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Substance Free Phonology

Charles Reiss
Concordia University
charles.reiss@concordia.ca

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1 Introduction

Imagine a theory of phonology that makes no reference to well-formedness, repair, contrast, typology, variation, language change, markedness, 'child phonology', faithfulness, constraints, phonotactics, articulatory or acoustic phonetics, or speech perception. What remains in such a phonological theory constitutes the components of the Substance Free Phonology (SFP) model I will sketch here. My task thus involves not only justifying the exclusion of all those domains, but also arguing that something remains that is worthy of the name 'phonology'. In support of the latter task, I'll provide some positive examples of recent research in SFP. Of course, the assumption underlying this theory is that there exists a correspondingly narrow object of study in the world, a substance free phonological module of the human language faculty.[1]

To get an idea of what I mean by a substance free theory, let's assume that there are human languages that show word-final obstruent devoicing and languages that do not, but no languages that show word-final obstruent *voicing*. A theory of phonology is *substance free* if it *cannot* capture such apparently true generalizations, and it is not substance free if it can.[2] The fundamental assumption of SFP is that the former type of theory, a "clean" theory, is preferable to the latter, "substance abusing", type theory. A phonological theory *should not*, by itself, account for every true generalizations about attestable phonological systems. In particular, it should not account for generalizations about statistics of attested or attestable patterns of phonetic substance, even those that are presumed to be absolute, such as the (assumed here) impossibility of final

[1] Most of the ideas in this chapter are either discussed in my joint work with Mark Hale, including (Hale and Reiss, 2008), or grew out of this joint work or more recent work and discussion with Alan Bale, Dana Isac and others.

[2] This example is chosen for expository purposes and relies on an assumption of binary features. With privative features, it will sometimes be possible to model such typological asymmetries because of the inventory of the representational primitives, not because the computational system treats the primitives differentially.

voicing.[3]

The SFP approach accepts the existence of an innate, universal feature set. The features relate, via complex transduction processes, to phonetic substance (more on this later), but varieties of substance do not have different effects on the computational system—the formal properties of a feature deletion process do not change depending on whether it is +VOICED or -ATR that is being deleted. With this distinction of substantive features and a formal computational system, SFP is not significantly different from what Chomsky (1965, p.28) lays out in in *Aspects of a Theory of Syntax*:

> The study of linguistic universals is the study of the properties of any generative grammar for a natural language. Particular assumptions about linguistic universals may pertain to either the syntactic, semantic, or phonological component, or to interrelations among the three components.
>
> It is useful to classify linguistic universals as *formal* or *substantive*. A theory of substantive universals claims that items of a particular kind in any language must be drawn from a fixed class of items. For example, Jakobson's theory of distinctive features can be interpreted as making an assertion about substantive universals with respect to the phonological component of a generative grammar. It asserts that each output of this component consists of elements that are characterized in terms of some small number of fixed, universal, phonetic features (perhaps on the order of fifteen or twenty), each of which has a substantive acoustic-articulatory characterization independent of any particular language.
>
> ...Substantive universals such as these concern the vocabulary for the description of language; formal universals involve rather the character of the rules that appear in grammars and the ways in which they can be interconnected.

In phonology, a formal universal would be the discovery that the phonology of all languages is a complex function, the composition of a strictly ordered set of rules of some well-defined class, or some alternative computational system.

Below, I return to the source, nature and number of features, the substantive universals admitted by Chomsky and SFP. For now, note that in *Aspects* Chomsky's characterization of substantive universals has no bearing on the statistical distribution of typological patterns, putative patterns in the course of acquisition, or other, so-called markedness phenomena. The substantive universals just determine the content of the representations that are the arguments of the computational system. In the SFP view, phonological UG cannot contain a condition that, say, only segments that are voiced and rounded are subject to

[3]This discussion parallels early generative work on the contrast between grammaticality and acceptability, for example in Katz and Bever (1976). See the discussion in Chapter 1 of Hale and Reiss (2008) for the purview of UG, and the analogy with grammaticality in Chapter 11 of Isac and Reiss (2013).

deletion; or a condition that only, say, round, back, nasal and ATR features can participate in harmony processes—even if this were a true generalization about *languages*, we would not want to encode it in UG as a property of *Language*, the human language faculty.

2 Is *SPE* substance free?

Despite the clarity of *Aspects*, the idea that the phonetic substance, or "intrinsic content", of phonological features should be relevant to the formal component phonological theory was entertained late in *The Sound Pattern of English* (*SPE*) by Chomsky and Halle (1968):

> The problem is that our approach to features, to rules and to evaluation has been overly formal. Suppose, for example, that we were systematically to interchange features or to replace [αF] by [-αF] (where α is +, and F is a feature) throughout our description of English structure. There is nothing in our account of linguistic theory to indicate that the result would be the description of a system that violates certain principles governing human languages. To the extent that this is true, we have failed to formulate the principles of linguistic theory, of universal grammar, in a satisfactory manner. In particular, we have not made use of the fact that the features have intrinsic content. [400].

Chomsky & Halle are bemoaning the fact that their model developed in the previous 399 pages is "overly formal". The model could easily be used to describe a language with final *voicing*, for example. This call for a theory of markedness in generative phonology is perhaps responsible for inspiring most work in phonology for the last five decades, from the universal processes of Natural Phonology to the universal markedness constraints of Optimality Theory.

Note that Chomsky & Halle seem to be suggesting the pursuit of a theory of markedness that would complement the formal theory of rules they have developed. Thus, even with a theory of markedness, there would remain in *SPE* what we can anachronistically call a formal phonology *module* that would potentially be substance free.

But we don't really have to worry about whether or not the markedness theory alluded to in *SPE* would impinge upon the formal phonology component, since Chomsky & Halle, within the same Chapter, point out the futility of pursuing such a model:

> It does not seem likely that an elaboration of the theory along the lines just reviewed will allow us to dispense with phonological processes that change features fairly freely. The second stage of the Velar Softening Rule of English ... and of the Second Velar Palatalization of Slavic strongly suggests that the phonological component requires wide latitude in the freedom to change features, along the lines of the rules discussed in the body of this book [428].

In other words, there are rules and rule combinations that effect alternations that are surprising given the "intrinsic content" of phonological features. In yet other words, an adequate model of phonology must be *substance free*—we can't combine the formal theory with a markedness theory that actually constrains it.

3 Is Optimality Theory substance free?

In Optimality Theory (OT) (McCarthy and Prince (1993); Prince and Smolensky (1993); Kager (1999), *inter alia)* the evaluation of candidates via the EVAL function proceeds without regard to the content of the constraints. Only the ranking of constraints and the relationship of an input form to the candidate forms play a role in applying EVAL. This part of the model is substance free.

However, substance is abusively present in other parts of OT, most notably in the content of the constraints: "The basic idea we will explore is that Universal Grammar consists largely of a set of constraints on representational well-formedness, out of which individual grammars are constructed" (P&S). Since the constraint set CON is assumed to be universal, the model contains specific constraints, like ones that are violated by voiced obstruents in codas. Such constraints will yield final devoicing, when appropriately ranked *vis-à-vis* faithfulness constraints that are violated by mismatches between an input and a candidate output. However, CON does not contain a complementary constraint violated by *voiceless* obstruents in codas. The supposed benefit of this model is that the factorial typology, the set of languages describable by reranking the innate candidate set, matches, in principle, the ones we could find attested. By assumption, we can find languages with final devoicing, and languages without voicing alternations in codas, but no languages with final *voicing*. OT aims to capture such (presumably true) generalizations. This aspect of OT is substance abusing.

Much OT work explicitly appeals to phonetic, physiological and physical factors to explain the inclusion of a given innate constraint in CON. For example, McCarthy and Prince (1995, 88) refer to a constraint *VgV as the "phonologization of Boyle's Law", a law that governs the relationships among volume, pressure and temperature of a gas. Prince and Smolensky (1993) explicitly reject the extreme formalist position of a substance-free phonology:

> We urge a reassessment of this essentially formalist position. If phonology is separated from the principles of well-formedness (the 'laws') that drive it, the resulting loss of constraint and theoretical depth will mark a major defeat for the enterprise [(Prince and Smolensky, 1993, 216), see also p.3].

In other words, OT (at least as represented by most work of its three founders, McCarthy, Prince and Smolensky) advocates building into Universal Grammar, as constraints in CON, phenomena that have independent explanations via phonetic, physiological and physical factors. As pointed out by John Ohala in

various contexts (e.g. 1990) it is not better in science to have two explanations (phonetics *and* phonology) rather than one (*just* phonetics) for a given observation.

Alan Prince (2007) has more recently retreated from seeking justification for OT constraints in phonetic, physiological and physical factors:

> A constraint, in the intended sense, is a principle within a theory and, like any other principle in any other theory, is justified by its contribution to the consequences of that theory. Since OT is a theory of grammar, the consequences are displayed in the grammars predicted and disallowed—'typological evidence'. A constraint which cannot be justified on those grounds cannot be justified. Further, justifying a constraint functionally (or in any other extrinsic way) can have no effect whatever on its role within the theory. A constraint, viewed locally, can appear wonderfully concordant with some function [ease of articulation, ease of perception, *etc.*—cr], but this cannot supplant the theory's logic or compel the global outcome ('efficiency') that is imagined to follow from the constraint's presence, or even make it more likely.

So, Prince is no longer appealing to grammar-external phonetic, physiological and physical factors to ground constraints. Instead he is proposing that we just posit for Con the constraints that are needed to get the analyses we observe. We posit them because we need them. This *appears* to be good science, basically Occam's Razor. And it appears to to be an improvement over Prince's earlier Boyle's Law-type explanations, because there is no appeal to phonetic, physiological and physical factors to explain the ontogeny of constraints. However, this position is even worse than the earlier position.

Instead of merely duplicating in the innate content of Con explanations of typological patterns due to phonetic, physiological and physical factors, Prince's new view contains the same redundancy, but tries to make a virtue of ignoring it. The new approach does not ground constraints, but it leaves the overwhelming phonetic naturalness of many phonological processes and their overlap with OT constraints as an unacknowledged mystery. This is a step backwards that is anticipated by Prince's "explanation" of markedness presented in Tesar et al. (1999, p.305):

> The concept of linguistic markedness, or inherent complexity of structures, has played a significant role in linguistic thought in the twentieth century, though more often as an unformalized perspective or side issue within generative grammar. Optimality Theory rests directly on a theory of linguistic markedness: "marked", or linguistically complex, structures are literally marked as such by the constraints they violate.

This attempt to formalize markedness is reiterated on the next page: "The basic notion of a marked structure is directly built into the theory: a marked

structure is one receiving a violation mark by a constraint in Con." Again, if markedness is equated merely with constraint violation 'marks', it remains a mystery why so many of the constraints that assign those marks to structures seem to have good phonetic motivation.

The way forward, in the twenty-*first* century, is to abandon markedness, and to develop a modular theory that distinguishes incidental facts about phonologies (including statistical generalizations about the set of attested ones) from phonological facts (facts about a module of the human mind). The nature of speech perception and sound change drive to a great extent the distribution of patterns we find in the languages of the world, but these distributions are facts about particular phonological systems; they are not facts about phonological UG. Reiterating arguments made elsewhere, I'll try to clarify this distinction in the next section.

4 Formedness, a.k.a. Markednesslessness

Consider the strings in (1):

(1) a. The cat left.
b. *Cat the left.

Grammaticality is a relative notion. A string s is grammatical with respect to a grammar G if and only if G generates s. The string s may be grammatical with respect to G, but not grammatical with respect to another grammar F. In other words, despite common usage, there is no such thing as an ungrammatical sentence in this technical sense. A string is a sentence with respect to a grammar, or it is not.[4] From this perspective, calling (1a) a grammatical or wellformed sentence is redundant once we know that the grammar in question is mine—it is a sentence of my language; and calling (1b) an ungrammatical or illformed sentence is nonsensical. Sentences are not ill- or well-formed—they are just what grammars generate. Sentences are 'formed' and strings that do not correspond to sentences generated by a grammar are not 'formed'. Of course, it is conceivable that a grammar other than mine generates (1b) but not (1a).

SFP adopts for phonology the same view of the notion of wellformedness presented here for syntax, namely that it is an empty or even misleading notion for understanding grammar. It also rejects the idea that grammaticality is a gradient status. Underlying phonological representations are generated as the output of morphological operations such as concatenation; the resulting underlying forms are fed into a phonological grammar (a complex function); and surface representations result as the output of this function. Those output representations are grammatical in the sense that they are generated by a morphological and phonological grammar. A form like [bunt] might be grammatical as the output of a grammar with (transparent) final devoicing. A form like [bund] will

[4] Equivalently, a string is a sentence in a language or it is not. Of course, we really are interested in *structures* associated with these strings, since sentences are not just strings.

not be ungrammatical or illformed as output of that grammar. It just won't be formed. It won't exist as a surface form of the language.

Calling [bund] 'illformed' represents a basic confusion. The grammar outputs [bunt] as a possible form, so [bunt] is grammatical or *formed*. The form [bund] is a hypothetical form dreamed up by a linguist to demonstrate what is *not* in the intensionally defined set of grammatical forms generated by the grammar in question. There is no reason to ascribe to the grammar in question the property of assigning to [bund] any status whatsoever. We, the linguists, call it 'ungrammatical' to mean 'not generated'. There are an infinite number of things that are not generated by the grammar, for an infinite number of reasons. My simple perspective is that the grammar intensionally generates a set of forms, and any form we linguistis pull out of the air that is not in that set is not formed (by that grammar), by implication.

The notion of markedness or wellformedness is fundamental to OT and to many of its predecessors, and that is why these models fall prey to substance abuse. The rhetoric of wellformedness can be quite colorful, with reference to "conditions" and "cures" or "repairs" being commonplace:

(2) Phonological pathology

a. "The main contribution of the OCP is that it allows us to separate out condition and cure. The OCP is a trigger, a pressure for change" [Yip (1988, p.74)]

b. "Repairs have the function of converting phonological configurations marked as illicit by active constraints into licensed ones." [Calabrese (2005, 75)]

c. "OT takes on a difficulty that held back earlier approaches to naturalness: the *what* is phonetically difficult is not the same as the *how to fix it*." (Hayes and Steriade, 2004, 2)

Calabrese's reference to "active constraints" allows him to account for cross-linguistic variation in which representations are allowed to surface in a given language and which need to be repaired.

Calabrese discusses the point made by Hale and Reiss (2008, 1998) that it is strange to build constraints into UG against front rounded vowels or ejective stops. For speakers learning languages that have such sounds, say, French and Navaho, respectively, the UG-given gift is misleading, since these sounds do occur. For a speaker of English, such constraints are irrelevant since there is no reason for an English learner to ever posit them–they are not present in the input. We argued that UG, which is supposed to help solve the paradox of language acquisition and explain how kids learn language should not be full of hints that are at best irrelevant and at worst misleading. Calabrese's response (p.44) is interesting:

> I agree with [the] claim that the fact that the English child has the "knowledge" that ejective stops are marked is irrelevant from the

> point of view of the grammar that has been learned. Not so, however, for the Navaho learner where markedness predicts that more effort is needed to learn and produce the complex ejective stops. The point is that for human beings certain actions are more complex than others. Thus, for example, a double backward somersault is more complex than a cartwheel in gymnastics insofar as it requires more complex muscular co-ordinations. Learning how to perform this acrobatic stunt will thus involve a lot of training and effort so that this stunt will be learned only after the easier cartwheel. Once the training is achieved, the backward double somersault is easily performed, albeit still intrinsically complex, by a trained gymnast. Notice that it will be easily lost with the passing of time and that any small health problem will affect its implementation. The same can be said of phonologically marked segments. For the speaker of Navaho, obviously well trained in the pronunciation of this language, the ejective stops, although intrinsically difficult, will be easy to pronounce. In contrast, the English speaker, who has never been exposed to ejective stops, will experience problems if exposed to them in not having been trained in their pronunciation. This is what the presence of an active marking statement indicates, and the solutions that speakers will find to the problems posed by segments disallowed by an active marking statement will involve "grammatical" repairs. Resorting to grammatical repairs is the only way speakers have to deal with these segments other than learning how to pronounce them, which means deactivating the relevant marking statement.

Calabrese's discussion can be critiqued from many angles, but most interesting perhaps is the unjustified claim that ejective stops are obviously universally difficult to make (and the implication that Navaho speakers will lose the ability to make them if they don't practice for a long time!); and the further assumption that the presumed physical complexity of the action should correspond to a complexity in the mental representation of the action.[5] This appears to be a good example of what Pylyshyn (2003) calls the "the mistake of attributing to a mental representation the properties of what it represents," and the mistake appears to be shared by Hayes and Steriade, whose statement in (2c) suggests that phonetic difficulties have to be fixed by the grammar.

The mental representation of a mouse is not smaller than that of an elephant; the mental representation of a brick is not heavier than that of a feather; the mental representation of the diameter of an atom is not smaller than the mental representation of the diameter of the Milky Way; and even if we demonstrate that producing [p'] is more physically challenging than producing [p], it does not follow that the mental representation of the former is more complex than that of the latter and needs to be fixed. As I become ever more decrepit, the

[5] As far as I know, there is no well-developed model of the mental representation of human physical actions. Do we know for example whether such representations can contain recursive structures?

"passing of time" and "small health problems" may have degraded my capacity to dunk a basketball or perform a gargouillade, but this is not a matter of mental representation.

It should be obvious that without a notion of well-formedness, there is no sense to the idea of the grammar optimizing output forms in any way. The grammar generates, not the best form, but just the form it generates. Other forms are not worse or less optimal, they just are not generated. The idea of well-formedness led OT into what we might call the *fallacy of imperfection* (in response to McCarthy & Prince's (1994) *fallacy of perfection*). For more general critique of the use of constraints in linguistics, see Reiss (2008) and relevant parts of Hale and Reiss (2008).

SFP rejects all repair and optimization approaches to motivating both language specific and universal phonological computations, because there is no useful sense in which linguistic representations are broken or ill-formed and need to be fixed. Mental representations either exist or they don't; forms are either generated by a grammar or not. There is no sense in which a mental representation can be illformed or wellformed, any more than a molecule can be illformed or wellformed. An existing molecule is compatible with the laws of physics; an "illformed molecule" that violates the laws of physics is no molecule at all—it does not exist. If a representation exists, it is a possible representation and does not neeed to undergo repair. Note that even mental representations of physically impossible objects, like the Devil's Triangle or an Escher print of an impossible staircase are well-formed *qua* mental representations.

SFP rejects language specific and universal notions of wellformedness for the simple reason that grammars map to outputs from inputs. By invoking a hypothetical form h that is not an output of a grammar G, we don't magically endow h with a status relative to G. In sum, the notion of wellformedness, or equivalently, markedness, needs to be rejected to achieve a substance free theory in any linguistic domain. We need to accept markednesslessness, the non-existence of markedness, to progress in phonology.

Mark Hale and I have elsewhere (1998; 2008) written about the irrelevance of so-called child phonology to discussions about markedness and complexity—let's just point out here that the work on infant speech perception (e.g. Werker 1995) guarantees that that Navaho children will distinguish ejectives from non-ejectives, and thus they have the capacity to represent the distinction. Calabrese's suggestion that "a lot of training and effort" is required for kids to have *represention*s of ejectives is basically a reversion to Piagetian views of cognitive development as dependent on sensorimotor experience (see Karmiloff-Smith (1992) for critical yet sympathetic discussion of Piaget). Once again, it should be pointed out that the SFP perspective is not new, since Chomsky made the same point in 1964, despite his close contact with Jakobson whose work (*e.g.*, Jakobson 1971) shows that he clearly believed that observation of child speech was revealing of deep generalizations about phonology.

Chomsky (1964, p39) commenting on a conference paper on child "phonology" notes that there is

> a general tendency to oversimplify drastically the facts of linguistic structure and to assume that the determination of competence can be derived from description of a corpus by some sort of sufficiently developed data-processing techniques. My feeling is that this is hopeless and that only experimentation of a fairly indirect and ingenious sort can provide evidence that is at all critical for formulating a true account of the child's grammar (as in the case of investigation of any other real system)... I make these remarks only to indicate a difficulty which I think is looming rather large and to which some serious attention will have to be given fairly soon.

It is gratifying to see that Neil Smith, one of the most prominent and careful scholars of child "phonology" has most recently re-evaluated the nature of the evidence to conclude, consistent with the SFP approach, that child speech does not bear on phonological UG in the way that most work in the field has assumed, such as OT work on the emergence of the unmarked in child speech. Smith now favors the view that "the major determinant of children's divergent productions is performance rather than the competence" (Smith, 2010, 103).

Ron Kaplan (1987, p.346-7) raises Chomsky & Halle's concession for the need for a formal model unconstrained by substance to the status of a methodological principle for linguists, illustrated with his jocular, but useful Figure 2, reproduced here in (3), along with his discussion:

(3) The shape of a linguistic theory (Kaplan's Figure 2)

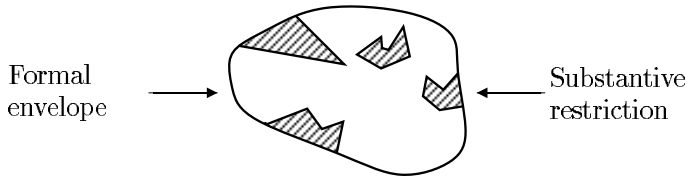

FIGURE 2 The shape of a linguistic theory

> A formal theory may have a relatively smooth outline ... [t]hen you start taking chunks out of it ... because you claim that no human language or grammar has such and such a property. ... It's a mistake to carry premature and unjustified substantive hypotheses into our computational and mathematical work, especially if it leads to mathematically complex, even if more restrictive, theories. ... [W]e should be wary of the seduction of substance.

Chomsky & Halle were uncharacteristically weak in their conclusions about markedness, to such an extent that their own students could later treat issues of substance as core, universal components of phonology in the form of OT markedness constraints against, for example, front round vowels and ejectives.

However, there were explicit calls for substance free phonology even around the time of publication of *SPE*, such as Fudge (1967): "phonologists (above all, generative phonologists) ought to burn their phonetic boats and turn to a genuinely abstract framework"; and Hellberg (1978): "A certain phonological rule may be perfectly well statable in terms of distinctive features. And more than that: it may to some extent have a good phonetic plausibility [but] it need not have any direct phonetic motivation whatsoever synchronically today." Fudge (2006) even recognizes an explicit substance free approach in the work of the Glossematicians Hjelmslev and Uldall, whose methodology was a general scientific approach in which "substance must be excluded from consideration" and one aims to "set up hypotheses about what must be the abstract formal system capable of accounting for the data". Considerations of substance, such as a feature system, are added on top of the fundamental work of characterizing a formal system.

So, why can't we just treat innate OT constraints on a par with the innate features of *SPE* (and most OT), as a complement to the formal system of ordered rules or EVAL or some alternative? To reiterate, if phonetic grounding (like Boyle's law) is supposed to play a role in determining the content of the constraints, then we have a duplication of explanation in phonetics and the content of CON; if phonetic grounding is irrelevant (Prince's more recent view) then the duplication is an unacknowledged mystery.

I have suggested (Reiss, 2008) that what Prince and Smolensky call the "principles of well-formedness (the 'laws')" of phonology, are actually just heuristics that we develop through our experience as linguists looking at lots of languages. In other words, they are not part of the ontology of phonology. For example, looking at a new language, we typically assume that it is likely that a sequence like [akra] has a syllable boundary before the stop-liquid cluster, rather than between the two consonants. This is because we seem to believe, rightly or wrongly that most languages 'maximize onsets' in such cases and leave the first syllable without a coda.

However, both syllabifications are found, for example, in the Ancient Greek dialects (Steriade, 1982). It may be useful to assume that the more common syllabification is present in a new, unfamiliar language until there is evidence to the contrary; and the guess will turn out to be correct more often than not, if our professional intuitions have any basis. However, we must take care not to confuse our intuitions concerning what happens often with the actual nature of the system under study. Based on our experiences and expectations, we apply our intuitions in attempting to solve the problems involved with analyzing data, but there is no reason to expect that these intuitions directly reflect the nature of the actual mental grammar constructed by a learner. The intuition that heavy things fall faster than light things is very useful when someone drops something from a window, but the intuition needs to be transcended to understand the workings of gravity. Heuristics are used by the analyst to make useful guesses about data, and guesses can be wrong. This is why OT constraints need to be violable—they reflect the fallibility of our guesses. If this perspective is valid, then the great innovation of OT, the violability of its constraints, represents a

basic category error—the constraints correspond to linguists' intuitions about what processes are common, not to the ontology of phonological UG. In this case, all the formal work on OT will have been for nought. Violable constraints will go the way of the ether.

5 Contrast

Another notion to dismiss as we construct SFP is *contrast*. This is sure to be yet another unpopular move: "Contrast... is one of the most central concepts in linguistics" Dresher (2009). It is important, before we proceed, to stress the distinction between (a) the components of our proposed model of the Human Phonological Faculty—our object of study; and (b) our methods for making hypotheses about that object—our sources of evidence, the heuristics we use. To use fancy names, we need to distinguish *phonological ontology* (Which asks *What are the components of universal and language specific phonologies?*) from *phonological epistemology* (which asks *How do we make discoveries and justify claims about universal and language specific phonologies?*).

Let's look at this distinction with respect to *contrast*. Contrast is *not* part of the ontology of SFP. However, practitioners of SFP have no qualms about referring to minimal pairs or the fact that French has a contrast between oral and nasal vowels. This is because we use our knowledge of the existence of semantic contrast and minimal pairs as a basis for hypotheses about the phonological content of elements in a French-type lexicon. We use minimal pairs epistemologically to justify our beliefs about French phonological ontology.

For SFP the notion of contrast is used as a heuristic, a tool to help us discover and understand phonology, whereas other work in phonology accepts contrast as part of the ontology of phonology. Work that appeals to contrast as part of the ontology of phonology falls along a spectrum of sophistication, however, most work fails to even consider the distinction I am making, or else explicitly rejects it. Some work, such as that of the Toronto school (e.g. Dresher (2009) and related work), is of course, quite complex in its argumentation for building contrast into the model of grammar. Other work, such as that of Flemming (2004; 1995) is refreshing in the honesty of its unabashed functionalism. Flemming (2004, p.232) is interested in "investigating the general character of the constraints imposed on phonology by the **need to minimise confusion** [which] is hypothesised to derive from the communicative function of language" [emphasis added]. This perspective sets Flemming's work, including his MAINTAIN CONTRAST OT constraints, apart from almost all modern phonological research in the generative tradition, which generally eschews overtly functionalist reasoning, and sometimes rejects it explicitly:

> Since language is not, in its essence, a means for transmitting [cognitive] information—though no one denies that we constantly use language for this very purpose—then it is hardly surprising to find in languages much ambiguity and redundancy, as well as other prop-

> erties that are obviously undesirable in a good communication code. [Halle (1975, p.528)]

Lexical and structural ambiguity, as well as the existence of neutralization rules (even if the neutralization is incomplete!), all illustrate Halle's point. In SFP, we carefully distinguish the nature of language from the use to which we sometimes put it.

It is not possible to evaluate here every appeal to the role of contrast in grammar, however, it is simple to appreciate in light of Halle's statements the connection between (ontological) contrast and another idea that is anathema to SFP, *functionalism*. The intended sense of functionalism here is the idea that insight into grammar can be gleaned from consideration of the fact that language is used for communication. It is apparent that the notion of contrast, as used in phonology, relates to the capacity of a phonological difference to communicate a difference in meaning. Many phonological discussions of contrast since Jakobson have borne the taint of functionalism and a failure to distinguish epistemology from ontology.

In this brief overview, I cannot survey the relatively sophisticated attempts to build contrast into phonology, for example, attempts to show that phonological processes can be sensitive to the distinction between contrastive and non-contrastive features in a language. However, given its relationship to functionalism, and given the failure to clearly distinguish epistemological from ontological questions, I suggest that the null hypothesis should be that contrast is *not* relevant to phonology.

6 Banishing Phonotactics

A staple of phonological discussion since Chomsky and Halle (1965) is the differential evaluation of forms like [blɪk] and [bnɪk] by English speakers. Neither form corresponds to a word (or morpheme) in a speaker's lexicon, yet the former is judged to be a possible word and the latter not a possible word. The robustness of such judgments is taken to reflect speakers' knowledge of the phonotactics of their language, generalizations about what sound combinations are existent/wellformed. Since phonotactic knowledge is knowledge about sound patterns, it is often assumed to be part of phonology. Also, some aspects of phonotactics are clearly due to the effects of phonological rules—a language with a rule that always assimilates /n/ to a following /p/ or /k/ will not have sequences like [np] or [nk] in output forms (barring opaque rule interaction).

Optimality Theory and other approaches to phonology assume that phonotactic patterns must be accounted for by the phonological grammar. The OT solution is interestingly elegant in that the surface inventory of segments and the relations among segments, the phonotactics, emerge from the same constraint ranking that accounts for alternations, without the need for morpheme structure constraints on the form of lexical entries or other statements.

I suggest that the arguments for assuming that phonotactics is part of grammar are weak, and that the null hypothesis should be that phonotactic judg-

ments reflect metalinguistic awareness, similar to awareness of rhyme or metrics for poetic composition and parsing. Hearkening back to the early arguments for rationalist generative grammar (*e.g.*, Katz and Bever (1976)), I suggest that phonotactic judgments are like acceptability judgments in reflecting many factors, and thus not necessarily a good indication of grammaticality status.

A first argument against putting too much weight on phonotactic judgments is that there is no reason to think that speakers' judgments are always valid. North American speakers, for example, take a lot of convincing to judge the flaps of *rider* and *writer* as identical. They also will assert that no words begin with a [pt] cluster, even when they clearly pronounce *potato* with such a cluster. It is actually not even clear to me what the "correct" answer is—if there *is* a vowel in the output representation of *potato* that is so reduced in speech as to be unidentifiable in a spectrogram, is there or is there not a phonotactic ban on initial [pt] clusters? Speaker's phonotactic judgments are colored by orthography, morphophonemic alternations, experience with other languages and accents, and so on.

Despite their potential lack of reliability, we can sometimes find surprising accuracy in judgments of phonotactic patterns in the definite *absence* of grammatical knowledge. Suppose a group of monolingual, literate, English speaking non-linguists are presented orally with forms like [pumehana] and [bɛzvzglɛndnɨ] and they are asked which one sounds like Polish and which like Hawaiian. I imagine the subjects would pretty much all agree in their judgments. Informally, we could say that their judgements are 'correct'. We will have thus demonstrated some kind of phonotactic 'knowledge' in the absence of a grammar, since our (hypothetical) subjects speak neither Polish or Hawaiian. If we asked for judgements about Polish words *vs.* Russian words, or Hawaiian *vs.* Samoan words, the subjects might perform worse, and we expect performance to be tied to past exposure to the relevant languages. In this context, we would expect the same (English-speaking) subjects, who obviously have lots and lots of exposure to English data much like their own dialects to have very strong judgments about English phonotactics. And this is what we find. Therefore, there is no reason to assume that strong, and often correct, phonotactic judgments reflect grammatical knowledge. People have such judgments about languages for which they clearly have no grammar. So, SFP, as a theory of grammar, has no problem with failing or refusing to account for phonotactic "knowledge".

A final reason to reject phonotactics as part of phonology is that Frisch et al. (2000) found that phonotactic judgments are gradient, suggesting that such judgments are non-grammatical in nature. Interestingly, the authors of the study drew a very different conclusion, claiming that the results on phonotactic judgments showed that grammar itself, and grammatical wellformedness (not just speaker *judgments* of grammaticality) is gradient. This strikes me as throwing out the categorical baby of discrete symbolic computation *instead of* throwing out the phonotactic bathwater that I am happy to see go.

SFP ignores phonotactic judgments—they are not phenomena that the *grammar* must describe. Even when accurately reflecting facts about occurring sound sequences, such judgments may be drawing on a wide array of factors, includ-

ing frequency of sequences in the lexicon, token frequency in spoken language surface forms, and generalizations that reflect the history of the language. For example, if a sound change in the history of L assimilated /n/ to /m/ before /p/, then the absence of /np/ sequences in L may be a static fact about the lexicon of L, about which speakers may have judgments. In the absence of alternations, there is no reason to expect the computational system to account for such a static generalization.

Work on phonotactics like (Daland et al., 2011) asserts that phonotactic principles like the Sonority Sequencing Principle are "known to be synchronically active in speakers' grammars", based on what they call 'sonority projection effects' concerning how speakers judge various non-occurring consonant clusters. I won't go into details here, but the important quality of these projection effects is that "the offending clusters are systematically and *equally* absent from speakers input, and yet speakers appear to *differentiate* some clusters as less well-formed than others." I suggest that the phonotactic literature is making the same mistakes that Katz and Bever (1976) pointed out in the generative semantics literature of the seventies:

> generative semantics has distorted grammar by including within its goals those of a complete theory of acceptability [p.58].
> ... The issue between the rationalist and the empiricist conception of the domain of grammar is an empirical one. Our estimate of the evidence at present is that it heavily confirms the rationalist strict separation of grammatical phenomena in the traditional sense from extragrammatical phenomena. ... [W]e have shown that the rationalist program can not only deal with the phenomena brought up but does so in a more satisfactory way. Moreover, as we have already indicated, the generative semanticists' criterion leads to a theory that rapidly becomes a study and compilation of everything. But a compilation of everything is a science of nothing: the advantage of the rationalist program, then, is that by distinguishing different contributions to linguistic behavior, explanation in terms of appropriate principles becomes possible in each case.

SFP, in excluding phonotactics from consideration, in *rejecting* the idea that "any phenomenon systematically related to cooccurrence is *ipso facto* something to be explained in the grammar" (Katz and Bever, 1976, p.58), is in the rationalist tradition of generative grammar. The old arguments for this approach seem to hold.

7 Features and rules

As pointed out above, the main tenet of SFP is that phonetic substance is not relevant to the formal properties of phonological computation—issues like how rules compose and the formal properties of rules. Strictly speaking, then, a substance free model has no automatic implications for the status of substantive

universals. In practice, however, we find a strange situation. On the one hand, SFP is strongly nativist, with full acceptance of a universal, innate feature set. On the other hand, there are other phonologists who adopt the substance-free *label*, but who interpret it as a rejection of universal features with consistent phonetic correlates and acceptance of arbitrary rules. To further muddy the waters, there is work that purports to argue against SFP, but in fact argues against views about features that have nothing to do with SFP. Rather than sort out all these matters, I attempt here to lay out the basis for the views favored in my own model that uses the term 'substance free' phonology, namely SFP. A good overview of various uses of the phrase "substance free phonology" can be found in Blaho (2008), many of which adopt a view of features at odds with that of SFP.

In brief, I argue in this section the following three points:

- phonology is epistemologically prior to phonetics
- features can't be posited on the basis of rules
- features must be innate

This whole section recaps a long tradition in generative grammar of defending the rationalist perspective over an empiricist perspective that resurfaces repeatedly. In addition to influencing much of the the early work on rationalism from a generative perspective, Chomsky (1965) and Chomsky (1966) trace similar ideas much farther back. As far as I can tell, the logical arguments presented by Chomsky and the others have never been seriously challenged.

7.1 The priority of phonology

Most of the discussion on innateness and rationalism *vs.* empiricism is concerned with syntax and semantics—it is really hard to think that features like PLURAL or ACCUSATIVE are actually in the signal. Phonologists, however, typically can't stop themselves from lapsing into thinking that they are working with sounds. In this context, let's take a lesson from an *rationalist phonetician*, a hero of the SFP school, Robert Hammarberg. Hammarberg (1976) leads us to see that for a strict empiricist, the somewhat rounded-lipped *k* of *coop* and the somewhat spread-lipped *k* of *keep* are very different.[6] Given their distinctness, Hammarberg makes the point, obvious yet profound, that we linguists have no reason to compare these two segments unless we have a paradigm that provides us with the category *k*. Our phonological theory is logically prior to our phonetic description of these two segments as "kinds of *k*". So, our science is rationalist. As Hammarberg also points out, the same reasoning applies to the learner—only because of a pre-existing built-in system of categories used to

[6] Of course, any two tokens of, say, the rounded-lipped *k* are also quite distinct from each other by any number of physical, observable metrics, but let's ignore that and assume we recognize two entities, "rounded *k*" and "spread *k*".

parse, can the learner treat the two 'sounds' as variants of a category: "phonology is logically and epistemologically prior to phonetics". Phonology provides equivalence classes for phonetic discussion.

Hammarberg's discussion is grounded in the philosophy of early generative grammar and general philosophy of science:

> Chomskian linguistics is explicitly anti-empiricist, and all indications are that current philosophy of science is moving toward a rejection of the empiricist programme ((Fodor, 1968, pp. xiv *ff*)). A key feature of the new programme is exactly a reevaluation of the concept of observation. Observations are now held to be judgments, and these judgments are made in terms of the criteria provided by the paradigm. Thus the taxonomy of a discipline is to be regarded as imposed from above, rather than emerging from below, i.e., rather than emerging in the form of brute facts before the unprejudiced eyes or ears of the researcher. The relevance of this to the study of phonetics and phonology should be obvious: the concept of the segment, which is indispensable to phonetics and phonology, is a creature of the paradigm, not of the raw data. [Hammarberg (1976): 354.]

Hammarberg (1981) revisits the "Kantian claim that objects conform to our modes of cognition" again drawing on Chomsky, as well as on modern physics:

(4) The 'furniture of the world' does not come prepackaged in the form of individuals with properties, apart from human intervention: [e]ither the analysis provided by the cognitive system that we might call 'common sense understanding' or the more self-conscious idealizations of the scientist seeking to comprehend some aspect of physical or mental reality [Chomsky (1980, 218-9)]. (c.f. also [Chomsky (1975)].)

(5) "The doctrine that the world is made up of objects whose existence is independent of human consciousness turns out to be in conflict with facts established by experiment" [d'Espagnat (1979)]

The point of all this is that phonological categories can't be learned from phonetics, since there can't be any phonetics without a pre-existing phonology. It is the intervention of our cognitive system, our 'cognoscitive powers' (a term Chomsky (2000a) adopts from seventeenth century philosophy), that packages sounds, say, into syllables, segments, sentences, and so on. These categories belong to the science of phonology as well as the object of the science of phonology (the human phonological faculty). As Hammarberg says, "[I]t should be perfectly obvious by now that segments do not exist outside the human mind. [354]" But they are not fictions: "there would be little value in such an approach. Science aims for a theory of the real, and to base one's descriptions and generalizations on a fictional taxonomy could only lead to one's theories being fictional as well."

7.2 Features are innate

SFP adopts fully Hammarberg's realist, rationalist views, and this ties in with general discussions of innateness, beyond phonology. I won't revisit here arguments for the *discrete, binary* phonological categories understood in terms of discrete, binary feature values—let's assume that phonological representations consist of data structures built from valued features, like, say, +NASAL and the like. Hammarberg's arguments reflect a vast literature on the topic, all leading to the conclusion expressed by Jackendoff (1990, 40): "In any computational theory, 'learning can consist only of creating novel combinations of primitives already innately available. This is one of the fundamental arguments of Fodor (1975), and one that I accept unconditionally".

As Fodor puts it, "Trivially, one cannot use the predicates that one is learning in order to learn the predicates that one is using. ... It follows immediately that not all the languages one knows are languages one has learned, and that at least one of the languages which one knows without learning is as powerful as any language that one can ever learn." More straightforwardly, whatever the features, the primitives of phonological representation, are, they have to be innate.

Chapter 1 of Chomsky (1965) contains a detailed discussion of the rationalist tradition supporting this nativist perspective, and many other serious scholars, including Fodor and Jackendoff cited above, have presented it as a logical necessity. Chomsky (1980) explains that without such innate abilities / constraints / categories, we would not be able to do much of anything:

> Were it not for this endowment, individuals would grow into mental amoeboids, unlike one another, each merely reflecting the limited and impoverished environment in which he or she develops, lacking entirely the finely articulated and refined cognitive organs that make possible the rich and creative mental life that is characteristic of all individuals not seriously impaired by individual or social pathology—though once again we must bear in mind that the very same intrinsic factors that permit these achievements also impose severe limits on the states that can be attained; to put it differently, that there is an inseparable connection between the scope and limits of human knowledge.[45-46]

In other words, without innate features, we would not be able to parse input at all, and of course, we would have no way of explaining how people exposed to different input can sometimes arrive at identical knowledge states.

In this context, it is distressing to see that the arguments and conclusions of the rationalist bases of generative grammar have been somewhat cavalierly ignored in the phonological literature in the latest revival of hyper-empricism. A wide array of scholars have asserted that there is no need for innate features, because features can be discovered or constructed on the basis of induction over the input to the learner. For example, Archangeli and Pulleyblank (2015) tell us to "See Mielke, 2004 [PhD thesis published as Mielke (2008)–cr] on why

features cannot be innately defined, but must be learned." However, Mielke's thesis and book do not mention the arguments for the logical necessity of innate representational primitives given by Fodor, Jackendoff, Hammarberg or anyone else. None of these are mentioned in the book at all. Mielke (p.27) asserts that "Chomsky and Halle's assumption that distinctive features are innate is treated in subsequent literature as if it were a conclusion", but Mielke is ignoring the centuries of discussion on the topic that is more general than phonology—the acceptance of a universal innate feature set is a specific conclusion based on a general argument made by linguists, philosophers and psychologists. Where Mielke does look beyond phonology, he restricts himself to syntax, and concludes that "Most of the evidence for UG is not related to phonology, and phonology has more of a guilt-by association status with respect to innateness" (p.34). This is hardly a sufficient refutation.

Hall (2014) characterizes the SFP view well: Hale and Reiss "assume that features are innate and universal, and have substantive phonetic content", but goes on to state that he and Elan Dresher (2015) have both offered rebuttals of the innateness view. Dresher's claim is unambiguous: "There is a growing consensus that phonological features are not innate, but rather emerge in the course of acquisition." Dresher's position is not completely anti-nativist. He proposes that "a fixed innate list of phonological features has been problematic on empirical grounds, and is not conceptually necessary because there is an innate mental mechanism for creating distinctive features in the course of language acquisition" (2016). This mechanism is said to rely on "an auditory system that allows us to make certain sound discriminations", suggesting that features can be derived from the basic categories of auditory perception.

Such a theory, one that can derive specific features from a general concept of 'feature' and the powers of auditory perception, is in principle a better theory than one that posits the richer innate structure of models like *SPE* and SFP. However, the model seems to fail in light of the problem of the lack of invariance (Appelbaum, 1996), as well as the vast acoustic differences among speakers when saying "the same thing". It also appears to be at odds with the results in infant speech perception and the need for parsing input into representations for learning to even begin, as appreciated by Fodor and others. It seems unavoidable to posit higher level equivalence classes for speech perception to even take off in acquisition, in other words, we need innate features.

The SFP position on the necessity of innate features is not something we discovered or adopted lightly. It follows logically from arguments extending at least to Kant, as the discussion of Hammarberg suggested, above. To my knowledge, the arguments have never been refuted.

7.3 Can rules tell us what the features are?

Some authors who reject innateness use the term 'substance free' to refer to the idea that features are not substantive universals, but are rather induced from patterns in the learner's input. Hall (2014, p.2-3) gives an excellent characterization of this view:

> In this view [also called 'substance free'—cr], features are not universal or innate, but rather are induced by the learner. Featural representations are assigned on the basis of phonological behaviour, not acoustic or articulatory substance (although phonological properties often do happen to correlate with phonetic ones). This allows for rules that are maximally formally elegant, even when they are phonetically unnatural. As Blaho (2008: 2223) puts it, "Features are indicators of the way members of an inventory behave, but they don't necessarily have any consistent phonetic characteristics even within the same system." Likewise, in Emergent Feature Theory, features do not necessarily have any content beyond identifying "the segments that do X" (Mielke 2008: 99).

The problem with this perspective is that there is no way to induce patterns from phonological behavior without an innate feature system with which to parse the input. In Fodor's terms, you need an innate language to learn another language. Isac and Reiss (2013) provide an accessible demonstration of this logic using toy grammars based on playing cards (adapted from Hale and Reiss (2008) and Hale and Reiss (2003b)).

Another problem with these approaches is that they assume that it is obvious what a rule is. Suppose the input provides evidence for alternations that delete word-final /a/ and word final /t/. Are we to expect the learner to posit a feature that makes {a,t} a natural class of segments? Of course not.

The SFP approach, laid out in Bale et al. (2014) and Bale and Reiss (Forthcoming) is basically that of *SPE*: the target and environment segments of a rule are sets of segments, each member of which is a superset of some set of features—the segments form a natural class. If we intersect the features of /a/ and /t/, we get a natural class that will contain lots of other segments that do *not* delete word-finally. In SFP, this result tells us that there must be two separate deletion rules, one for /t/ and one for /a/. You can't induce the features from the rules without a theory of rules, and a theory of rules relies on natural classes. You can't figure out which segments "do X" without the means to identify segments and identify X.

Let's now turn to a familiar example where the observable evidence suggests that the triggers of a rule are *not* a natural class, but where a deep analysis provides a solution in which the rule is in fact based on natural classes. The regular English plural marker is underlying /-z/. The single segment of this morpheme surfaces as [s] when occurring after a root ending in one of these segments: {p,t,k,θ,f}.[7] Now, the participation of the segments {p,t,k,θ,f} as triggers suggest to the observer that these segments should form a natural class. However, they do not according to all feature systems in use—any class that contains these segments should contain [s] and [ʃ], too.

A simple solution to this problem is to say that the rule inserting a vowel between two coronal stridents (as in *bushes*) comes before and bleeds the devoicing

[7] Let's say that this is an observable fact, even though it assumes a lot of filtering of the data—"plurality" and the property of being "regular" are not in the signal, for example.

rule. In other words, the devoicing rule *intensionally* can be formulated to apply to sounds specified -VOICE, but the extensionally defined set of observable segments that trigger the rule is {p,t,k,θ,f}, which is not a natural class in English. It is impossible to decide if rules are natural or unnatural with respect to an innate feature set if we depend only on "observable distributional regularities". There is a vast literature demonstrating that the "relation between a phonemic system and the phonetic record ... is remote and complex" (Chomsky, 1964, 38). Neither segments nor rules are observable. They are instead the outcome of an analysis (by linguist or learner) in which the "essential properties underlie the surface form" (Katz and Bever, 1976, p.12).

The extent to which this basic logic of rules and natural classes is misunderstood is quite surprising. Even authors interested in the formal properties of grammars get the relationship backwards. For example, Kornai (2008), assuming a language with a segment inventory that includes at least [p,t.b,d], says that "Phonologists would be truly astonished to find a language where some rule or regularity affects *p*, *t*, and *d* but no other segment"—presumably he has *b* in mind. The SFP view is that there cannot be such a rule because the natural class defined by features would force us to posit *two* rules, not one. Without innate substantive features, we can't determine what the rules are, and we can't determine what the segments are. Features are the primitive units that allow the learner to parse the signal into segments and determine if various alternations can be collapsed into a single rule.

In the absence of any engagement with the logical arguments of Fodor and Chomsky and others, assertion that features can be induced from the rules cannot be taken seriously. SFP continues to assume, therefore, an innate set of phonological primitives.

This is not to say that the exact set of features proposed in the literature, or anything close to the number of features proposed is close to accurate (see Hale et al. (2007) for arguments that standard proposals are way too low). But we must accept that there is an innate feature set.

I wish to reiterate that the SFP phonology perspective is not new. The following quote illustrates the extent to which the idea that generalizations of grammar are observable on the surface was rejected in syntax, at least, in the early days of generative grammar:

> From the general intellectual viewpoint, the most significant aspect of the transformationalist revolution is that it is a decisive defeat of empiricism in an influential social science. The natural position for an empiricist to adopt on the question of the nature of grammars is the structuralist theory of taxonomic grammar, since on this theory every property essential to a language is characterizable on the basis of observable features of the surface form of its sentences. Hence, everything that must be acquired in gaining mastery of a language is "out in the open"; moreover, it can be learned on the basis of procedures for segmenting and classifying speech that presuppose only inductive generalizations from observable distributional regu-

> larities. On the structuralist theory of taxonomic grammar, the environmental input to language acquisition is rich enough, relative to the presumed richness of the grammatical structure of the language, for this acquisition process to take place without the help of innate . . . principles about the universal structure of language. Rationalists, on the other hand, find the the taxonomic theory uncongenial because, for them, the essential properties of language underlie the surface form of sentences and are thus unobservable in the sense in which atoms are unobservable.[(Katz and Bever, 1976, p.12)]

The real English devoicing rule, one which is featurally natural (it has to be or it would not be a rule for SFP) is not observable. Instead, we observe a phonetically unnatural set of triggers. The solution comes from a sophisticated analysis in terms of composed rules that cannot be read from the signal.

Before leaving the issue of the innateness of features, it is worthwhile pointing out that the innateness model appears to be the only one compatible with the well-established experimental results of infant speech perception found by Janet Werker and her collaborators (see Werker (1995) for an overview). It beggars belief that the sensitivity to every possible phonemic distinction which Werker finds in infants is unrelated to an innate capacity for phonological representation.

7.4 Rejecting a particular model of features

SFP accepts the existence of innate substantive universals, but it does not have to accept all versions of innate representational schema for features. One version that is particularly odious to the SFP perspective is Feature Geometric models that mimic (somewhat) the structure of the vocal tract with the effect of sneaking substance back into the computational system. McCarthy (1988:84), in an exposition of Feature Geometry, states that "The goal of phonology is the construction of a theory in which cross-linguistically common and well-established processes emerge from very simple combinations of the descriptive parameters of the model". For example, "Assimilation is a common process because it is accomplished by an elementary operation of the theory—addition of an association line" (86). With hindsight, it is apparent that this argument is invalid. The human phonological faculty is only one factor determining the set of attested phonological systems. There is no obvious way in which its properties could determine the common-ness of attested patterns. Such reasoning, extended to syntax, would lead to a theory that makes *do*-support simple to model, because English-type grammars are quite widespread. A more promising view of the goal of phonology and linguistic theory in general is "to abstract from the welter of descriptive complexity certain general principles governing computation that would allow the rules of a particular language to be given in very simple forms" (Chomsky, 2000b, p.122). In Bale and Reiss (Forthcoming); Reiss and Shen (ms.); Bale et al. (2016) we explore the limits of basic set theoretic notions for accounting for the behavior of phonological entities. The argument does not rely on what is common versus rare, but rather follows standard scientific method

of expanding the model's power only when necessary.

There are many, many issues that remain unsolved with respect to the nature of featural representation and transduction between features and, say, sound. However, these cannot be addressed here. Some of them, like the problem of the lack of invariance are profound, longstanding issues in the study of speech perception (see Appelbaum (1996) for discussion) that should probably be considered as separate from the computational system of phonology proper. This problem is part of the issue of the abstract nature of even the substantive universals accepted by SFP. In SFP the old question of whether features are primarily acoustic or primarily articulatory does not make sense—they are mental representations, primitives of mental data structures, in the sense of Gallistel and King (2009), each with a complex transduction relation to input and output systems. Given that even non-linguistic auditory perception is subject to illusions and thus sometimes non-veridical (Bregman, 1990; Reiss, 2007), it can't be the case that phonological features would be straightforwardly related to acoustics or articulation.

7.5 How rich is phonological UG?

SFP adopts the view of Gallistel and King (2009) that mental representations in general must consist of a hierarchy of data structures that are ultimately composed of some set of atomic symbols. The effect of having such a taxonomy of symbols is that a relatively small number of lower level symbols can be combined into a very large number of higher level symbols. This idea is consonant with Chomsky's 2007 point that "the less attributed to genetic information (in our case, the topic of UG) for determining the development of an organism, the more feasible the study of its evolution" —it is simpler to study the evolution of a simple system than a complex one. Gallistel and King's discussion of how to use combinatorics to deal with combinatoric explosion is illustrated for phonology in Reiss (2012) where I walk through the simple math that shows that a UG with just four binary features and the option of having underspecified segments allows for 2.4 septillion languages.

So, the combinatorics makes the point that we don't need a lot of features in UG to get a lot of descriptive power from UG, but obviously we need more than four phonological features. It is common to find in the phonological literature that twenty-five or so features is a reasonable number (recall that Chomsky suggested "perhaps on the order of fifteen or twenty" in *Aspects*). The SFP perspective is that this is an arbitrary number with no current justification—we are nowhere close to knowing the exact number of features, and it is silly to think that any of the features discussed in the literature are, without a doubt, correctly identified and individuated. Hale et al. (2007) argue that it is preferable to increase somewhat the number of features in our models of UG, rather than posit language specific phonetic implementation rules. The combinatoric explosion that differentiates models of UG with twenty-five or fifty or one hundred features is unfathomable, but irrelevant. If we sensibly think of this all in terms of an intensionally defined UG, the differences among

these numbers of features grows merely linearly, and they are all of the same order of magnitude.

8 Some SFP

So, what's left?

Much discussion in the literature of the formal properties of phonological computation is either compatible with SFP, or, if it proves superior to current SFP proposals, should be incorporated into SFP in their stead. Every phonological theory contains a substance free component, and many of the ideas from this work have informed SFP. Because of its rejection of so much recent work, SFP can appear reactionary, but the intention is maintenance of the good arguments concerning the rationalist basis of generative linguistics with a critical evaluation of longstanding notions like markedness and contrast in phonology, all accompanied by technical and analytic contributions.

The impressive body of work by Jeff Heinz and his colleagues and students on characterizing phonology from the perspective of formal language theory is probably the most influential recent work in formal phonology that is substance free in the SFP sense. I refer the reader directly to this work, including Heinz and Idsardi (2013); Heinz (2010); Chandlee et al. (2014); Chandlee and Heinz (2012); Chandlee and Koirala (2014). While this work is refreshing in its explicitness and in the coherence of this budding research community, its ultimate worth for a linguist in the generative tradition will depend on the significance of formal language theoretic results to generative concerns and the extent to which the work can generate new insights into phonological phenomena, and not just formalize potentially problematic aspects of traditional phonological work, such as phonotactics.

I exclude from consideration as relevant to SFP the even larger literature on the formal properties of OT, because, despite the sophistication of these discussions (*e.g.* work by Jason Eisner, Jason Riggle, Giorgio Magri, Alan Prince, Bruce Tesar and many others) in all instantiations, OT ends up being substance abusing by virtue of the content of Con and the notion of well-formedness embedded in the very idea of optimality—there can be no optimal form unless the other forms are less optimal, less wellformed, as discussed above. Recall that "Optimality Theory rests directly on a theory of linguistic markedness." If markednesslessness is correct, this foundation is rotten. It remains to be seen if all that impressive mathematical work on OT formalism can be salvaged.[8]

Much work in syllabification and metrical phonology either is untainted by substance abuse, or else can be fairly easily detoxified and recast in substance free terms. An excellent body of work compatible with SFP developed from

[8]There remain other apparent problems with OT. The original arguments against intermediate representation appear empty (Karttunen, 1998), and although current versions of OT have reintroduced derivations, with intermediate representations (see work in Harmonic Serialism and Stratal OT, for example), the claim that OT is a two-level model, and the idea that this is somehow desirable, persist.

Raimy's 2000a research on precedence relations and linearization in phonology. It is worth pointing out that Raimy (2000b) was able to demonstrate, *contra* the claims of McCarthy and Prince (1995), that a derivational model can in fact handle supposed cases of under and over-application which were paraded as demonstrating the failure of derivational models and the need for parallel constraint satisfaction models like OT. One SFP approach to reduplication Reiss and Simpson (2017), inspired somewhat by Raimy's work, does without the notions of markedness, BASE and COPY, and CORRESPONDENCE, notions which play an important role in much functionalist and surface-oriented OT work in morphology and phonology (see Hale et al., 1998).[9]

The SEARCH and COPY models developed somewhat in parallel (and with useful cross-fertilization) by Andrew Nevins (2010), on the one hand, and Shen (2016); Mailhot and Reiss (2007); Samuels (2011) on the other, are quite similar. Nevins' approach, in its reliance on contrast and markedness is not fully compatible with SFP. However, it would probably be useful to extract the best formal aspects from the two traditions and combine them into a better substance free model of locality in phonological rules.

In the following paragraphs I will describe a few more aspects of my own work that exemplify my vision of SFP. Obviously, my discussions of these topics are all subject to criticism on empirical and theoretical grounds, but I hope that they are at least consistently substance free.

Quantifiers in phonology In Reiss (2003) I argued that, to handle so-called anti-gemination and anti-anti-gemination phenomena, phonological rules must be able to compute identity and non-identity between segments. This work is an outgrowth of Odden (1988), a phonologist whose interest in formal issues has generated a lot of SFP-compatible work. (See Bakovic (2006) for an interesting, basically substance-free OT critique of my claims.) I argued that such computation is best expressed *via* the power of first order quantificational logic. These conditions, which I expressed with the existential and universal quantifiers, are part of language specific rules. In these quantificational computations over sets of features the particular nature of the features play no role, so the rules are substance free. It should be obvious that many other models, including OT, may also make use of similar quantificational logic, for example to determine whether a constraint against geminates is violated.

Phonological Acquisition In Hale and Reiss (2003a, 2008, 1998) we attempt to model phonological acquisition without recourse to markedness, with an appreciation of the contribution of performance issues to the nature of children's output, and in a manner consistent with findings from infant speech

[9]Note that the Output-Output Correspondence and Uniform Exponence Constraints of much OT work parallel the transderivational constraints of the generative semantics literature, strengthening the comparisons between the approaches made above: "Thus, there must be rules that apply not to individual derivations, but to classes of derivations. In short, transderivational constraints are required, since there are cases where the well-formedness of one derivation depends on certain properties of other, related derivations" (Lakoff, 1973).

perception studies. This work accepts the existence of innate substantive features, but the modeling of the acquisition process proceeds in a substance-free fashion, based on simple set theoretic operations—there is no appeal to markedness and no attempt to account for children's superficial speech output, in light of all the evidence from covert contrasts (Gibbon, 1990; Gibbon and Scobbie, 1997) and comprehension studies that demonstrate the sophistication and detail of their representations. We obviously deny the validity of "the emergence of the unmarked" (TETU) phenomena widely discussed in the OT literature on phonological acquisition and computation, such as Struijke (2014).

Operations in phonology In Bale et al. (2014), we deconstruct the arrow of traditional phonological rules and argue that the arrow corresponds to at least two different operations: set subtraction and unification. We make use of this distinction to revive and formalize an old idea from Poser (1993, 2004): "feature changing rules" should be understood as deletion (which we model via set subtraction) followed by insertion (which we model via unification).

Types and underspecification in phonology Building on the work in Bale et al. (2014), Bale et al. (2016) explore the ramifications of treating segments as sets of valued features. For example, we demonstrate that it is possible to extensionally target only the fully underspecified segment that contains no features, { }, by writing a rule that intensionally targets *all* segments.

This small sample merely suggests the range of issues that are relevant to the SFP perspective. A good example of a substance free topic that has engendered healthy controversy and interesting observations about what various formal systems are capable of is the question of whether there can be *polarity rules*, rules that turn αF into $-\alpha$F, for some feature. Work in OT and various rule based frameworks has addressed this quintessentially formal problem (see Moreton (1999); Bale et al. (2014); Fitzpatrick et al. (2004) for a sample).

9 Conclusions

Three core questions for SFP are the following:

- What kind of data structures are phonological representations?
- What is a possible phonological rule?
- What kind of complex function is a phonology?

Future research will explore these questions, as well as guide exploration of issues in phonological acquisition and the interface of phonology with other modules of grammar.

In this sketch, I have tried to clarify several issues that surround the use of the term 'substance' in phonology, especially in the phrase 'substance free

phonology'. The focus has been on laying out my own idiosyncratic model and laying claim to the phrase as a proper name, Substance Free Phonology. Importantly, the freedom from substance in SFP refers only to the nature of the computational system. SFP accepts, indeed embraces, the innate substantive entities of, say *SPE*, that is the idea of innate universal features. The exact number of features needed for UG and the exact phonetic correlates of the features remain questions for future research.

Although the issues are poorly understood, we entertain the possibility that the *specific* substance of the features is universal across spoken languages because of the universality of the interface of the substantive primitives with the human transduction systems. If the same innate feature set interfaced with a different transduction system, the phonetic correlates would be different. Perhaps this is what happens in signed languages—the same innate feature set interfaces with the visual and manual motor systems. This is very speculative, but has no bearing on the substance free nature of phonological computation, as understood in SFP.

Finally, despite rejecting a tremendous array of topics that are typically considered to lie within the purview of phonological theory, topics like contrast, typology, child speech and markedness, I suggested that there are rich opportunities to understand the nature of phonological computation and representation from a purely formal perspective. There is a lot of work to be done on the nature of representations as data structures; on the kinds of operations that apply to these structures, such as unification and set deletion, as well as quantificational operations and SEARCH and COPY procedures.

Finally, I hope to have demonstrated that SFP takes seriously the arguments for rationalism and other philosophical foundations of generative grammar, including the competence-performance distinction, that have been ignored or rejected (sometimes with uncanny parallels to mistakes of the past) without argument in much recent work. Whatever its failings, I hope the SFP approach rests on the right foundations.

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