forms the derivational aspect of the theory, whereas both input and output form representational levels, which as such require a basic vocabulary and a combinatorial system.

of the function resulted from two factors. Firstly, since few restrictions were imposed on the *rule format*, the rules could get quite complicated, especially since various notational conventions allowed collapsing seemingly independent rules (*landscape* printing or unfolding pages were not uncommon in those days). Secondly, since rules could be extrinsically ordered and no restrictions were imposed on the distance between input and output, derivations could get quite long.

It can be said that the explanatory goal of SPE was to relate as many surface forms as possible, where 'relating' means 'deriving from the same input form'. Hence, with rules that could *do* anything and input forms that could be anything, only 'poverty of imagination' stood in the way of deriving *paternal* (minus suffix) and *father* from the same input source (cf. Lightner 1972). The absence of a morphological theory that could place *semantic* limits on the notation of relatedness stimulated the creative quest for 'common' sources. Clearly, with so much freedom, chances to arrive at real explanatory accounts diminished in inverse proportion to the depth of the derivations that were proudly proposed.

2.2 Derivational issues predominate the seventies

It did not take long before articulated criticism and alternatives were launched. Indeed, the seventies showed lively debates concerning the *derivational side* of the SPE-model. Both the freedom of rule format and the absence of restrictions on the derivational distance between input and output came under attack. The derivational distance could be tackled in two ways (together or independently). On the one hand, one might propose constraints on the distance between input and output. From the outset it had been suggested that this distance had to be *as small as possible*; this was called 'the naturalness condition' (Postal 1968), but that in itself embodied no specific limitation. More tangible restrictions involved the idea that the input needed to be identical to some of its outputs, or at least be composed of segments that occur in its outputs. Such proposals were essentially attempts to formulate the 'naturalness condition' in more precise terms (cf. Kiparsky 1968, 1973; Vennemann 1971, 1974). The second, perhaps more direct, approach toward reducing the input/output distance was to eliminate the notion of extrinsic rule ordering, or to reduce ordering to cases that would follow from general principles (Koutsoudas, Noll, & Sanders 1974).

The problem with rules was, of course, that no one knew what a 'possible rule' was. Do rules have to be '(phonetically) natural' and if so, what does that mean? And if not, what sets the limit? In the absence of knowing, or agreeing

Anderson's (1985) discussion of phonology in the twentieth century also views this broader development as one that involves a focus on theories of rules and theories of representations which he construes as resulting from a focus on *grammar* (as a mental construct) or on language (as a collection of sentences), respectively. Perhaps today, that opposition would be construed as one between I(ternal) language and E(xternal) language. I most certainly refer to Anderson's insightful book for a broader historical perspective (which covers generative phonology up to the early eighties); also see Fisher-Jørgensen (1975). However, as I will show, within the development of generative phonology, which as a whole is a theory of I-language or the *internal phonological system*, one can see a dialectic shift between a focus on this internal system as a *procedural rule system* and a *declarative representational system*. Both systems consist of a set of 'statements', typically called rules and constraints, respectively, although use of the term 'rule' (or statements that contain arrows) does not in itself imply a commitment to a procedural model (cf. Mohanan 2000). The important distinction lies in reference to intermediate levels (levels between the input and output) and the concomitant use of extrinsic rule ordering.

The phonological theory developed in the early days of generative phonology, culminating in Chomsky and Halle's (1968) *The Sound Pattern of English* (SPE) focused mostly on the derivational aspect. With respect to the representational assumption, SPE's theory was deliberately minimal. There is, of course, nothing wrong with minimalism if it simply refers to the attempt to get away with the fewest number of primitives and mechanisms. Hence, Chomsky and Halle tried to get away with a view of representations that involved a linear arrangement of phonological segments, characterized as unordered, unstructured sets of a finite set of binary-valued features, a minimal view indeed. It must be said that Chomsky and Halle devote considerable attention to the development of their feature system, as well as to the expression of the markedness of feature values. These are largely representational issues, which shows that my characterization of the successive stages of generative phonology is somewhat 'idealized'. In SPE, the function F was, on the other hand, highly complex, although, on the other hand, also minimal. The basic unit of F was the *phonological rule*, an operation that could change properties of the input. For simplicity's sake, let us say that the changes did not affect the vocabulary or combinatorial system as such (although it was argued that certain *plus* values (like for the feature [stress]) could be changed into *n*-ary values). The simplicity of the function F lay in the fact that there was only one type of mechanism in it, viz. the phonological rule, which was essentially a transformational rule, like the so-called transformations in syntactic theory. The complexity

on answers to these questions, one could at least try to exclude certain types of rules. One approach was to compare the structural description and change of the rules to the output of the whole derivation and require that the output does not contain positive exceptions (apparent overapplication) or negative exceptions (apparent underapplication). By excluding such situations (involving what came to be called *opacity*; cf. Kiparsky 1973), in other words by requiring that rules express 'true generalizations' (cf. Hooper 1976), extrinsic rule ordering was effectively rendered unnecessary.

Thus, several alternatives to the SPE-model arose, all differing in terms of the kinds of restrictions that were imposed on the derivational side of the overall theory. The most far-reaching rejection of SPE were, to my mind, *Natural Generative Phonology* (Hooper 1976; cf. van der Hulst 1978) and Stampe's *Natural Phonology* (1973), both of which restricted phonology proper to automatic processes.

Then, morphology kicked in and one idea that came up was to formalize an apparent distinction between 'inner' (Class I) and 'outer' (Class II) affixes or word formation processes (cf. Siegel 1974; Allen 1978). The empirical basis for this idea did not remain undisputed (cf. Aronoff 1976), but it caught on and influenced phonologists to cut up the phonological derivation accordingly, leading to the so-called *Lexical Phonology* model (cf. Kiparsky 1982). This model offered, in part, a solution to a problem that many critics of the SPE-model had advanced concerning its treatment of phonological alternations that do not reflect 'surface generalizations' but rather co-occur with specific (classes of) morphological affixes or processes. With its 'one rule format' minimalism, SPE had lumped together rules that accounted for surface generalizations (sometimes called 'automatic processes', whether allophonic or neutralizing) and rules that accounted for so-called *morphophonological* alternations, here referring to alternations that are limited to specific morphological contexts (cf. Dressler 1978 for many examples; Spencer 1998 for an overview).

Approaches that aimed at eliminating extrinsic rule ordering or its result, opacity, had effectively kicked morphophonology out of phonology, leaving an account of the relevant alternations to the lexicon (in case no 'real' morphology was involved) or to the morphological component (cf. Vennemann 1974; Hudson 1974; Hooper 1976; Strauss 1982). Recognizing a separate class of morphophonological rules (sometimes also called *morpho-lexical rules*, Anderson 1976, or *allomorphy rules*, Aronoff 1974) did not always result from a rigid ban on extrinsic rule order or opacity. Some researchers simply felt that recognition of such a class was called for, irrespective of the exact workings of the remaining part of the phonological component (cf. Anderson 1976). Ef-

fectively, proposals to cut up the derivation into a morphophonological part, a 'phonological' and perhaps even a 'phonetic' part help to shape the overall theory into the direction that was then also taken by the Lexical Phonology model (cf. supra) with its distinctions between level I and level II phonology of the lexical phonology, and the distinction between lexical and post-lexical phonology. A substantial part of the former cases of extrinsic rule ordering were taken care of by the extrinsic ordering of (*sub*)components.

All the above developments were largely driven by the desire to arrive at better accounts of the observed data (especially alternations) and not necessarily by a concern with 'psychological reality'. It had not escaped people's attention that SPE-type accounts using extrinsic rule ordering had a tendency to reflect the *historical development* of languages. Heuristically, it actually paid to consider the historical phonology of languages (if known) before embarking on a synchronic analysis, although it was also understood that the synchronic account could not be motivated on the basis of historical arguments. However, the analysis was ultimately an attempt to tackle the logical problem of language acquisition and not to reconstruct the historical development. The input and derivation had to be justified on internal grounds plus whatever principles and mechanisms UG was making available to the child (which, presumably, did not include introductions to the historical development of the language). Thus, phonological analysis was supposed to be 'psychologically real', i.e. there was supposed to be some sort of relationship between the elements of the analysis and what was actually happening in the mind of (idealized!) native speakers.

This mentalistic side of the SPE-model was also criticized in its own right, quite independently of the kinds of formal considerations that we discussed above. Linell (1978) offers both an overview of and a contribution to this line of work. Overall, *experimental* work offered little confirmation of the 'history-recapitulating' aspects of SPE-analyses that the more concrete approaches (such as *Natural Generative Phonology*) were also seeking to dismantle. In other words, experimental research and concerns with the restrictiveness of theory were going in the same direction.

2.3 Representational issues predominate the eighties

Meanwhile, 'back at the fort' (i.e. MIT), things were not standing still. Proponents of the SPE-approach, having channeled some of the discussions concerning rule ordering and depth of derivations into the development of lexical phonology (Kiparsky 1982), had shifted their attention to the *representational* properties of the SPE-model. As of the mid-late seventies and continuing dur-

ing a good deal of the eighties, an explosion of new ideas concerning various aspects of phonological representations started dominating the phonological scene (cf. van der Hulst & Smith 1982a). The incentive for some of these developments came from the rejection of SPE's ban on syllable structure (Vennemann 1971; Fudge 1969), as well as from pre-SPE models that had argued for parallel or syntagmatic organization in phonological representations, alongside vertical, syntagmatic organization (cf. Firth's Prosodic Analysis, Firth 1948; Harris's long components, Harris 1948). In some cases isolated individuals had independently developed similar ideas (cf. Rischel 1972 concerning the representation of stress).

The minimalist idea of a phonological representation being a strict linear arrangement of unordered feature bundles gave way to a rich view of representations in which segments sprouted into hierarchically arranged tiers and association lines, leaving 'a pair of empty brackets' (represented as Cs and Vs or Xs on a skeletal tier) as a trace of the linear segment string. The skeletal units not only functioned as anchor points for features on their tiers, but also themselves formed the starting point of a hierarchical organization, starting with syllabic (and subsyllabic) groupings, and resulting (via feet and groupings of feet) into various higher level categories such as 'prosodic words', 'prosodic phrases', 'intonation phrases' and so on (cf. Nespor & Vogel 1986). Representations became (very) multilinear; cf. van der Hulst and Smith (1982b) for an early overview of all these developments.

Even though all these developments concerned the representational aspect of the theory, there were potential intrinsic trade-off relations between the derivational and the representational aspects. The enrichment of the representational side of the theory had, or was hoped to have, an effect on the derivational side. It was expected that rich representations might allow for the reduction of rules to elementary operations that insert or delete association lines. However, even such 'elementary operations' can be extrinsically ordered or be responsible for dramatic changes if not properly restricted. In addition, adding subsegmental hierarchical structure and adding rules that construct suprasegmental hierarchical organization lead to novel possibilities for the creation of intricate derivations in which rules that manipulate the subsegmental feature structure may precede or follow the addition of, for example, syllable structure.

With respect to the 'features', now organized in a hierarchical structure (cf. Clements 1985), an independent issue (already raised in Sanders 1972) arose concerning the question of whether all of the features are binary-valued. Leaving aside isolated proposals to invoke multivalued features, the question of

unary (or *monovalent*) features drew some attention. Since SPE's chapter nine it was, of course, known that there are significant recurrent asymmetries between the two values of most, if not all, features. Certain values are universally more 'expected', 'more natural', or 'more frequent' (either across the board or in specific phonological environments) and Chomsky and Halle had tried to capture this in their theory of markedness. They replaced the 'favored' values by 'u' (for *unmarked*) that were not supposed to 'count' for or contribute to the cost of rules and representations. Markedness theory was superseded by a re-emergence of underspecification theory, with the significant extra idea to leave 'unmarked' values simply unspecified (Kiparsky 1982). Chomsky and Halle had rejected the mechanism of underspecification of redundant feature values because of technical problems, notably the emergence of 'ternary power'. (This problem disappears when extrinsic ordering is rejected, as shown in Ringer 1977, but this, as we have seen, was not accepted at MIT.) Briefly, Kiparsky solved the ternarity issue by imposing that for each feature only one value can be left unspecified. This idea of 'radical underspecification' (not only leaving out redundant but also unmarked values) made all features, at least in the input representation, virtually single-valued and this effect 'bled' any serious consideration of true single-valued features in 'mainstream' generative phonology.

My goal, as in the preceding section, has not been to offer a detailed and motivated account of the developments and proposals, but simply to show that, indeed, there was this period in the history of generative phonology, of intense focus on the structure of phonological representations. However, I wish to make one additional point before we turn to the nineties.

In discussing the representational developments, I have limited my reference to what could be called 'mainstream' generative phonology, i.e., proposals and developments that stem from North American institutions, mainly MIT. However, 'meanwhile, back in the old world', very similar and interesting proposals had been developed concerning phonological representations that were (and still are) completely ignored in the 'mainstream'. I am referring here to the model of *Dependency Phonology* (developed by John Anderson and various other phonologists, mainly in Great Britain). This work (that goes back to the early seventies!) culminated in Anderson and Ewen's (1987) *Principles of Dependency Phonology* (PDP). I regard the PDP-theory as the most principled and fullest alternative to the SPE-theory, and it is in many respects just as programmatic and unconstrained. The important insights that we referred to above as being characteristic of mainstream phonology in the eighties are all there. We find subsegmental, hierarchical organization of phonological features (pre-

ceding similar 'mainstream' ideas by a decade), a truly radical expression of markedness in terms of single-valued features (called components), hierarchical organization of segments into syllabic constituents and higher structure, and, perhaps most importantly, a consistent recognition of the importance of asymmetrical relations between phonological units. The latter relations are expressed in terms of the notions *head* and *dependent*, which occur at all layers of the phonological representation. Independently of PDP, headedness had also found its way into several aspects of the 'mainstream' models. For example, the metrical theory of stress and its extension into 'prosodic phonology' was founded on the idea of heads and dependents, obscured in the earliest versions by the use of S and W-labels (for heads and dependent, respectively); cf. Hayes (1995). In the theory of 'feature geometry', headedness was used in the guise of 'arrows' that identified major articulators (cf. Sagey 1986) or features that occupied the root node (McCarthy 1988), or vowels being 'heads' of syllables (sometimes in the context of head-based theories of the syllable; cf. Levin 1985). However, there was never a general claim concerning the omnipresence and potential of this notion. This was only so in Dependency Phonology, which therefore had that name. The PDP-model was also founded on the idea of there being a *Structural Analogy* between the various components of the grammar (Anderson 1992). According to this idea, structures in the various components of grammar are expected to be parallel, modulo the effects of differences that result from starting out with different primitives. Mainstream generative phonology, on the other hand, proceeded on the assumption that phonology is different, and that, hence, significant parallels between different components are not to be expected (cf. Bromberger & Halle 1989); cf. van der Hulst (2000, to appear) for further discussion of these points. I will return to the PDP-model at the end of this article.

The reasons why Dependency Phonology generated so little interest and research are largely a result of a perhaps more empirically-driven research mentality in North America, as opposed to a more conceptual attitude in Great Britain and perhaps Europe at large. Other (sociological) factors (involving fame and fashion) play a role too, but a consideration of these would take us too far afield.

2.4 Derivational issues predominate the nineties

We now expect, following the dialectic model, that the next shift in attention will bring us back to derivational issues. Indeed, this is exactly what we see and it all happens right at the beginning of the nineties. The nineties are dom-

inated by the approach called *Optimality Theory* (OT), a 'non-derivational', *constraint-based* approach to phonology. At first sight, it may seem odd to introduce a *non-derivational* theory as the main player in a decade that is supposed to focus on derivational issues. However, in accordance with the diagram in (1), OT is a theory about the function F, and in that sense it concerns the derivational side of the theory. The above reference to 'non-derivational' as a property of OT refers to the fact that OT does not recognize or need so-called *intermediate levels* as a consequence of there being no extrinsically ordered rules or subcomponents (such as a lexical and post-lexical component).

2.4.1 Constraints in phonology

The history of constraints in phonology does not start with OT. Constraint-based phonologies (or proposals moving in that direction) have been around in phonology (and linguistics) for a long time. Shibatani (1973), Sommerstein (1974), and Clayton (1976) offer early proposals to use 'output constraints'. In general, all proposals to abandon extrinsic ordering, going back to Koutsoudas, Sanders and Noll (1974), Vennemann (1971, 1974), Ringen (1977), and Hooper (1976) lead to phonologies without intermediate levels. In other cases, even though several levels are adopted, mapping between these takes place in one step; cf. Goldsmith (1993), Lakoff (1993), among others. Prince and Smolensky (1993) also mention a long list of researchers that have anticipated their OT constraint-based approach. For an historical perspective on constraints in phonology see Paradis and Prunet (1993) and Bird (1995). OT instantiates a particular version of this approach with the specific property that, in this model, constraints are violable (or 'soft'). This softness of constraints results from the possibility of imposing an extrinsic ordering on the constraints, allowing them to be violated in the output if more highly ranked constraint enforces this.

In a striking way, OT, while being derivational in the most minimal sense, resembles the champion of derivationalism, viz. the SPE model. Both theories specify the function F as a set of extrinsically ordered statements. There are additional parallels between both models (cf. van der Hulst & Ritter 2000b). Both models employ one *type* of statement (rules or constraints, respectively), which means that both models are 'holistic'. OT, like SPE sees no need to formally distinguish 'morphophonological', 'phonological' and 'phonetic' phenomena. It might, furthermore, be argued that OT attributes to the (idealized) native speaker an account of the data that may very well be plausible from the viewpoint of historical developments. When introducing the concept of historical changes to students, most linguists will make reference to the fact

Once these two ingredients are teased apart, it becomes possible to say that one constraint may be 'served' by a variety of repairs that act in different ways on violations of that constraint, a situation referred to by Kisseberth (1970) as a 'conspiracy'. Conspiracies cannot be dealt with in a satisfactory manner in a rule-based approach without the loss of generalization, since several rules would necessarily repeat the statement of the relevant constraint. For example, if the constraint states that two adjacent obstruents cannot disagree in voicing, and such a situation arises due to morpheme concatenations, different morphological contexts may trigger different repairs. In Dutch, for example, the second obstruent will agree with the first if the latter is part of the past tense suffix (in which case it is a dental stop) (3a), or in other cases (such as compounds or prefixes) if the second obstruent is a fricative (3b). If the second obstruent is a stop (and not part of the past suffix), the first obstruent will be the chameleon (3c):

- (3) a. klop + de $\Rightarrow$ klopte (to knock, sing. past tense)
 straf + de $\Rightarrow$ strafte (to punish, sing. past tense)
 b. op + zeggen $\Rightarrow$ opzeggen (to cancel [a subscription], or to recite)
 af + zeggen $\Rightarrow$ afzeggen (to cancel [a date])
 c. op + doen $\Rightarrow$ opdoen (to put on)
 af + doen $\Rightarrow$ afdoen (to take off)

Thus (3a) and (3b) show *progressive assimilations*, while (3c) shows *regressive assimilation*; these assimilations are the 'repairs'. All three repairs serve the same constraint. The resulting analysis (offered here for expository purposes only in a simplified form) could look like this:

- (4) a. Constraint: * [αvoice] [-αvoice]
 b. Repairs: i. [-continuant] & past tense $\Rightarrow$ [-voice]
 ii. [+continuant] $\Rightarrow$ [-voice]
 iii. [] $\Rightarrow$ [+voice]

The separation of constraints and repairs can also be motivated on *cross-linguistic* grounds. A specific constraint, let us say one that bars a sequence of two vowels, may in one language trigger deletion of the left-hand vowel, while in another language the right-hand vowel will fail to show up. In a third language, epenthesis (of some consonant) might be used to remove the violation.

Repairs, then, are primarily said to come into action when a combination of morphemes, which may be well-formed from the viewpoint of the morphological (categorical) and semantic requirements, violates a phonolog-

that all aspects of languages are under constant pressure to change in the direction of 'preferred patterns'. However, language is a system that must meet various requirements. Its grammar must be learnable and its representations are interfaces to other modules (involving production and perception on the phonological end and conceptual structures on the semantic end). We all, at some point or other, make reference to the 'ongoing battle' between 'production' and 'perception', implying that certain sound changes are motivated by 'articulatory ease' while others facilitate 'ease of perception'. And we then add that what is good for one may be bad for the other. In addition, the outcome of both 'forces' may pose problems for learnability by causing allomorphy, and thus increasing the distance between input and output. Hence, there is *potential* conflict between various sound changes and *inherent* conflicts between sound changes and learnability. The latter force, then, may cause various forms of analogy, undoing 'the point' of sound changes. Any language, at any given point in time, is thus the result of this ongoing battle in which certain universal tendencies temporarily take precedence over others. It seems obvious that OT, like SPE, but in a different way, translates a plausible diachronic scenario into a synchronic account of how native speakers capture the regularities and irregularities of their language (cf. Boersma 1998). This does not mean that OT is wrong as a viable model of a speaker's competence. It could be on the right track and I return to the issue of OT psychological plausibility below.

1.4.2 Constraint-and-repair approaches

Before we look at the way in which OT works in somewhat more detail, I will discuss another constraint-based approach that predated OT, the so-called *constraints-and-repairs* (CR) model. The idea of a constraints-and-repairs approach has been put forward in, among others, Hockett (1955), Stewart (1983), Singh (1987), Calabrese (1986, 1988, 1995, to appear), Paradis (1988) and Paradis and B  land (2002); cf. La Charit   and Paradis (1993) for an overview of several constraint-based theories. In comparison to the rule-based approach advanced in Chomsky and Halle (1968), one might say that the CR-approach reworks up the 'rules' that were used in classical generative phonology, into two parts:

- (2) $A \Rightarrow B / C - D$ (SPE rule format)
- | | |
|------------|-------------------|
| Constraint | Repair |
| | |
| *CAD | A $\Rightarrow$ B |

ical constraint. This leaves open the possibility that violations are not repaired, in which case the morpheme combination would simply be blocked. For example, in English, the comparative suffix *-er* cannot be added to polysyllabic adjectives (except those bisyllabic adjectives that end in *-ow* or *-y*):

(5) *importanter

There is no repair and the form is blocked.

The use of repairs need not be limited to accounting for polymorphemic words. Monomorphemic words also must be wellformed and it is, in fact, likely that many constraints are valid for both classes of words. With reference to monomorphemic inputs, however, we do not, at first sight, expect any repairing since one would assume that such items, being stored in the lexicon, are wellformed to begin with. Why clutter the lexicon with items that are not wellformed? However, things are not that simple. Firstly, if wellformedness constraints refer to 'words' (i.e. free occurring items), why would bound forms such as affixes or (bound) roots have to comply with these constraints? In fact, often they do not. Secondly, to account for certain cases of allomorphy, we may set up underlying forms that, as such, cannot occur as free forms. Notoriously, this is so in any account of final devoicing in languages such as Dutch, where the voiced allomorph must be taken as basic. This latter case shows that repairs may apply in the absence of morpheme concatenation, because of the way that the underlying form is (or must be) chosen. If 'abstract' analyses are permitted requiring (absolute neutralization), more instances of strictly morpheme-internal repairs are called for; cf. Calabrese (to appear) for discussion.

In any event, perhaps wellformedness constraints do not need to apply to lexical forms as such; this is precisely what various authors have argued (cf. Paradis & Prunet 1993; cf. also Paradis 1993; Booij 1999 on this issue). OT also adopts the view-point that lexical forms are not restricted; this is referred to as 'freedom of the base'. The general idea is that since lexical entries can only manifest themselves as part of wellformed words, no special precautions are necessary for these entries.

Thus, repairs can only be seen in case violations of constraints are created in morphological concatenations or because of certain choices concerning the underlying forms, choices that are also driven by accounts of allomorphy. But morphological analysis induces violations to only a subset of all constraints. For example, English does not have any morphology that causes illformed syllable-initial clusters, but, *sur -jy, t' ere* are constraints on such clusters, which, some would say, arise as inductive generalizations over the set of

all words! (We can also think of their emergence in the grammar as a result of parameter setting.) However, a certain area of phonology, namely *loan word adaptation*, reveals not only that such constraints really exist, but it also makes us realize that grammars must have access to repair strategies that play no role in the adjustment of morphologically induced constraint violations. It is now evident why constraint-based approaches have shown great interest in the field of loan word adaptations (cf. Paradis & LaCharité 1997 for an overview; cf. also Paradis & Béland 2002).

A question that needs to be addressed is whether there are cases where several repairs apply to the same input, and whether in such cases extrinsic ordering is required. I refer the reader to Calabrese (to appear) who discusses cases of this sort. In the model proposed by Paradis (cf. 1988), it is shown that constraints and repairs may be restricted to certain domains, which may create the impression that violations are tolerated to persist simply because certain combinations arise across the boundaries that delimit the scope of certain constraints and repairs. A detailed discussion of such issues is beyond the scope of this article.

2.4.3 Constraint-only approaches

2.4.3.1 Declarative Phonology.

I start this section with a brief introduction of a constraint-based approach, called *Declarative Phonology* (Scobbie 1997; Bird 1990, 1995; and Coleman 1992, 1995a). This approach views the (phonological) grammar as consisting of constraints only; no repair rules are allowed, based on the principle that structure-changing operations cannot be allowed in the grammar. Constraints are not extrinsically ordered and thus not violable or 'soft'. As a consequence, every constraint must be met in the entire output (which is effectively equivalent to the requirement in *Natural Generative Phonology* that all phonological rules must be 'true generalizations of all the surface forms'). Ogden (1999) combines the declarative model with a Firthian approach to phonological representations, while Coleman (1995b) integrates the lexical/post-lexical distinction. Declarative Phonology does not claim that phonological constraints are universal or innate, and consequently lexical entries are seen as constraints themselves. The surface forms result from the *unification* of constraints. Thus 'applying' a process to a form is achieved by unifying the relevant lexical entries with the constraint that expresses surface-true processes or generalizations. To accommodate certain types of phonological and (I suppose, all) morphophonological alternations, lexical forms must be provided with choices (disjunctions). Declarative Phonology strikes me as a restrictive, sound and promising approach which therefore deserves

more attention and exploration even if it perhaps relies on representational assumptions that are too strict or too 'phonetically-driven'. More on that below.

2.4.3.2 *Optimality Theory*. I now turn to OT. In what way does this model differ from constraint-and-repair models? We have just seen that for any given input that violates some constraint, a variety of possible repairs is in principle available (either within a single language or cross-linguistically). In the constraint-and-repair approach, one needs to specify which repair applies in the appropriate circumstances. This is a parametric choice, assuming that UG makes the set of choices available to the child. One might, although this has never been done in principle-and-parameter approaches, formalize the choice of parameter values in terms of ranking of the choices, putting the choice that applies to the language at issue at the top of the list. Suppose now that all possible repairs of an input that violates some constraint are simply freely made available (in the form of a so-called candidate set) and that the correct output is selected by choosing the one that conforms to the repair that is at the top of the list of possible repairs. We then arrive at an approach to repairs that is almost the one found in OT, but not quite yet.

Before we get to that, let me mention that approaches that select the correct output from a set of viable candidates have been proposed now and then (cf. Lapointe & Feinstein 1982). Such models, essentially prefiguring the gist of OT, interestingly never gained much support (sometimes being accused of being 'global', i.e. using look-ahead mechanisms.).

In OT, repair rules are disguised as 'anti-repair' constraints. Instead of rules that either delete the first vowel, or the second, or insert a consonant (all to repair a constraint against vowel clusters), OT has *constraints* that *forbid* the deletion of a left-hand vowel or right-hand vowel, or the insertion of a consonant. Such 'constraints', however, are very different from the constraints that trigger the 'repairs' in the first place. The latter (the *real* wellformedness constraint, I would say, which OT calls '*markedness constraints*') make reference to the structure and content of output representations. The former (called '*faithfulness constraints*'), on the other hand, *compare* two levels of representation, the input and the output. In order to know that some segment has been inserted, deleted or altered in the output it is, after all, necessary to look at the output form. So how do faithfulness constraints and markedness constraints work together in OT to select the correct output?

To answer this, we first need to account for the situation in which there is no repair at all. How can a form that has a vowel cluster surface at all? (OT ignores the option that such a form is simply rejected, maintaining that there

always is an optimal output and thus there are no such things as *absolute ungrammaticality*; I ignore that point here.) A possible answer is that a vowel cluster can surface in case the language simply lacks the constraint against vowel clusters. OT does not follow this course. It assumes that all markedness constraints (and all faithfulness constraints) are part of the UG contribution to every grammar; they are innate. After all, with the mechanism of ranking already in place, we can simply say that constraints that are violated rank below all possible faithfulness constraints that refer to possible repairs of this constraint:

- (6) don't delete V1, don't delete V2, don't insert C >> *VV

In (6), it is said that it is worse to violate any of the anti-repair constraints than to violate the wellformedness constraint that forbids vowel clusters. Hence, the language allows vowel clusters. The faithfulness constraints do not even have to be ordered among themselves. When repair does apply, the markedness constraint can be located just above the faithfulness constraint that can be violated:

- (7) a. don't delete V1, don't delete V2, *VV >> don't insert C
b. don't delete V1, don't insert C, *VV >> don't delete V2
c. don't insert C, don't delete V2, *VV >> don't delete V1

(7a) applies when the language has consonant insertion, (7b) when it has deletion of the right-hand vowel, and (7c) when it has deletion of the left-hand vowel.

This aspect of OT (i.e. the relative ranking of faithfulness and markedness constraint) recapitulates the conflict between learnability and phonological processes. Faithfulness constraints serve learnability by militating against any difference between input and output. Markedness constraints serve ease of production and perception at the cost of allowing processes that change the input. OT also recapitulates potential conflicts between different phonological processes. My favorite example comes from an account of iterative stress systems that can be found in two basic varieties, i.e. quantity-sensitive systems (cf. Hayes 1995). In non-OT parametric approaches this choice is represented as a parametric option. As said before, parameter choices *could* be represented in terms of ranking the two values. Effectively, this is exactly what happens in OT, but the ranking is, in some cases, claimed to involve two independent markedness options that are in conflict. In the case of iterative footing one might argue that there is an inherent conflict between a regular binary (thus rhythmic) grouping, which turns every other syllable into a rhythmic beat,

and the acknowledgment of intrinsic weight differences between syllables. Such weight differences militate against placing 'heavy syllables' in non-beat positions. Given, then, that there are two potentially conflicting constraints, both language types can simply be seen as resulting from different orderings:

- (8) a. Regularity (i.e. have a regular binary grouping)
 b. Weight (i.e. place heavy syllables in beat positions)
 Quantity-sensitive: Weight >> Regularity
 Quantity-insensitive: Regularity >> Weight

The parametric approach 'shortcuts' the prioritization of the two independent constraints by positing one (binary) constraint. Once the parameter is set, it functions effectively as a constraint and the difference with OT is that 'the other constraint' (i.e. the other value of the parameter) is simply not there. (It would be an error to conclude that no grammar can display evidence of both settings; this is certainly possible in case both settings apply to different domains; cf. van der Hulst & Ritter 2002 for discussion.)

The OT-model (first introduced in Prince & Smolensky 1993; McCarthy & Prince 1993; cf. Kager 1999; McCarthy 2002 for introductions) has been enormously popular (both among phonologists and non-phonologists), so much so that agendas for conferences and job vacancies changed almost overnight. Its popularity was due, aside from intrinsic properties of the model, to widespread marketing and distribution of the seminal manuscripts, and subsequent practically unmonitored posting of papers on an internet site that sends out messages of new arrivals to all who register. In addition, the model was introduced with rigorous (or at least impressive) formal underpinnings and an almost obligatory set of notational conventions. At the same time its essential design was almost ridiculously simple (constraint and ordering, that's it). All this created enormous homogeneity among the fast-growing number of applications of the theories to both old and new data.

But there are certain problems. As might be expected, these problems have led to a number of variants of the model, while having little effect on the enthusiasm for the general idea of constraint ranking. Here, I cannot discuss variants of OT in any detail; cf. Ritter (2000) for a collection of critical articles.

The most interesting problem concerns *opacity*. OT predicts that it does not exist. The effects of counterfeeding and counterbleeding rule application creating the effects of overapplication and underapplication, respectively) that required the extrinsic ordering of rules in SPE, cannot be replicated in OT. SPE could postulate a rule A that could be violated in the output because some later

rule B had introduced potential new inputs for A (counterfeeding), or a rule A whose output would be modulated by later rules so that it would seem that it that rule A failed to apply in the relevant cases (counterbleeding). It was felt that such rules, despite being opaque, express *significant generalizations*.

Let us imagine that in the case of Dutch, involving the voicing assimilation facts, a further rule enters the grammar that deletes vowels in certain cases, creating clusters of voiced and voiceless obstruents (this is actually possible in fast speech):

- (9) tabak $\Rightarrow$ tbak 'tobacco'

Given an OT grammar that selects [db] as the optimal output for inputs that contain /tb/ there is just no way to get [tbak] as the optimal candidate, given the free availability of other candidates (such as [dbak]) that do not contain the violation. (With enriched representations in which, for example, an empty vowel slot remains between *t* and *b* the output form [t-bak] would not violate the voicing constraint since *t* and *b* would not form a cluster.) Hence such an approach would allow certain types of opacity; cf. van der Hulst and Ritter (2000a) or Goldrick and Smolensky (1999).

This prediction of OT, one might argue, fits nicely with the widespread suspicion that arose in the seventies against opacity and the mechanisms that made it possible (rule ordering) and as such it could have been seen as a welcome result. However, the architects of OT had never been suspicious of opacity as such and they never had quarrels with rule ordering in the seventies or (eighties). OT was not developed to arrive at more 'concrete' grammars and its consequence for opacity thus came as somewhat of a shock and disappointment. In this sense OT, or rather its proponents again view phonology very much in the spirit of the SPE-model. They are not prepared to give up on the 'significant generalizations' that were embodied in the opaque SPE-rules. There is by now a variety of (to my mind compromising) approaches to 'solve' the opacity problem (cf. McCarthy 1999, 2002). Among the more interesting ones are those that reintroduce the extrinsic ordering of phonological subcomponents, for example Kiparsky (2000), who combines OT with the lexical/post-lexical division, thus allowing the post-lexical constraint system to introduce opacity with respect to the lexical system; in the Dutch example above, the vowel deletion process in (9) could be seen as belonging to the post-lexical phonology where the voicing constraint occupies a different place in the ranking. Another approach (advocated in Goldrick & Smolensky 1999) seeks to understand opacity effects in terms of a richer notion of representations (cf. supra, and the discussion in van der Hulst & Ritter 2000a who compare

this idea to an approach called Government Phonology; Kaye, Lowenstamm, & Vergnaud 1985, 1990; Kaye 1990, 1995; Harris 1994; Ritter 1995; van der Hulst & Ritter 1999, 2002).

But OT faces other problems as well. It is hypothesized that all constraints are universal. This may appear reasonable for those phenomena that involve 'natural' processes (cf. Stampe 1973 who also argued that such processes are innate and have to be unlearned if not applicable in the language that is being learned.) However, many phonological alternations are morphophonological and thus limited to specific environments which can sometimes be quite arbitrary or 'crazy'. It is simply ridiculous to claim that quite arbitrary phoneme substitutions (which result from specific historical scenarios involving 'tele-scoping' of several processes) manage to make their way into the human genome. Calabrese (to appear) argues that such cases simply reflect learned rules in which there often is no generality in the configuration that is targeted, nor in the change that is made.

The innateness claim is not only totally unlikely when it comes to morphophonological effects, it is also untestable. Any given constraint (needed for a set of quirky facts in some language) can be tucked away low in the constraint hierarchy of other languages so that its effect will never emerge. This problem of unfalsifiability, however, is not devastating for OT. There is no deep reason why OT could not embrace the concept of learned constraints. In fact, Hayes (1996) proposes that *all* constraints are learned, although he especially focuses on constraints that express natural processes. He proposes that such constraints are derived from intrinsic phonetic tendencies (or language-specific 'exaggerations' of these) in the process of language acquisition.

OT also faces psycholinguistic problems. According to the theory, optimal outputs are selected from an infinite set of candidate outputs, rather than from a small set of 'reasonable' alternatives. It is not clear how this translates into a realistic model of (even an idealized) native speaker's competence. Selecting an optimal candidate from infinite sets of all possible structures is a way of looking at the problem of characterizing wellformedness, but it does not seem viable as a model of what occurs in people's finite minds.

This brings me, finally, to a criticism of current OT that does not regard the theory as such, but rather the practice of it. Let us assume that the problem of dealing with morphophonological alternations can be dealt with in a satisfactory way; either by following Calabrese's proposal to allow learned rules, or by allowing learned constraints (both of which make reference to specific morphological contexts). Let us also accept that it is possible to translate parametric choices between repair rules into ranking of anti-repair (faithfulness) con-

straints and translating all other parametric choices that regard wellformedness ('markedness') of phonological representations into ranking of these choices. Finally, let us realize that so-called 'principles' (expressing properties that all languages have) can also be seen as constraints, albeit that they have to be universally 'top-ranked'. In short, let us admit that it is *possible* to view the grammar as a set of ranked constraints. What, then, is the (or my) problem with the practice of OT? The problem is, in my view, a serious neglect of representational issues. Following the dialectic pattern of phonological theory, it seems that the nineties must be the decade for paying attention to the content of wellformedness constraints, i.e. to the *phonological representations that they are supposed to characterize*. OT is often referred to as a theory of constraint *ranking*. As such, it can be applied to anything (ranging from language to religion and everything in between). The interest of OT as a phonological (or linguistic theory) lies, however, in the content of its constraints. So, one might ask: what is a possible constraint? I don't believe that the question is fair. Rather, it seems to me that we need to realize that the set of wellformedness (or 'markedness') constraints IS, as a whole, a theory of phonological representations. This means that the constraint set (whatever the exact form of constraints) must form a *tightly organized and coherent set of statements*, based on a finite set of primes and a finite set of principles, regulating the set of complex expressions that can be formed from those primitives. It would seem obvious that the set of primitives and combinatorial constraints must be kept as minimal as possible so that the set of possible phonological systems (or grammars) is kept small. After all, unless something has changed lately, it would seem that a theory's explanatory value is still inversely related to its restricted make-up. Theories that employ constraint ranking have to be extra careful in this respect since the addition of every constraint causes an exponential increase in the number of possible grammars; for n constraints, adding one, multiplies the set of possible grammars by $n+1$. (I am ignoring here that the set of possible grammars is further enlarged by allowing equal ranking of constraints, as well as a possibly infinite array of 'conjoined constraints'.) Compare this to parametric theories in which the addition of one parameter multiplies the set of grammars by two. One would think that this fact cautions OT proponents in proposing new constraints. It does not look that way. In defense of the exponential growth of possible grammars, proponents of OT argue that many of the OT grammars allowed by any set of constraints 'converge' by allowing the same language. But should this be a license for increasing the set of constraints without caution (since they are innate anyway) and without any attempt to

construct the constraint set with reference to a coherent view of phonological representations?

To control the explosion of constraints, OT has argued that constraints need to be 'grounded', which is another word for 'natural' and in practice means that there must be a phonetic basis for the constraint. But little progress can be seen in developing such phonetic bases, despite the fact that more and more phonologists have returned to studying their phonetic text books, while formulating constraints with reference to phonetic notions involving formant frequencies and all sorts of gradient properties (timing, among others). The appeal to phonetics is, I suppose, stimulated by the idea that constraints must make reference to the output. It turns out, however, that the output is no less an elusive notion than the input. Where the permitted degree of abstractness of the latter can by no means be determined on a priori grounds, the same is true for the former. Is the output a 'phonological' level of representation (I would say: yes), or it is a 'phonetic' representation (stated in terms of motoric-articulatory instructions or perceptual-acoustic primitives)? Or is it perhaps the case that the distinction between phonology and phonetics is illusory to begin with? The latter view has become increasingly popular both in OT-circles and in *Laboratory Phonology* circles. There appears to be much confusion on this issue, but the idea that phonological output representations form a cognitive-symbolic level of representation, related to, but distinct from an also cognitive-phonetic level (or levels, if production and perception are kept apart) becomes increasingly unpopular.

In sum, notwithstanding some insightful and interesting results, we must conclude that OT faces serious problems, mainly due to a lack of discipline regarding the formulation of 'markedness' constraints. It is not possible to construct a theory of phonological representations by randomly formulating constraints that make reference to all sorts of undefined phonological and phonetic concepts. OT, while focussing on the derivational aspect of phonology (following the dialectic path) cannot proceed fruitfully without starting to pay serious attention to constructing a theory of phonological representations (which is what the set of 'markedness constraints' should be).

Without this discipline, OT is 'easy' and all problems can be solved during a 9-to-5 working day (and that is not necessarily a good thing although our families will be happy). To demonstrate that OT is 'easy' and to encourage its users to side-step real solutions, I conclude with an example (that I made up to illustrate the point more clearly and to not point my finger at a specific analysis). Suppose one looks at a language in which vowels reduce in metrically weak positions, with one exception: final vowels never reduce. Here's an 'analysis':

(10) NonFinalReduce >> WeakReduce

This strikes me as the kind of analysis that is based on a logical mistake due to which *what needs to be explained* becomes the *explanation* (*Modus Explanando Explanans*).

3. Conclusions

There is, I believe, no intrinsic problem with viewing the choices that languages make in terms of ranking mechanisms instead of parameter setting (although the two remain notational variants until significant differences can be shown to exist). The crucial point I am making here is *not* that constraint ranking is bad per se. In certain cases, it has an appeal and it therefore may prove to have a place in phonology (perhaps mostly in the post-lexical phonology). The point is rather that OT does not offer a serious candidate for a theory of phonology because its constraints comprise no view, let alone a coherent view on the representations that they are supposed to define.

I believe that there are serious representational theories 'on the market'. Many were developed during the eighties and I would like to draw specific attention (reflecting my own bias) to theories such as Dependency Phonology and, more recently, Government Phonology, which elaborates on and refines central concepts of Dependency Phonology. Government Phonology is, effectively, a constraint-based model, sharing properties with Declarative Phonology in allowing only hard constraints, although proposals for some ranking have been made (cf. Polgárdi 1998). In van der Hulst (in prep.) I provide a detailed overview of Dependency and Government phonology (cf. also Scheer to appear). Currently, proponents of both approaches are small in numbers, which means that much work remains to be done. But the ideas are, to my mind, interesting and powerful, deserving more attention. It is perhaps also possible to benefit from the formalisms such as those advanced in Declarative Phonology circles, given that these lend themselves to the formulation of constraints that refer to somewhat more abstract representations (restricted in terms of universal principles and parameters) than are usually adopted in Declarative Phonology analyses. Let us hope, in any event, that the present decade is well-behaved in terms of our dialectic schema and that phonologists will start focussing on representational issues which would allow them to develop constraint-based approaches (possibly with a bit of ranking) that are truly explanatory. It is time.

Acknowledgements

I am grateful to Andrea Calabrese and Nancy Ritter for their valuable comments on an earlier version of this article.

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Types of languages and the simple pattern of the core of language

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1. Introduction

I would like to present and substantiate several points which may be of crucial importance to the development of theoretical linguistics, although they have been largely neglected in the "mainstream" trends. After several decades of investigations and discussions first in the context of V. Skalička's typology and then in that of the Functional Generative Description, I am convinced that they are significant:

1. The *physical character* of natural languages, which use phonetic means, imposes *strict limitations* on the relationships between meaning and expression. The limitations are decisive for the ways in which grammatical values are conveyed. Basically, grammatical values can only be expressed by (a) morphemes (b) alternations, and (c) the order of lexical items in a sentence. These properties constitute the fundamental background to the types of languages. A characterization of the types is discussed in Section 3.1 below, after a brief examination of the nature of general concepts used in connection with language typology in Section 2.
2. One of the main results of the history of typological thought is the transition from overestimation of Indo-European (old or modern) languages to the recognition that typological change does not constitute a line of "progress" and that language types are *not* directly connected to the semantic *richness* of languages (see Section 3.2 below).
3. The interactive nature of language is reflected in the sentence structure by the opposition of topic (T) and focus (F), i.e. by the relation of *aboutness*, of a "psychological" predicate F and its argument T as the background to