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**THE CONTRIBUTORS**

**Ricardo Bermúdez-Otero** is Senior Lecturer in Linguistics and English Language at the University of Manchester. His research focuses on the morphosyntax–phonology and phonology–phonetics interfaces, with particular attention to diachronic issues. He works predominantly on Germanic (especially Old, Middle, and Present-Day English) and Romance. His publications on historical phonology include chapters in *Optimality Theory and Language Change* (2003, Kluwer), *The Cambridge Handbook of Phonology* (2007, CUP), and *The Oxford Handbook of the History of English* (2012, OUP).

**Juliette Blevins** is currently Professor of Linguistics at the CUNY Graduate Center, with research interests in Austronesian, Australian Aboriginal, Native American, and Andamanese languages. Her book *Evolutionary Phonology* (2004, CUP) presents a unique theory synthesizing results in historical linguistics, phonetics, typology and phonological theory. She currently has over 100 publications, most recently in such journals as *Phonology*, *Oceanic Linguistics*, and *Language and Cognition*.

**David Bowie** is an Associate Professor at the University of Alaska Anchorage, where he is one of the English Department's linguists. For the past few years he has been conducting research on the sociolinguistic effects of changes in age and religious affiliation, so as to provide insights into the linguistic ramifications of involuntary and voluntary changes in identity. He is also currently laying the groundwork for a planned dialect atlas of Alaskan English.

**Joan Bybee** is Distinguished Professor Emerita of Linguistics at the University of New Mexico. Her work utilizing large cross-linguistic databases, e.g. *Morphology: A Study of the Relation between Meaning and Form* (1985, Benjamins), *The Evolution of Grammar: Tense, Aspect and Modality in the Languages of the World* (1994, University of Chicago Press, with Revere Perkins and William Pagliuca), provides diachronic explanations for typological phenomena. Her books presenting a usage-based perspective on synchrony and diachrony include *Phonology and Language Use* (2001, CUP), *Frequency of Use and the Organization of Language* (2007, OUP), and *Language, Usage and Cognition* (2010, CUP).

**András Cser** is Associate Professor at Pázmány Péter Catholic University. He has published on theoretical and historical phonology, specifically on Latin phonology, as well as on the history of linguistics (Hungarian and European). His works include *The Typology and Modelling of Obstruent Lenition and Fortition Processes* (2003, Akadémiai Kiadó).

that these datasets do not settle the debate definitively, being limited to a small set of phonological units and processes in one language. We also concur with Bloomfield (1933): no single explanation is likely to satisfy all nuances of what is a very complex question.

However, our conclusion is that early errors are highly unlikely to lead to change. Errors diminish with time, and some early processes disappear by around age 3. Others appear and disappear sporadically, and are limited to particular individuals. Initiation changes, if ascribed to vocal tract dynamics or perceptual factors, are no more likely to be the responsibility of children than of adults. It is also imperative to acknowledge the extent of variation between individuals learning the same language, especially at younger ages, a fact largely ignored by previous commentators. For a new generation to recast a grammar would appear unlikely when members of that generation take so many different paths to acquire it (see further Vihman et al. 1994).

Children's role in transmitting changes in progress is more readily demonstrable. But it is to the performance of older rather than younger children that we must look, and it is more profitable to conceptualize child patterns as learned features than as errors. Understanding the social context in which learning takes place is crucial, to explain why certain individuals influence others linguistically, and why linguistic variants confer some sort of social or communicative advantage to language users.

The answers to many other questions about change remain partial. To what extent are innovations the product of vocal tract dynamics, and to what extent the product of forces internal to the grammar (cf. Jones, this volume)? In transmission, what factors are necessary for, or conducive to, new forms being transmitted? Finally, for both innovation and transmission, we can only echo Saussure and Bloomfield: *'why here?, why now?'*

## CHAPTER 19

# HOW DIACHRONIC IS SYNCHRONIC GRAMMAR? CRAZY RULES, REGULARITY, AND NATURALNESS

TOBIAS SCHEER

### 19.1 INTRODUCTION

REGULARITY in linguistic patterning is the result of grammatical computation: it is due to the fact that lexically stored pieces are run through a computational system (made of rules or constraints) before they reach the surface. What we see, then, are the traces that grammar leaves on the lexical ingredients, and these traces are regular.

This view is held in generative quarters at least for synchronic patterning. Another classical tenet which is intuitive and widespread among linguists is the idea that phonological processes are natural. This may mean a variety of things (Postal's 1968: 53 ff. Natural Condition is an influential early reference), but essentially has two requirements: (1) there are constraints on which segment any other segment can alternate with (i.e. the relationship between A and B in  $A \rightarrow B / C$  is not arbitrary), and (2) there is a non-arbitrary causal relationship between the triggering context (C in our example) and the structural change observed ( $A \rightarrow B$ ).

This chapter tackles the question of naturalness in generative phonology: what is an innocent rule<sup>1</sup> ( $k \rightarrow \bar{f}s / \_i,e$ ), what is a suspicious rule ( $k \rightarrow s / \_i$ ), and what is a crazy rule ( $k \rightarrow m / \_n$ )? Crazy rules are rules that make no (phonetic or phonological) sense (Bach & Harms 1972). It also enquires into the sources of computation-created regularity. In both cases, diachrony underlies synchronic patterns (see Blevins, this volume, for

<sup>1</sup> I talk about rules throughout, rather than about constraints, when referring to phonological computation. All statements could be translated into constraints.

a related position). The real issue at stake in the chapter, then, is the relationship between synchronic computation and diachronic evolution in sound patterns. How much diachrony, if any, is in synchronic grammar? How does it get there? How is diachronic information represented in the synchronic computational system? Can computation itself age, and if so, what does the diachronic evolution of a phonological rule look like? Of course there is no cognitive computation over time: the human brain cannot compute a twenty-first-century output form based on an input form from, say, the nineteenth century (see the opening dilemma in Hale et al., this volume). Given this, what might it mean for a phonological process to be diachronic: aren't all rules necessarily synchronic?

The classical generative position sees sound change as the result of a modification of the rule system through rule addition, rule suppression, or rule reordering (e.g. Halle 1962, Kiparsky 1968: 174–5, King 1969: 39 ff., Drescher, this volume).<sup>2</sup> Generativists thus implicitly claim that synchronic alternations and diachronic innovations are the same thing: they are the product of grammatical computation, and this computation is only synchronic. Strictly speaking, then, there is no such thing as diachronic innovation: diachronic patterns are simply 'old' synchronic computation which may have aged and is looked at with hindsight. Today's synchronic processes are tomorrow's diachronic patterns, and what we identify as diachronic processes today were synchronically active processes at some earlier stage. In sum, diachrony in sound patterns is a matter of computation and of nothing else.

An alternative scenario is Bach & Harms's (1972) idea that grammar need not evolve by modifying its rule inventory, but rather by modifying existing rules. Hence  $A \rightarrow B / C$  may age by substituting, say, B with Z. This is how crazy rules are born, i.e. rules which 'make no sense', phonetically or phonologically. This chapter discusses crazy rules and presents one in detail: the alternation between l and ʁ that is found in external sandhi in certain varieties of Sardinian (section 19.5). My aim is to evaluate the consequences of crazy rules for phonological theory: are these patterns really the result of synchronic phonological computation? Which (ideally theory-neutral) criteria allow us to decide whether an alternation derives from distinct lexical items or is the result of computation? In the latter case, how do we know whether this computation is (1) phonological, (2) morphophonological (in the structuralist sense), (3) allomorphic, or (4) analogical?

I consider the response of a number of phonological frameworks to crazy rules; they fall into two major categories, either claiming that 'small is beautiful' (such that phonology is shrunk: only a small subset of what Chomsky & Halle 1968 (henceforth *SPE*) managed is due to phonological computation) or that 'big is beautiful' (*SPE* was right: anything can turn into anything in any context). The former approach defines and upholds a notion of naturalness, the latter does not (sections 19.3 and 19.6). (Non-generative approaches to change are considered elsewhere in this handbook, for example in Phillips, this volume, and Bybee, this volume.)

<sup>2</sup> See the critique of this position in Hale et al. (this volume). Of course, this position also allows for the restructuring of underlying forms. This is not spontaneous, but the result of pressure from the computational system. Innovation, generativists say, is only due to the modification of the rule system.

Finally, the chapter shows that crazy rules are only ever *melodically* crazy: there is no craziness reported from syllable structure or stress assignment (section 19.7). This provides further evidence that the areas above and below the skeleton are fundamentally different in kind.

## 19.2 DIACHRONIC AND SYNCHRONIC PHONOLOGY: THE SAME OR DIFFERENT?

### 19.2.1 Processes: a large Overlap, also in What is Unattested

Let us begin by asking whether synchronic and diachronic regularity are the same: is phonology one, or are there two distinct phonologies, one diachronic, the other synchronic? If there are two, we might expect that the two computational systems produce different patterns because they may be inherently different (compare Kiparsky, this volume, on 'Saussure's Firewall').

Even a cursory consideration shows that synchronic and diachronic patterns by and large overlap. We do not need here to review pattern after pattern in order to see whether there is a synchronic and a diachronic match (e.g. Ohala 1992: 310 ff., 1993: 239 ff.). Rather, let us take a global look at the processes that exist on both sides.

I distinguish here between *processes* and *patterns*. Palatalization is a process, while  $k \rightarrow \text{tʃ} / \_i, e$  is a pattern. The same processes are known from synchronic alternations and diachronic evolution. Closed syllable shortening, open syllable lengthening, and compensatory lengthening for example are found both in diachronic and synchronic phonology. On the other hand, closed syllable lengthening, open syllable shortening, and compensatory shortening are as outlandish on the synchronic side as they are in diachronic evolution.<sup>3</sup>

What is interesting is that the list of processes that are absent from the record also seem to coincide. This is true for closed syllable lengthening, open syllable shortening, and compensatory shortening, but also for a very broad and deeply rooted asymmetry in phonology such as the absence of velarization, as opposed to the plethora of palatalizations that occur. The general trend is fairly consensual: there is a great overlap between the phonological processes found in synchronic and diachronic phonology, both in terms of occurring and non-occurring items.

<sup>3</sup> The notions 'closed syllable lengthening' and 'open syllable shortening' are frequently misunderstood. Cases where vowel lengthening in closed syllables is observed in isolated forms or for melodic reasons (e.g. before voiced consonants) *do not* count as closed syllable lengthening. What would count is the existence of a *causal relationship* between the syllabic environment and the modification of length. Hence a closed syllable shortening is a shortening that is *specifically* triggered by closed syllables. It therefore occurs in *all* closed syllables and only in this environment. The same goes for putative closed syllable lengthening, which would be a process whereby short vowels lengthen *specifically* in closed syllables and *nowhere else*.

Are there *any* phonological processes that occur synchronically, but are absent from the diachronic record, or vice versa? Candidates that exhale a distinctly diachronic flavour include metathesis and rhotacism. The former was already suspicious to the neogrammarians, who excluded from sound change all processes that do not substitute one segment with another, as well as those that cannot be gradient in the substitution of the old by the new form (e.g. Hock 1991: 630–1, Murray, this volume). Metathesis meets both criteria: there is no way of having a consonant jump over another segment just a little bit. On top (or because) of its disqualification from the class of sound changes, metathesis was held to be ‘inherently sporadic’, rather than systematic and regular (Lass 1997: 134). Together with analogy, haplology, and dissimilation, metathesis was thus classified as a ‘psychological’ change (i.e. one which is partly under the spell of conscious control, as opposed to sound change, which is a natural event).

The presence of metathesis in the synchronic computation of certain languages is certainly beyond any doubt. Sardinian for instance has a metathesis in external sandhi, i.e. that applies across word boundaries, which therefore guarantees synchronic activity.<sup>4</sup> In the dialects of Genoni and Sestu Campidanese (see map under example (4)), the R of vowel-initial words of the shape VRTV such as [ˈɛrba] ‘grass’ appears to the left of the initial vowel if preceded by a consonant-final word: /sa ɛrba/ ‘the (sg.) grass’ and /papaat ɛrba/ ‘he eats grass’ come out as [ˈsrɛβa] and [papaaˈðrɛβa], respectively (data from Molinu 1998: 142, Bolognesi 1998: 54–5, 419, see Lai 2014).

I leave open the question whether similar evidence (with a guarantee of online activity) can be found for other processes such as rhotacism. In any event, all ‘diachronic’ processes must have been innovated by speakers: there must have been a point when some synchronic event occurred. It is therefore not clear how the distinction between diachronic and synchronic metatheses makes sense, anyway.

### 19.2.2 Patterns: Regular and Suspicious

Let us now consider the match between synchronic and diachronic *patterns* that instantiate phonological processes. Both synchronic and diachronic practitioners are confronted with outlandish-looking patterns like English velar softening, whereby the velars k, g seem to be turned into s, dʒ before i (*electri*[k] - *electri*[s]-*ity*, *analo*[g]*ue* - *analo*[dʒ]-*y*). The question as to whether velar softening is a synchronically active process whereby the surface variation is derived from a common underlying form of the root was a matter

<sup>4</sup> On my view, words are not stored together in the lexicon, except for certain constructions such as idioms and the like. The idea that more constructions are stored than is traditionally believed is developed by Construction Grammar (see e.g. Bybee 2001: 167 ff. on the phonological side, and also Bybee, this volume). Whatever the amount of constructions stored (and unless the existence of an online computational system that concatenates items is denied), the argument based on external sandhi remains unaffected: the alternations at hand do not selectively occur in some constructions. They are observed whenever the intervocalic context is met.

of quarrel for decades and still is today,<sup>5</sup> namely with regard to the abstractness debate of the 1970s (Kiparsky 1968–73 *et passim*, on which more below). The lexicalist alternative considers *electricity* and *analogy* as single, i.e. morphologically non-complex, lexical entries whose pronunciation requires no concatenation and no phonological activity.

While phonologists will probably agree that English velar softening is a suspicious pattern, it is not exactly trivial to define what ‘outlandish-looking’ really means: cross-linguistic (and also diachronic) experience leads us to mistrust k → s, since typical results of palatalization are f̄s and t̄j, or possibly [c]. Velar softening also is riddled with restrictive phonological and morphological conditions (as is common in suspicious synchronic alternations). Unlike typical palatalizations, it occurs only before i, rather than before all front vowels, and is restricted to a number of suffixes, such as -y, -ity, and -ism, which Kiparsky (1982b: 40–1) identifies as belonging to class 1: velar softening does not occur morpheme-internally (*king* is not pronounced \*[s]ing) or before i-initial class 2 suffixes (*hik-ing* is not \*hi[s]-ing, etc.). But there are also exceptions to this pattern: *monar*[k] - *monar*[k]-*ism*, *patriar*[k] - *patriar*[k]-*y*.

Finally, while the alternation may be productive with recent loans, it fails the productivity test with words like *Iraq*: native speakers seem unable to even parse *Ira*[s]ity (‘the property of being typically like Iraq’), but are able to make sense of *Ira*[k]ity.

In addition to what intuition, experience, and the cross-linguistic typology of palatalizations might tell us, velar softening thus bears a number of characteristics that arouse suspicion: limited regularity, limited productivity, morphological conditioning, and questionable phonetic/phonological plausibility.

### 19.2.3 Unattested Intermediate Stages in Diachronic and Synchronic Analysis

My aim is not to argue for a specific synchronic treatment of velar softening: much ink has been spilled on that, and a consensus may still be far off. My point is that those patterns which are suspicious in synchronic phonology raise exactly the same reservations on the diachronic side. Historical phonologists will not accept that a k could change directly into s. The reaction on the diachronic side is broadly agreed on, unlike the uncertainty in synchronic treatments: there must have been an unattested intermediate stage, f̄s in our case. What the synchronic picture thus offers traces of is k > \*f̄s > s, where the original velar was affected by two entirely independent processes, a perfectly regular and unsuspecting palatalization to f̄s, followed by context-free deaffrication.

For English velar softening, we can show that this scenario is correct: velar softening entered the language through extensive borrowing of French vocabulary from the eleventh century on. Modern French has the same alternation (*électrique* [k] - *électrique*-*ité* [s]),

<sup>5</sup> Among much else, relevant literature includes Halle (2005) and McMahon (2007). Green (2007: 175 ff.) provides an overview.

and we know that Latin *k* was pronounced  $\widehat{ts}$  before *i, e* in Old French and Norman (e.g. Bourciez & Bourciez 1967: 128 ff.), which was exported to Britain after the battle of Hastings. Consistent with this scenario is also the fact that only class 1 suffixes effect velar softening: it is well known that affix classes typically arise through language contact that mixes vocabulary strata of different origins (e.g. Mohanan 1982): class 1 affixes are of Romance origin, while class 2 affixes represent the Germanic heritage.

Encouraged by numerous cases of this kind, it is established practice since at least neogrammarian times to make sense of suspicious diachronic evolutions by postulating unattested intermediate stages. There is a broad consensus among diachronicians that there are no ‘crazy’ diachronic processes, only incomplete data. Labov (1972b: 100) formulates it thus: ‘[t]he great art of the historical linguist is to make the best of [...] bad data—‘bad’ in the sense that it may be fragmentary, corrupted, or many times removed from the actual production of native speakers.’<sup>6</sup>

*SPE*’s rule ordering allowed phonologists to adopt the same strategy, to make suspicious synchronic alternations look like perfectly regular phonological derivations. The only thing that needed to be done was to make the intermediate stages synchronic: instead of relating two forms that are diachronically distant, they were understood as intermediate derivational stages that relate an underlying and a surface form. Hence in our example, instead of  $k > * \widehat{ts} > s$ , the synchronic derivation transforms  $//k//$  into  $/\widehat{ts}/$  by a regular palatalization rule, which is followed by a context-free deaffrication rule that derives  $[s]$  (Chomsky & Halle 1968: 224).

The effect of this kind of abstract analysis is that synchronic grammar mimics recent diachronic evolution. On the basic perspective of early generative phonology (phonological change is rule addition, subtraction, reordering), the diachronic evolution of English velar softening involves the addition of context-free  $\widehat{ts} \rightarrow s$ , ordered after the original palatalization rule  $k \rightarrow \widehat{ts}$ . Synchronic grammar may thus recapitulate historical events and literally memorizes them through the piling up of rules.

### 19.3 BUILDING ON THE RUINS OF THE ABSTRACTNESS DEBATE: THE QUEST FOR THE HOLY GRAIL

The abstractness debate was never decided: despite much effort, nobody was or is able to provide a set of formal criteria (called the ‘evaluation measure’ or ‘evaluation metrics’ in the

<sup>6</sup> Blust (2005) takes exception to this view: he discusses a host of sound changes in the Austronesian family (e.g.  $*y > p$ ,  $*dr > k^h$ ,  $*b > k$ ) and concludes that unattested intermediate stages should only be assumed if there is relevant dialectal or historical evidence. Crazy-looking sound changes of the kind he discusses are to be interpreted as one-step modifications of the diachronically primitive form. In this sense Blust represents the big-is-beautiful position (on which more in section 19.3.2) in sound change.

1970s, e.g. Kiparsky 1974b) that allows us to decide whether an item that seems morphologically complex is really considered as such by the grammatical system, and if so, whether or not its computation is phonological in nature. Competing computational mechanisms are a distinct morphophonological device (present in structuralism), allomorphy, and analogy.

Ricardo Bermúdez-Otero (p.c.) has described this as the Holy Grail of phonology: phonologists need to decide whether an alternation falls into the realm of phonological computation *before* they propose a phonological analysis for it. Syntacticians do not face the same issue: sentences are not stored (except for idioms and the like; see note 4 and Scheer 2004b). I consider below two extreme positions on this quest, represented by four theories. Optimality Theory is not among them because the issue is not high on its agenda: the debate is typical of the 1970s, where it was the central line of division between orthodox *SPE* on the one hand and revolutionary Natural Generative Phonology (inside the generative realm) as well as Natural Phonology on the other.

Much like *SPE*, much OT literature jumps into a phonological analysis without considering whether the alternation at hand is phonological. Unlike the four theories discussed, OT has not established a set of properties that define what counts as a possible phonological process. This is not unrelated to a strong tendency in OT to scramble everything: deciding whether an alternation is phonological or not presupposes that phonology and non-phonology are distinct. This, however, is far from obvious in OT, where phonetics and morphology are often held to be mixed with phonology (in the same constraint ranking or even in the same constraint): the computational power of OT feeds a natural inclination to blur or abandon modular contours (Kingston 2007: 432; Scheer 2010: 208 ff., 2011b: §523).<sup>7</sup>

#### 19.3.1 Small is Beautiful

Natural Phonology, Natural Generative Phonology,<sup>8</sup> and Government Phonology minimize synchronic phonological computation (‘small is beautiful’). By contrast, Hale & Reiss (2008 among other references) maximize phonological computation, which is granted the ability to transform any segment into any other segment in whatever context. Both points of view are considered in this volume, the former by Donegan & Nathan, the latter by Hale et al.

<sup>7</sup> There is individual work in OT that has clear modular demarcation lines, and this is also typically where the Holy Grail is considered: cases in point include Bermúdez-Otero 2012: 44 ff., 2013), Bermúdez-Otero & McMahon (2006: 383 ff.), Green (2003, 2004).

<sup>8</sup> Natural Phonology originates in David Stampe’s (1972) PhD and directly inspired Natural Generative Phonology, although the theories took quite different directions in the 1970s. Natural Phonology is explored in Donegan & Nathan (this volume), and references for Natural Generative Phonology include Vennemann (1974a,b) and Hooper (1976a), with some echoes found in the ideas in Mailhammer et al. (this volume). Despite their differences, both share the idea that much of what *SPE* thought of as phonological computation belongs in the lexicon or morphophonology, and this is the point to be made here.



In the two 'Natural' Phonologies, the mechanism that takes over the function of those alternations that do not qualify for synchronic phonological computation is morphophonology: following structuralist thinking, morphophonology is an independent computational system where the structural change of a rule  $A \rightarrow B / C$  may be phonological, while the conditioning context  $C$  is morphological. In this perspective, velar softening is expressed as  $k \rightarrow s / \_ -ity/-ism$ , etc. Note that this mechanism is different from allomorphy which manages cases such as *good-better* and would need to assume two distinct lexical forms of the same morpheme (*electri/k/* and *electri/s/* in our case): the rule  $k \rightarrow s / \_ -ity/-ism$  transforms roots that have only one single underlying form.

The criteria in (1) are used to decide whether a given alternation is the result of phonological computation or not. If not, alternatives are the lexicon, morphophonology, allomorphy, and analogy.<sup>9</sup>

- (1) an alternation cannot be phonological
  - a. if it is not 100% regular, i.e. surface-true OR  
[Natural Generative Phonology, Government Phonology]
  - b. if it has conditioning factors that are morphological (i.e. non-phonetic) OR  
[Natural Phonology, Natural Generative Phonology, Government Phonology]
  - c. if there is no plausible causal relationship between the change observed and the triggering context  
[Natural Phonology, Government Phonology]

Usually (1a) and (1b) go hand in hand. Following Baudouin de Courtenay (1895), Natural Generative Phonology proposes a diachronic perspective on regularity (e.g. Vennemann 1972b): alternations are born as phonetic regularities, then move into grammar where they are first phonological but at some point start to add morphological conditions, followed by lexical factors, and finally are levelled out or eliminated from the language by some other means. During this life cycle, alternations become less and less regular: they apply to 100 percent of those items that satisfy the triggering conditions in their initial stage, but adding morphological and/or lexical conditions subtract more and more items from their influence (this notion of a 'life cycle' can be contrasted with that discussed by Bermúdez-Otero, this volume).

The question as to how much of what we see is controlled by phonology is thus, if not identical, at least concomitant with the question of how much diachrony there is in synchronic sound patterns. A recurrent observation is that what we see in synchronic patterning are more or less fossilized processes that once ruled over larger parts or all of the language.

<sup>9</sup> See Singh (1994) on morphonology. Donegan & Stampe (1979: 143 ff.) consider the criteria that set apart phonological from non-phonological computation in Natural Phonology: the former produce processes (natural, innate, either obligatory or optional, style- and tempo-dependent, apply involuntarily and unconsciously, also to slips of the tongue, are not markers of any grammatical value), the latter rules (conventionalized, learned, style- and tempo-independent, always obligatory, do not apply to slips of the tongue, serve grammatical functions).

Natural Generative Phonology requires a phonetic interpretation of (1b): phonological processes must be phonetically transparent and hence surface-true (the True Generalization Condition, Hooper 1976a: 13 ff.). Government Phonology and Natural Phonology do not share this view. In Natural Phonology, phonetic factors also include intention, which may not be realized: a vowel may be nasalized before nasals that are deleted on the surface. Also, phonetic factors include tempo, style, effort, and attentiveness to speech. In Government Phonology, conditioning factors are *never* phonetic: they are only phonological. The GP version of (1b) thus, rather, assumes that an alternation cannot be phonological if it has conditioning factors that are non-phonological (i.e. morphological).

(1c) is an explicit condition on phonological processes in Government Phonology: 'non-arbitrariness: There is a direct relation between a phonological process and the context in which it occurs' (Kaye et al. 1990: 194; see also Gussmann 2007 and Pöchtrager 2006: 19 ff.). In NP, the causality between the triggering context and change appears in the opening sentence in Stampe's (1972: 1) dissertation: '[a] phonological process is a mental operation that applies in speech to substitute, for a class of sounds or sound sequences presenting a specific common difficulty to the speech capacity of the individual, an alternative class identical but lacking the difficult property.'

The small-is-beautiful option thus shrinks phonology: most of what SPE thought is due to phonological computation (say, 90 percent), is in fact something else (lexicalized alternations, morphophonology, allomorphy, possibly analogy).

### 19.3.2 Big is Beautiful

Hale & Reiss (2000a,b, 2008) take exactly the opposite position: 'big is beautiful'. Their work most clearly expresses the 'abstract' tradition which holds that phonological processes are phonetically arbitrary, arguing that phonological computation does not care for the objects that are manipulated, or for the causal relationship between the triggering context and the change observed: anything can become anything in any context. That is,  $X$ ,  $Y$ , and  $C$  are interchangeable in  $X \rightarrow Y / C$ :  $n \rightarrow \eta / \_ k,g$  is as good a phonological process as  $n \rightarrow \eta / \_ p,b$  and  $n \rightarrow m / \_ k,g$ , or  $n \rightarrow p / \_ \eta$ .

On this perspective, substance is entirely divorced from phonological computation. Substance (or melody) is everything that occurs below the skeleton, i.e. depending on the theory, binary or monovalent features, or unary primes. The substantive/melodic world works as Ohala assumes, Hale & Reiss (2000a: 162 f., 2008: 169 f.) argue: phonetics already accounts for the alternations of this type. Allowing the phonology to do this work again would be what they call 'substance abuse'. The restrictions that phonology, a purely cognitive system, imposes on phonological computation are of a different kind: they concern everything that substance, i.e. phonetics, is not responsible for. Syllable structure is an example: there is no phonetic rationale for syllabic conditioning. Syllable-related processes are thus truly phonological in kind, and the occurring and non-occurring patterns must follow from genuinely phonological restrictions on the

computational system. The same holds true for the absence of outlandish logical possibilities such as 'stress every prime-numbered syllable'.

Given that nothing objects against melodically outlandish rules such as  $n \rightarrow \eta / \_p, b$ , Hale & Reiss need a reason why there are substance-related universals at all: many logically possible patterns do not occur in natural language. The answer, Hale & Reiss (2000a, 2008: 158 f.) argue, lies outside of phonology, and also outside of synchronic computation: substance is phonetics, and non-occurring patterns are due to the fact that phonetics does not produce them. They endorse the widespread idea (which is dubious, see Foulkes & Vihman, this volume) that sound change is due to misperception in the course of first-language acquisition. A rule such as  $n \rightarrow \eta / \_p, b$  could thus perfectly well exist in phonological computation, but it does not occur because phonetics/misperception does not produce the relevant pattern. In other words, substance-related universals are accidental, rather than systematic gaps at the phonological level: they are due to the fact that children's misperception is based on universal properties of the phonetic signal and universal properties of the human system of sound perception.

#### 19.4 EVIDENCE AGAINST SMALL IS BEAUTIFUL: CRAZY RULES

Evidence that supports the big-is-beautiful perspective comes from so-called crazy rules. Bach & Harms (1972) use this term for rules that make no phonetic sense. A number of cases have been reported in the literature: see Buckley (2000, 2003) on Southern Pomoan ( $i \rightarrow u / d \_$ ), Vennemann (1972a) on Sanskrit (palatalization before a), and Hyman (2001: 147 ff.) on Ndebele (where labials palatalize before w).

The goal of Bach & Harms (1972) is to understand how crazy rules come into being. They assume that crazy rules are not crazy at birth: they are perfectly plausible at first (in the sense that the trigger and the effect are plausibly related phonetically), but then a diachronic substitution occurs (of X, Y, and/or C in  $X \rightarrow Y / C$ ) that creates craziness without the language reacting against this departure from (phonetic) transparency. That is, craziness is a property of rule *change*, rather than of rules: 'some rather strong plausibility conditions seem to play a crucial part in determining what rules a language can initiate, these same conditions do not seem to bear any relation to changes that take place in rules' (Bach & Harms 1972: 6).

The literature often makes a parallel with fashion, where an originally functional piece of clothing may lose its function but continues to exist because it now represents the social status or group identity of the bearers (e.g. Postal 1968: 283, Lass 1997: 326, Calabrese 2005: 46 f.).

Bach & Harms (1972: 16 ff.) discuss a crazy rule in the Oboyan dialect of Russian: like in Russian and other Eastern Slavic languages, the vowel inventory is restricted in pre-tonic position. But the specific pattern in Oboyan is strange: after palatal

consonants, pre-tonic non-high vowels appear as [i] if the following stressed vowel is [ɛ, ɔ, a], while they are [a] in case the following stressed vowel is [e, o, i, u]. If anything, the reverse is expected. Bach & Harms argue that the key to the problem is the fact that [ɛ, ɔ] were high vowels in Common Slavic, so-called yers which today alternate with zero, and which are also said to have been lax: [ɪ, ʊ]. Hence the original rule would produce [i] before high lax vowels (i.e. some kind of schwas), and [a] before all other vowels. This is significantly more plausible: the colour of the vowel that is weakened because of its pre-tonic position is entirely determined by the preceding palatal if there is no influence through vowel harmony from the following vowel, i.e. when this vowel is a colourless schwa itself. Otherwise [a] is encountered.

In further diachronic evolution, a context-free rule has changed all high lax vowels of the language into [ɛ, ɔ] (so-called yer vocalization). Bach & Harms argue that facing this situation, speakers have adapted the rule according to the phonetic properties of the new triggers: [ɛ, ɔ] are [+low]. Under this reanalysis, [+low] vowels are triggers, and this extends to independent [a] which was never a trigger diachronically speaking. Speakers do not care for these considerations though, and happily use the modified rule, which has become crazy and is now triggered by [ɛ, ɔ, a].

Bach & Harms (1972: 9) explicitly blame spontaneous sound shifts for making plausible rules implausible: '[w]here we have documentary evidence we find that this further shift [i.e. which causes craziness] occurs later, and rather significantly, it generally occurs by context-free rule.' That is, individual vocabulary items (X, Y, and/or C in  $X \rightarrow Y / C$ ) are arbitrarily replaced by diachronic evolution without this changing anything in the operational character of the rule.

Typical discussions of crazy rules do not provide information regarding regularity, productivity, and possible morphological conditioning. The Sardinian case presented in the following section clearly displays all the characteristics of a synchronically active, productive, and morphologically unconditioned rule. In addition, the dialectal continuum allows us to control for diachronically intermediate stages, which faithfully reproduce Bach & Harms's scenario of how crazy rules come into being.

#### 19.5 SARDINIAN L → ʎ IN EXTERNAL SANDHI

##### 19.5.1 Context

The Sardinian dialects constitute a well-studied body of evidence: dialectologists have produced descriptions since Wagner (1941); Contini (1987) offers a particularly detailed picture. All data are oral, gained through elicitation. Also, sociological factors and language contact are typically controlled for.<sup>10</sup>

<sup>10</sup> The discussion below closely follows Molinu (2009), where more detailed material and literature can be found. Lucia Molinu, to whom I am indebted, has confirmed data beyond that considered here.

The Sardinian evidence is particularly apt for our purpose since it offers an unquestionable guarantee for the synchronic character of the alternation observed: external sandhi. Phonological rules that apply across word boundaries (which are thus treated as if they were not there) is an areal feature that Sardinian shares with Middle Italian dialects (Giannelli & Savoia 1978, Contini 1986, Dalbera-Stefanaggi 2001, Marotta 2008). Processes that apply in external sandhi cannot be due to lexicalization since word sequences are not stored in the lexicon (except for idioms and the like, see note 4). That is, were English velar softening to apply across word boundaries (e.g. /I like it/ → [aj lajs it]), there would be no way to escape the conclusion that it is a synchronically active rule.

### 19.5.2 The Alternation in Genoni

The Genoni dialect of Sardinian has a crazy rule that applies in external sandhi, and may also be observed morpheme-internally in diachronic evolution (i.e. through comparison with Latin):  $l \rightarrow \text{ɣ} / V\_V$ .<sup>11</sup> (2) provides illustration (# represents a word boundary, ## an utterance boundary).

#### (2) Genoni Sardinian $l \rightarrow \text{ɣ} / V\_V$

##### a. in external sandhi after C-final words

##__	C#_V	gloss
'lampaðaza	in 'lampaðaza	June, in June
'lettu	i 'llettuzu	bed, the beds
'longu	'vu 'llongu	long, it was long

##### b. ##\_\_ C#V gloss

##__	C#V	gloss
o'riyaza	iz o'riyaza	ear, the ears
a'fu'γau	'kaŋðu 'vuð a'fu'γau 'bēi	dried, when (the wheat) had well dried

##### c. in external sandhi after V-final words

##__	V##_V	gloss
'laðru	su 'kaðru	bacon, the bacon
'liŋwa	sa 'riŋwa	tongue, the tongue

<sup>11</sup> Descriptions do not agree on whether the result is a uvular [ɣ] or a pharyngeal [ʕ] fricative. Bottiglioni (1922: 37), Pellis (1934: 68), Wagner (1941: §188), Bolognesi (1998: 465), and Molinu (2009) report [ɣ], while Contini (1987) and Cossu (2000) transcribe [ʕ]. Contini (1987 I: 355 note 2, 2006: 192) admits variation between [ɣ] and [ʕ] based on the vocalic context and speech rate (Molinu 2009: 133, note 7 describes this variation). I assume [ɣ] here; the variation is irrelevant for the argument: a pharyngeal would only make the rule crazier.

'littera	ūa 'ɣittera	letter, a letter
'lonɣa	'braβa 'ɣonɣa	long, long beard
li'mōi	'binti ki'mōizi	lemon, twenty lemons
'lampaðaza	ε kkomini'fsau 'ɣampaðaza	June, June has just begun

#### d. word-internally in intervocalic position V\_\_V

Genoni	Northern Sardinian	Latin	gloss
'piku	'pilu	pilu(s)	hair
'tʃeku	'kelu	caelu(m)	sky
da'vori	do'lore	dolore(m)	pain
'orkia	'olia	oliva	olive

(2a) shows that word-initial  $l$  appears as such after consonant-final words, while it surfaces as  $\text{ɣ}$  after vowel-final words under (2c). Under (2a) the word [in] 'in' is overtly consonant-final; the presence of a word-final consonant in the words [i] 'definite article, plural' and ['vu] '(it) was', however, is not overt. It is established under (2b) where the two items are followed by vowel-initial words. In classical autosegmental terms, the word-final consonant of *is* and *fut* would be said to be floating: it is elided when the following word is sonorant-initial. Its position is preserved, though, and targeted by the following consonant, which spreads and appears as a geminate (note that word-initial consonants do not geminate after word-final stable consonants as in [in 'lampaðaza]).

The word-internal evidence in (2d) does not allow us to conclude anything regarding a synchronically active process  $l \rightarrow \text{ɣ} / V\_V$ : since there are no alternations, we may have underlying forms that bear the lateral (e.g. /pilu/ 'hair') or the uvular fricative (/piku/ 'hair').<sup>12</sup> The former abstract analysis would allow the rule to take a free ride on /pilu/, whose lateral would be absolutely neutralized. There is no evidence from other word-internal positions: the lateral has undergone unrelated changes in branching onsets (where it appears as  $r$ : Lat. *plenu(m)* > 'prēu 'full, flore(m) > 'frōri 'flower') and internal coda position (where it has also become  $r$ , but in addition was subject to metathesis, with subsequent gemination of the following consonant: Lat. *culpa* > 'kruppa 'mistake'). Finally, there is no evidence for final codas since there are no  $l$ -final words in Sardinian.

This said, (2d) shows that a diachronic evolution produced  $\text{ɣ}$  on the basis of the Latin lateral. The end point of this evolution is identical with the result of the synchronically active process under (2a–c).

<sup>12</sup> Relevant to this discussion may be that Genoni has no opposition between [ɣ] and [l]: the former is always a contextual variant of the latter (rather than the reverse, at least diachronically). Contini (1987: 551, 559), however, argues for underlying /ɣ/ in word-internal contexts (Molinu 2009: 149).

## 19.5.3 Diatopic Variation of the Alternation

Let us now consider the dialectal variation on the island regarding the result of the external sandhi rule in (3).<sup>13</sup>

- (3) diatopic variation in Sardinia:  
word-initial l in external sandhi after V-final words

dialect	result	utterance-initial	V#_V	Gloss
a. North (Budduso)	l	'luna	sa 'luna	moon, the moon
b. Genoni	ɣ	'latti	su 'ɣatti	milk, the milk
Sestu Campidanese	ɣ	'luʒi	sa 'ɣuʒi	light, the light
c. San Vito	ʔ	'lūʔā	sa 'ʔūʔā	moon, the moon
d. Nurachi	zero	'limba	sa 'imba	tongue, the tongue
e. Sanluri	w	'linna	sa 'winna	wood, the wood
f. Cruccuris	β	'lonʒu	ū gu'tedʒu 'βonʒu	long, a long knife
g. Gesturi	g <sup>w</sup>	'leppuri	su 'gweppuri	hare, the hare

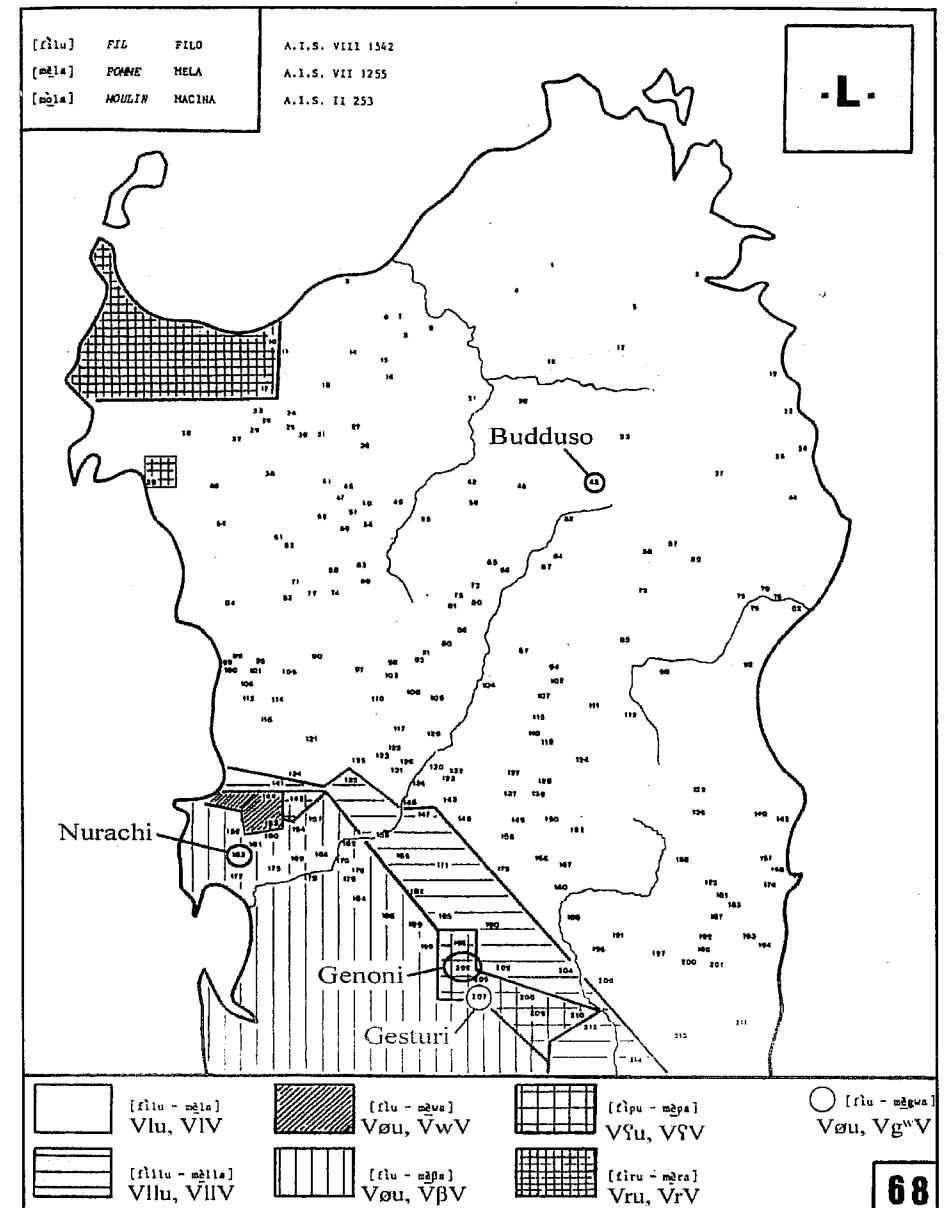
The north of the island does not show any alternation of l in external sandhi. The table is further divided into those varieties where the absolute regularity of the alternation in external sandhi is confirmed in the fieldwork of the authors: l ~ ɣ in Genoni (Molinu 1998, 2009) and Sestu Campidanese (Bolognesi 1998: 43–4), l ~ ʔ in San Vito (Contini 1987 I: 486, note 48). By contrast, the regularity across the lexicon of the alternations under (3d–g) for any given variety is not warranted: existing descriptions (Contini 1987; Viridis 1978) report cross-lexical variation (also across individual utterances) whereby the most frequent realization in ... V#\_V ... is the lateral; the resulting segments mentioned are less frequently observed.

The map in (4) shows the distribution of the varieties.<sup>14</sup>

<sup>13</sup> Data are from Contini (1987 I: 485–6, Budduso, Nurachi, Gesturi, San Vito, but Contini does not represent the latter in his map that is shown here in (4)), Wagner (1941: §194, Sanluri, Cruccuris), Bolognesi (1998: 43–4, Sestu Campidanese), Molinu (2009, Genoni) (see also Viridis 1978: 55 ff.). Very similar variation is found in Occitan dialects of the Massif Central (France) where Latin [l] appears as [l], [w], [g<sup>w</sup>], [g], [ɣ], and [ɣ] in intervocalic position (Dauzat 1938: 63).

<sup>14</sup> Numbers are points of inquiry (villages) where Contini conducted fieldwork. The map only shows word-internal intervocalic l, and systematically distinguishes between l before u (witness word: *filu* 'thread') and before other vowels (witness word: *mela* 'apple'). In the former context *w* and related reflexes are often missing altogether. Also note that 'p' which Contini uses for the transcription in the Genoni area stands for ɣ.

- (4) Latin l in Sardinian according to Contini (1987 II: map 68)





I first identify those mechanisms that cannot be held responsible. The occurrence of the alternation in external sandhi guarantees its synchronic (rather than diachronic) character. Together with the fact that it is 100 percent regular in relevant dialects,<sup>15</sup> it also leaves no room for a lexicalization-based solution (sequences of words are not stored in the lexicon).

Remaining non-phonological candidate mechanisms are (1) a morphophonological computational system (MP rules in NGP), (2) allomorphy, and (3) analogy. The alternation does not show the hallmarks of analogy: all relevant items are concerned (rather than a subset), and there is no impact of type- or token frequency. Allomorphy is not an option either: morphemes (and possibly phonological conditions) select allomorphs, but words do not select 'allo-words'. A selection of the root alone by the preceding word is not workable since affixes are merged before independent words become visible to the derivation. Also, alternations that are the result of morphophonological or allomorphic computation require some morphological condition. Since the Sardinian *l-ɾ* does not have any, it cannot be the result of these devices.

The successive elimination of candidate mechanisms leaves us with just phonological computation. This is true for NGP and GP, but not quite for NP. As was mentioned earlier, in NP the computational system that manages alternations which in NGP and GP are morphophonological is not defined by the fact that these alternations have morphological conditions. Processes (phonological computation) are natural, while rules (non-phonological computation) are conventional. Patricia Donegan (p.c.) *expects* conventionalized alternations to bear morphological conditions, but this is not a necessary property of rules. NP can thus interpret Sardinian *l-ɾ* as a conventionalized rule. Note that it could not be a natural process since these, on NP standards, always have a plausible causality (see (1c)).

The same result arises from NGP's and GP's requirement that phonological computation is 100 percent regular (see (1a)): since this is the case for Sardinian *l-ɾ*, the two theories must consider it phonological. NP, on the other hand, allows the results of phonological computation to be non-surface-true. As regularity is no criterion for NP to decide whether an alternation is natural or conventionalized, Sardinian *l-ɾ* may be either.

It thus appears that NGP and GP are forced to recognize Sardinian *l-ɾ* as a truly phonological alternation. In other words, they will have to acknowledge that Hale and Reiss's 'big-is-beautiful' position is correct. NP has an escape hatch. In NP, the only evidence that allows us to decide whether alternations are natural or conventional are slips of the tongue, language games, and L2. Because only alternations with a plausible causality qualify as natural processes, NP predicts that Sardinian *l-ɾ* will not be carried over into these three areas.

<sup>15</sup> Authors are explicit on this. Contini (1987 I: 485–6 notes 47–8) for example claims absolute regularity for Nureci (point of inquiry 195), Nuragus (205), Isili (208), Nurri (210), and Orroli (212) (all *l-ɾ*), as well as for San Vito (1 ~ ?).

## 19.6.2 OT

It was mentioned in section 19.3 that (a few initiatives aside) OT is not really concerned with the question of how to identify possible phonological processes. Parallel to *SPE*, OT simply analyses all alternations in terms of phonological constraint interaction. Hence it seems that OT is on the big-is-beautiful side by the simple absence of reflection, and therefore receives support from crazy rules in general and Sardinian *l-ɾ* in particular. Appearances are deceptive, though: the consequences for OT may turn out to be more dramatic than for other theories because two cornerstones of the theory, universal markedness and the finite character of the constraint set, are impacted.

If crazy rules exist in phonological computation, they need to be managed by constraints. The set of constraints, however, is supposed to be finite and universal: if anything can be a crazy constraint, there must be as many constraints as there are crazy rules, which makes the constraint set potentially infinite. One way out would be to go along with *SPE* and mimic the diachronic evolution (which is not crazy) in the synchronic analysis (i.e. first  $k \rightarrow \text{fs}$ , then  $\text{fs} \rightarrow s$ ). This, however, would be a difficult strategy to implement in OT: a given constraint ranking (and hence even a stratal version of OT) is unable to produce intermediate derivational stages. Facing this difficulty, Bolognesi (1998: 464 ff.) gives up on the universal and finite ambition of the constraint set: he allows for language-specific constraints.

Regarding markedness, Bermúdez-Otero (2006b) points out that if the melodic properties of phonological processes are arbitrary, the entire justification of markedness constraints disappears. A reaction parallel to Bolognesi's is explored by Boersma (1998) and Bermúdez-Otero & Börjars (2006) who argue that markedness constraints are acquired/constructed on the basis of available data, rather than innate. Note that this solution also abandons the finite character of the constraint set. Facing the same problem, Green (2003, 2004) takes a different direction: regarding the notorious Celtic mutations, he argues that universal markedness is the yardstick for phonological computation: alternations that cannot be done within the limits of universal markedness are not phonological in kind.

## 19.6.3 Phonetic or Substantive Reductionists

Finally, it is useful to recall that crazy rules are traditionally used in order to argue against the phonetic determinism of phonological rules, and in favour of the existence of an autonomous phonology, i.e. a computational system that does not care for the phonetic properties of the items that it manipulates (e.g. Anderson 1981, Hyman 2001). Phonology is phonetically arbitrary, as Bermúdez-Otero (2006b: 498) puts it.

That is, crazy rules provide evidence against phonetic (or substantive) reductionists (as Bermúdez-Otero calls them), i.e. voices like John Ohala's (1983, 1992) who deny the existence of a phonological system that is independent from phonetics and claim that *all*

variation in sound that we see is substantive in nature. The functionalist and phonetically oriented 'grounded' strand of OT that was popular in the late 1990s and early 2000s (cf. 'inductive grounding', Hayes et al. 2004) also comes close to this position.

On a different but related count, Dependency Phonology also faces problems: John Anderson (2011, vol. 3) holds that 'all aspects of linguistic structure are grounded in non-linguistic mental 'substance' (first page of the book), and also that '(a) the categories of phonology are phonetically grounded; (b) the categories of syntax are semantically grounded' (p. 10). Finally, we might note that crazy rules certainly do anything but support Port & Leary's (2005) idea that there is no need for any (formal) phonology in grammar.

#### 19.6.4 Experimental Evidence: Are Phonetically Plausible Alternations Easier to Learn?

In recent years, artificial language experiments have tested whether natural patterns are learned more easily than unnatural patterns. Results are inconclusive: while Wilson (2003), Peperkamp et al. (2006), and Hayes et al. (2009) find that there is a learning bias in favour of natural alternations, Pycha et al. (2003), Peperkamp & Dupoux (2007), and Seidl & Buckley (2005) report that phonetically plausible and arbitrary stimuli are learned with equal ease.

Factors that may stand in the way of converging results are (1) what authors exactly understand by 'natural' and (2) whether the experiment is conducted with adults or with infants. Regarding the latter, Seidl & Buckley (2005) take exception to the other experiments by working with nine-month-old infants. Regarding the former, Peperkamp et al. (2006) use three criteria that make an alternation natural: (1) phonetic proximity (A and B in  $A \rightarrow B / C$  are phonetically close), (2) contextual relevance (C is 'homogeneous' with respect to the properties of A that are modified), (3) markedness reduction (the string containing B is less marked than if it contained A). This is certainly one way to interpret 'natural', but we have seen that the three criteria may or may not be used by theories (phonetic proximity is used by none that is discussed above, and the purely surface-based calculus of markedness will make, say,  $\int \rightarrow p$  natural if p improves the markedness of the string that it is surrounded by).

Be that as it may, all these tests do not speak to our main issue: how do speakers manage crazy alternations synchronically? As independent lexical entries or by a computational mechanism, and if the latter, by which kind of computation exactly? What the experimental evidence may be able to show, if a learning bias in favour of 'natural' alternations turns out to be compelling, is the existence of what Moreton (2008) calls an analytic bias, i.e. a grammatical (or more broadly cognitive) predisposition to accept 'natural' patterns. This does not mean that speakers are unable to learn and accommodate crazy rules, but it may be held responsible for their typological rarity (Moreton 2008). Bach & Harms's (1972) alternative explanation is that it takes the coincidence

of quite some diachronic events to produce a crazy rule (while non-crazy rules are non-crazy without any diachronic telescoping).

## 19.7 WHAT IS CRAZY AND WHAT IS NOT

### 19.7.1 Crazy Rules Only Concern Melody, Never (Syllable) Structure

The literature on crazy rules is eclectic and there does not appear to be a synoptic collection of cases that are on record. Also, descriptions do not usually provide information regarding the checklist that determines the impact on different theories of synchronic phonology. This being said, a striking convergence of all crazy rules that I have found is that they concern only melody. A, B, and C in a rule  $A \rightarrow B / C$  may change over time in unpredictable and arbitrary ways, but only if they are melodic items: there does not seem to be any equivalent for syllable structure or stress placement. That is, crazy rules which operate compensatory shortening, place stress on light but not on heavy syllables, lengthen vowels in closed syllables, or shorten them in open syllables do not appear to exist (recall note 3). No more than, say, a vowel-zero alternation whereby *zéro* is observed in closed syllables, while the vowel occurs in open syllables. There is only compensatory lengthening, closed syllable shortening, open syllable lengthening, and the regular distribution of vowels (in closed syllables) and zeros (in open syllables) in vowel-zero alternations (see Scheer 2004a: §§16, 416, 470 for an overview).

If this is true, we simply have another diagnostic for the fundamental difference between melody and structure in phonology. Recall that Hale & Reiss (2000a, 2008) propose this division for the split between phonology and substance, the latter escaping grammatical well-formedness restrictions. The same opposition between the areas below and above that skeleton is made by McMahon (2003) for different reasons. Also, I argue (Scheer 2004a: §§215–16, 229) that items below this line of division, i.e. melodic primes, are categories that combine phonetic and phonological properties: they entertain a non-arbitrary relationship with vocal tract anatomy and/or acoustic properties of the signal (which is possibly due to phylogenetic conditioning: had human language used vision/signing for some hundred thousand years, the categories may not look the same today), but are still manipulated by the grammatical system. By contrast, items above the skeleton are not liable to any extra-grammatical constraints. This is where Saussurian *langue* and Chomskyan competence lies: only items and processes above the skeleton have a chance to be universal (also across modalities, i.e. vocal and signed expression of the language faculty) and hence to qualify for UG. Grammatical restrictions can therefore only be expressed at this level.

A list of phenomena that are insensitive to melody or unable to impact it (or both) is established in Scheer (2012): phonology-free syntax (Zwicky & Pullum 1986) is in fact

melody-free syntax (syntax can be impacted by properties above the skeleton, but not by melody, Scheer 2011b: §§412, 660), infixation and allomorphy (which may be sensitive to phonological properties, but never to melody), category-sensitive phonology (nouns, verbs, or adjectives produce specific phonological patterns, but never impact melody), stress and syllable structure (which may be sensitive to positional factors, but never to whether a segment is palatal, velar, etc.—note that sonority has been independently identified as not behaving like a melodic prime).

### 19.7.2 Melodic Arbitrariness Concerns Only Input-Output Relations

The melodic arbitrariness of input-output relations does not mean that nothing needs to be represented below the skeleton, or that all melodic properties and processes are arbitrary. For example, phonological primes have a non-arbitrary identity: some cross-linguistic slack in the melodic representation of segments notwithstanding, an [e] cannot be [-back] in one language (or contain I if privative primes are used), but [+back] (or be made of U alone) in another.

Also, melodic properties play a role in phonological processes beyond the fact that they are subject to input-output transformation: rules may refer to natural classes of segments in either the definition of the input set or the definition of the triggering items. A naturalness requirement for these natural classes can be maintained even if the transformation of segments (or of a natural class of segments) into some other segments is arbitrary. This distinction is suggested by Bermúdez-Otero & Börjars (2006). Consider for instance the ruki-rule: Indo-Iranian and Balto-Slavic develop *ʃ* (or *x* in Slavic) from *s* when this segment is followed by either *r*, *u*, *k*, or *i* (e.g. Beekes 1995: 134–5). This alternation fails as a case of phonological computation since *r*, *u*, *k*, and *i* cannot be construed as a natural class. The change from *s* to *ʃ/x* is covered by the arbitrariness of melodic transformations, and so is the causal relationship between the triggering environment and the effect observed. However, the fact that the triggering environment is a non-natural disjunction is not.

In sum, the only thing covered by the license for melodic arbitrariness is the transformation of one melodic item into another. All other melodic properties may be argued to be still under grammatical control.

## 19.8 CONCLUSION

Our inquiry into regularity and naturalness essentially leads us to the conclusion that the idea that *SPE* was mistaken because it massively overgenerates was wrong. *SPE* was right: phonological computation can transform anything into any other thing in any context. This is what Hale and Reiss have maintained, and it is what the external sandhi evidence from Sardinian suggests.

In other words, there is no synchronic device that enforces naturalness. In its absence, the naturalness that we perceive in a fair number of rules can only have a diachronic origin. 'Diachronic' in this context refers to the shape of rules when they are born, i.e. upon innovation: they are always regular and natural then. Also, they are as synchronic at this point as they are a couple of centuries later when they have aged. It is this aging process of rules (Bach & Harms's 1972 idea) that introduces irregularity, opacity, unnaturalness, and may produce what is called crazy rules.

In this sense, there is no such thing as suspicious rules: all rules are equally well-formed from the point of view of grammar. What is taken to be suspicious (*k* → *s* for example) always has a diachronic explanation: several independent steps, each plausible and natural, have produced a suspicious or crazy rule that was plausible when it was young.

There is thus no way to understand synchronic patterns in absence of a diachronic analysis. Or, put differently, sometimes there is a whole lot of diachrony in synchronic phonology. In this respect again, *SPE* was right and its critics of the 1970s and 1980s wrong: *SPE* was criticized because it argued that modern English speakers were equipped with Middle English underlying forms and rules that recapitulate historical events of the past millennium. *SPE* did indeed assume that rules such as velar softening, which entered the language in the eleventh century, are still active today. If relevant alternations are not lexicalized, the result of allomorphy or analogy, the eleventh-century rule may indeed still be active today.

*SPE* may have been wrong, though, in holding that the rule sailed through the centuries without being modified, i.e. without aging. Following the early generative take on innovation that was mentioned in the introduction, *SPE* accounts for the modification of the output by adding an independent rule (context-free  $\bar{f}s \rightarrow s$  that applies after  $k \rightarrow \bar{f}s$ ). In other words, *SPE* tried to maintain the naturalness requirement of synchronic rules by mimicking their diachronic development. This led to a synchronic grammar that mimics historical events, and critics were right to pinpoint that.

The alternative suggested by Bach and Harms's general scenario, the Sardinian pattern, and the fact that input-output relations do not need to be natural, is that diachronic events are encoded in synchronic grammar not through intermediate stages, but through their flattening: the modification of the output of a rule is not due to the addition of independent rules, but to the modification of the vocabulary of the rule itself:  $k \rightarrow \bar{f}s$  becomes  $k \rightarrow s$ . Hence synchronic phonology stores quite some diachronic events, but in flattened, or telescoped manner. Sardinian children raised in Genoni have no evidence for any diachronically intermediate stages but still happily build a rule that transforms *l* into *κ*.

Finally, all that has been said in this conclusion so far only applies to melody, to those items that occur below the skeleton. Grammar is toothless for the transformation of melody, but does control syllable (and other) structure as well as stress assignment. This ties in with independent evidence for an ontological split between the areas below and above the skeleton: only the latter accommodates 'real' phonology, and it is only here that candidates for UG are found.



The perspective of Natural Phonology is entirely different, and the conclusions drawn above do not apply. Like structuralism, NGP, and GP, which oppose phonological and morphophonological computation, NP also builds on the architectural distinction of two distinct computational devices, which are different in kind. One produces alternations that have a plausible causality and enforces naturalness (but not 100 percent regularity or surface truth). This is what NP calls processes, which are the equivalent of true phonology in other frameworks. The other computational system does all the rest: it produces alternations that may have morphological conditioning and does not need to implement a plausible causality. In NP, these alternations are said to be conventionalized, i.e. freed from the naturalness requirement. Crucially, though, this computational system *may* implement morphological conditioning, but does not need to. Hence a purely phonological alternation like Sardinian l-ʀ may fall into its competence. This is not like other theories, where the equivalent computational system, morphophonology, necessarily works with morphological conditions.

We thus face two perspectives: both have two distinct computational systems that manage phonological alternations, but the line of division is not the same: pure phonology vs morphophonology on the one hand (structuralism, NGP, GP), natural vs conventional on the other (NP). The impact of crazy rules in general and of Sardinian l-ʀ in particular on phonological theory depends on this architectural choice: while NP can claim small-is-beautiful for phonological computation, other theories are forced into big-is-beautiful.

## CHAPTER 20

# AN I-LANGUAGE APPROACH TO PHONOLOGIZATION AND LEXIFICATION

MARK HALE, MADELYN KISSOCK,  
AND CHARLES REISS

## 20.1 INTRODUCTION

ADOPTING the I-language perspective (e.g. Chomsky 1986) in diachronic linguistics makes it difficult to talk about even the most well-established results and the simplest notions. A statement like 'the English word *knight* began with a *kn* cluster in Middle English' seems innocuous enough, until we realize that there is no scientifically useful or coherent definition of 'English', or 'Middle English', or 'the word *knight*!' There is obviously no direct sense in which a mental representation in Chaucer's mind/brain (or even a sound Chaucer made) that we might refer to as 'Chaucer's word for *knight*' has turned into a representation or sound that we might refer to as 'Chomsky's word for *knight*'.

One strategy for dealing with such difficulties is to ignore them—it is shockingly easy, as we ourselves have done for much of our professional lives, to teach students and write papers as if English, Marshallese, and Vedic Sanskrit were legitimate objects of scientific study. Such work engages our intellect and provides a certain amount of satisfaction, but our acceptance of the correctness of the I-language approach, and the problems and inconsistencies that arise by not adopting that approach, sometimes force us to confront the contradictions that we have helped to perpetuate. There is no question that doing so is difficult and sometimes tedious, but the investment will generate a deeper understanding of what is traditionally called 'language change', and ultimately allow us to reformulate informal statements, like the one about 'the word *knight*', in terms that do not conflict with our basic scientific understanding of languages as properties of individuals.

A tradition among generative linguists interested in diachrony treats 'sound change' or 'phonological change' (or at least the most interesting types of these events) as resulting

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