# Lexical Phonology and the Lexicon

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It would be a time-consuming but straightforward task to compile a complete list of exceptions, at least for the rules of word-level phonology.  Given the purposes of this study such an effort would be beside the point unless it were to lead to the formulation of new and deeper rules that explained the exceptions or to a different theory that accounted both for the regularities that our rules express and for some of their defects and limitations.

<div align="right">Chomsky and Halle (1968:<i>ix</i>)</div>

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**0.     A different theory**

The question I wish to address in this paper is quite simple:  what makes lexical phonology (i.e. morphophonology or word phonology) lexical?  My answer is equally simple: knowledge of lexical phonology is essentially identical to knowledge of the lexicon itself.  For example, when linguists say English speakers know the generalization of velar softening, as expressed in the pair *critic-criticism*, only one aspect of their knowledge is certain, namely surface phonological forms like [krɪtɪk] and [krɪtɪsɪzm].  Their knowledge (if indeed they have any) of velar softening itself would be entirely dependent on two properties of these words:  (a) the fact that *critic* and *criticism* form a "pair," i.e. are linked in some non-phonological way (here, morphologically and semantically); (b) the observation that the *k~s* alternation seen in the pair *critic-criticism* also shows up in other pairs, such as *critical-criticize*, *opaque-opacity*, and so forth.  The term I use for such generalization driven by specific lexical items is <u>analogy</u>.

My first goal is to support this view of lexical phonology solely with evidence that the generative tradition considers "internal," namely lists of words found in dictionaries or transcriptions of categorical speech patterns.  Specifically, I will show that an analogical approach to lexical phonology can help deal with many hitherto neglected aspects of many familiar patterns in English.  I will not need to rely on evidence from experiments, speech errors, language acquisition or language change to make my major points.

My second goal is to express the notion of analogy in a formal model that builds as much as possible on work in the generative tradition.  To this end, recent developments in <u>Optimality Theory</u> (OT; Prince and Smolensky 1993, McCarthy and Prince 1993a,b, 1995a,b) will be quite useful.  In fact, somewhat to my own amazement (and perhaps to the dismay of readers for whom the term "analogy" is already throwing up warning flags), the necessary formal devices have already been independently motivated in the OT literature.  All I will have to do is tweak them a little and spell out the consequences.

The organization of this paper is as follows.  In section 1, I motivate the assumptions concerning the lexicon and morphology that make my view of lexical phonology possible, ending with an informal sketch of the role I envision for analogy.

In section 2 I get down to cases, demonstrating the analogical nature of many familiar patterns in English lexical phonology.  That is, from highly peculiar patterns like the vowel alternation in *drive-drove* right on through pervasive alternations affecting vowels and consonants, I show that all of these lexical patterns are (a) real phonology, not random accidents that can safely be ignored by phonologists; (b) inexpressible using general rules or universal constraints; (c) understandable given an analogical view of lexical phonology.  The last claim is supported by signs that the patterns emerge from interactions between the lexical items themselves, rather than being imposed by rules or constraints that exist separately from the specific list of items in the lexicon.

In section 3, I discuss formalism.  I begin by showing how OT seems to hold much promise for the proper analysis of lexical phonology, a promise that unfortunately has not yet been fulfilled; this includes previous recent attempts to formalize analogy within OT that have

come up short (e.g. Kenstowicz 1995, 1996; Steriade 1996). I then describe my proposal, which relies on the following four established OT devices: (a) extrinsic (i.e. language-specific) constraint ranking; (b) parochial constraints (i.e. universal constraints that can be parameterized to apply only to specific lexical items; see e.g. McCarthy and Prince 1993a; Hammond 1995, 1997; Golston 1996; Benua 1995, 1997a,b; and references therein); (c) output-output correspondence (e.g. McCarthy and Prince 1995b; Kenstowicz 1995, 1996; Benua 1995, 1997a,b; and references therein); and (d) constraint conjunction (e.g. Crowhurst and Hewitt 1997; Smolensky 1995a; and references therein). In this section I also exemplify the formalism with applications to some of the patterns given in section 2.

In section 4 I address an important element that my formalism specifically neglects: markedness. I provide several independent arguments for the conclusion that phonetically motivated markedness constraints cannot play a direct role in lexical phonology, and instead unmarkedness in lexical phonology must be explained through Lexicon Optimization (Prince and Smolensky 1993) during language acquisition.

Finally, section 5 concludes with musings on where we've come to and where we should go next.

## 1. Background

My view of lexical phonological knowledge, a rejection of what Bybee (1994) (after Langacker 1987) calls the "rule-list fallacy," is quite popular among phonologists with an interest in linking linguistic theory with the other cognitive sciences (e.g. Bybee 1994; Stampe 1973/1979; Donegan and Stampe 1979; Ohala 1986a, 1990; Skousen 1989). That generative phonology has not embraced this view is perhaps understandable, given the various objections I will have to meet in the course of this paper, but it is also something of a missed opportunity. The "external" evidence for viewing lexical phonology as analogy is so overwhelming that one would hope that at least some scholars would be working to shrink the gap between such evidence and generative formalisms, without sacrificing the descriptive success that the generative approach has already achieved.

Here I first review some of this "external" evidence, showing how it has already had some influence on generative phonology. Then I turn to a description of some crucial assumptions concerning morphology. Finally I sketch out what I mean by analogy.

## 1.1 Knowledge of lexical phonology

As noted above, there is an enormous amount of "external" evidence for viewing knowledge of lexical phonology as completely dependent on knowledge of the surface forms of lexical items. First, unlike postlexical phonology (i.e. pure phonology, phrase phonology, phonetics), lexical generalizations are not automatically extended to novel (nonlexical) forms, whether they are found in normal language production (e.g. Stampe 1973/1979; Donegan and Stampe 1979; Holden 1976; Hooper 1976), created by speech errors (e.g. Mohanan 1982; J. Myers 1993; Shattuck-Hufnagel 1986; Stemberger 1983, 1986), or given to subjects as experimental nonce forms (e.g. Armbruster 1978; Aske 1990; Bybee 1994, 1996; Cena 1978;

Hochberg 1988; Hsieh 1970, 1975, 1976; Jaeger 1984, 1986; McCawley 1986; Myerson 1976, 1978; Ohala 1974; Ohala and Ohala 1986; Steinberg and Krohn 1975; Wang 1985, 1995; Wang and Derwing 1986; Wright 1975; Zimmer 1969).  In other words, lexical patterns seem to be "stuck" to actual lexical items, and do not easily generalize to nonlexical forms.

Second, when lexical patterns are extended to nonlexical forms, experimentally or in natural speech, they betray clear signs of deriving their existence from the lexical items they describe rather than having independent status as general rules or constraints.  For example, they are influenced by surface phonological similarity to existing lexical items (e.g. Frisch 1996; Ohala and Ohala 1986), token frequency (Bybee 1996; Fidelholtz 1975; Frisch 1996; Hammond 1997; Kaisse 1985; Myers and Guy 1997) and the degree of the perceived "relatedness" of lexical items showing the purported alternations (e.g. Armbruster 1978; Guy and Boyd 1990; Jaeger 1984, 1986; Marslen-Wilson, Tyler, Waksler, and Older 1994; McCawley 1986).

Finally, although there are occasional claims by phonologists that lexical representations must be as nonredundant as possible in order to save precious memory space (Bromberger and Halle 1989), this is exactly the opposite of reality.  Psychologists have long known that cognitive processes work much more efficiently when as much as possible is memorized, and as little as possible is calculated on-line (see Stemberger 1982/1985 for references and discussion).  For example, auditory memory for word forms even includes phonologically irrelevant information about the identity of the speaker (e.g. Nygaard, Sommers, and Pisoni 1994), suggesting that words reside in memory as overlapping sets of exemplar tokens, rather than abstract types (see Kirchner 1999 for discussion of some implications of this).

Recognition of the lexical nature of lexical phonology has filtered into mainstream generative phonology via the theory of Lexical Phonology (Kiparsky 1982, 1985, Halle and Mohanan 1985, Mohanan 1982/1986).  A particularly revealing quotation in this regard comes from Kiparsky (1982) in a discussion of what it means for lexical rules to "apply":

> This does *not* imply that the speaker or hearer need in any way mentally "derive" the words he says or hears by means of such rules as Velar Softening.  What it *does* mean is that the alternations they govern belong to the regular phonological pattern of English, while for example a hypothetical *k~s* alternation in the reverse context, such as *criti[k]ize~*criti[s]al*, would be irregular.  The claim made is that someone who knows English implicitly knows that pattern, and will under appropriate circumstances recognize the difference between regular and irregular alternations, though he may not be able, even after reflection, to verbalize the rules that underlie it.  [Kiparsky 1982:34-35, emphasis in original]

Of course, the theory of Lexical Phonology never went so far as to claim that knowledge of lexical phonology is completely identical to knowledge of lexical forms.  Instead, in this theory, lexical phonology is represented redundantly, both in the set of word forms, and also in generalizations that have independent formal status (e.g. the velar softening rule).  Moreover, the generalizations are all derivational (e.g. /k/ → /s/ before /i/), interacting in such a way that they generate a long series of intermediate representations.  For example, to explain the alternation between /k/ and /s/ in *critical* and *criticize*, one must also employ the rule vowel shift (which

changes /i:/ to /ay/), and extrinsically order it after velar softening. The result is a view of the lexicon with a charming disregard for internal coherence: in the Lexical Phonology lexicon, all words are (actually, psychologically) listed and also (virtually, descriptively) derived.

Contrary to what is sometimes insinuated, OT is in principle perfectly compatible with the Lexical Phonology philosophy (e.g. see Booij 1997, who exploits level ordering and allomorphy in an OT framework). The hope, then, is to accept the evidence that lexical phonology is literally lexical, and yet express this concept a more coherent way than has hitherto been done in derivational or OT formalisms.

## 1.2    Lexical knowledge and morphology

One assumption obviously important to my proposal is that knowledge of morphology is essentially identical to knowledge of words, just as lexical phonology is. That is, lexical items are not morphemes, but entire words; knowledge of morphological structure is expressed by something like the morpholexical redundancy rules that have been argued for many times (c.f. S. Anderson 1992; Aronoff 1976; Bochner 1993; Jackendoff 1975; Lieber 1980). Thus for example, an English speaker's knowledge that *critic* and *criticism* contain the "same stem" is not expressed by deriving *criticism* from *critic*, in particular through the concatenation of the morphemes *critic* and *ism*, but rather by the linking (in a sense that varies from theory to theory) of the two lexically listed items *critic* and *criticism*. At the very least, the word-based view of morphology explains why derived words are often more than the sum of their parts, including unpredictable properties in semantics (e.g. *transmission* being a car part) and phonology (e.g. irregular inflection). Whether or not morphological generalizations are indeed rules or actually exemplar-driven analogy is not crucial for my discussion, though obviously it would be more parsimonious to say that the lexicon is run by analogy all the way through.

However, there is an important difference between lexical phonology and lexical morphology: by definition, only the latter can create new words. This generative power can be quite intimidating, especially in languages with a lot of inflection (e.g. Turkish; see Hankamer 1989). Since we must grant morphology the power to create new words, it is difficult to categorically revoke it in the case of old words. Thus in any given utterance, it is possible in principle that even an old word has actually been created (whether by rule or analogy) rather than simply looked up in memory (see J. Myers 1993 for further discussion). Of course, it makes a big difference if this old word is something like *permit* (i.e. *per+mit*) or something like *cats* (i.e. *cat+s*). Coopting the traditional terminology, I will call this difference productivity, using this to describe the following gradient property: the likelihood that a given morphological generalization actually applies in real language processing (for similar definitions, see also Aronoff 1976; Bolinger 1948; Baayen and Renouf 1996).

Note that productivity as defined in this way does not necessarily correlate in a simple way with distributional patterns of existing words. Aronoff (1976) emphasizes this with the observation that total numbers don't help establish relative productivity. For example, while more English words ending in *-ive* take *-ness* than *-ity*, more words ending in *-ile* take *-ity* than *-ness*. Furthermore, he notes that the more productive an affix is, the less likely it is that any given word formed with the affix will already be in one's lexicon. One result is that published

word lists (e.g. dictionaries) tend to systematically underrepresent words formed with highly productive affixes. The true nature of morphological productivity only becomes clear when one examines neologisms (natural or experimentally induced, produced or comprehended) or hapax legomena (words, whether old or new, that appear only once in a large corpus). Using such methods, Baayen and Renouf (1996) confirm many facts about the productivity of various kinds of morphology in English, for exampling showing that -*ness* is indeed more productive than -*ity* overall.

Providing a model for morphological processing is not a goal of this paper. Thus I have little interest in the question of whether words can be stored as wholes and yet still encode their internal morphological structure (see discussion in Feldman 1995, among many other places), though I'm not above exploiting positions in this controversial area that help me through some difficult spots. I also have no interest here in harmonizing evidence that regularly inflected words are stored in memory (e.g. Bybee 1995, 1996; Sereno and Jongman 1997; Stemberger and MacWhinney 1988) with evidence that they are not (e.g. Pinker 1991; Marcus, Brinkmann, Clahsen, Wiese, and Pinker 1995; Myers and Guy 1997), merely assuming that, by definition, words involving more productive morphology are built on-line more often than words involving less productive morphology.

Then there is the problem of <u>productive allomorphy</u>. Linguists have long noted the irregular nature of lexical phonology, and some have decided to analyze the most troublesome cases with allomorphy rules embedded under the "real" phonology (e.g. Aronoff 1976; Booij 1997). This assumption will not be necessary in my approach, and in fact I think it goes against the spirit of what I am trying to do. By contrast, what I mean by productive allomorphy is allomorphy that actually seems to happen partially "outside" the lexicon in some sense. Specifically, in terms of Hayes (1990) and Tsay and Myers (1996), lexically distinct allomorphs can be actively selected in real time for placement in appropriate contexts. For example, English speakers can choose between *a* and *an* in real time, as indicated by their behavior with novel forms generated through speech errors (Fromkin 1971) or variations due to phoneticky processes like h-deletion in unstressed syllables (e.g. Kaisse 1985). It is true that in this case there is a logic to which allomorph is chosen when (e.g. *an cow* would violate NOCODA while *a cow* does not), but we still must assume that only this morpheme has this option (cf. *can eat/*ca drink*). In the OT literature, such phenomena have been handled by including the alternative allomorphs in the input and letting general markdness constraints choose among them (e.g. Anttila 1997; Tranel 1994).

What makes productive allomorphy a problem is that, like morphological generativity, once we turn it on, we can't turn it off. Hayes (1990) provides several examples where productive allomorph selection is not driven by phonological factors at all. Tsay and Myers (1996) even present evidence that the complex phrasal process of tone sandhi in Taiwanese, which affects virtually all morphemes in the language, involves allomorph selection alone, and otherwise no phonology at all. The existence of this human ability to select allomorphs on-line thus allows for the possibility that some of lexical phonology could actually be productive allomorphy, e.g. the voicing assimilation associated with the very productive English suffixes -*s* and -*ed*. If such phonology is indeed lexical (see Mohanan 1995), then I must acknowledge an additional source of lexical phonology beyond what I call analogy. Since I don't plan to deal with

everything in one paper, I will mostly ignore this important problem in the remaining discussion, except to point out that it's a problem for everybody.

In short, I will simply assume the following syllogism, where (c) follows automatically from premises (a) and (b).

(1)    a.    Tokens of words generable by more productive morphology are less likely to have a direct counterpart stored in the lexicon than those generable by less productive morphology.

        b.    Analogy only affects items stored in the lexicon.

        c.    There is therefore an inverse correlation between morphological productivity and analogy.

Note, by the way, that I never define the notion "word." This is an extremely difficult thing to define, especially in languages like Chinese (see e.g. Li and Thompson 1981). Thus in principle the above syllogism can apply to what have traditionally been called phrases rather than words, meaning analogy (i.e. "lexical phonology") can potentially hold at the phrasal level as well. However, since the combinatorial possibilities (i.e. productivity) of phrases far outstrip those of words, analogical effects at the phrasal level will be extremely limited. To simplify discussion, I will therefore focus on "words" as intuitively understood in languages like English.

In any event, during the course of this paper, I will point out some interesting consequences of this inverse correlation between morphological productivity and analogy.

## 1.3    What analogy is

If the knowledge of lexical phonology consists in nothing more than knowing lexical items, by what mechanism can this knowledge ever extend to novel forms, as the above cited "external" evidence shows that it occasionally can? My answer, of course, is analogy. This term was adopted by historical linguists in the Neogrammarian tradition to deal with semi-systematic lexical exceptions to otherwise general sound changes (see e.g. Bynon 1977). The intuition is that such exceptions arise through the influence other lexical items, specifically items that are related both phonologically and non-phonologically (e.g. morphologically). For example, in older forms of English (and even in many modern varieties), the past tense of *dive* is *dived*. The replacement of *dived* with *dove* in some varieties is described as resulting from the completion of a four-part proportional analogy of the form *drive:drove::dive:X*. Another kind of analogy, paradigmatic leveling, reduces allomorphy within a productive paradigm. For example, English plural forms were formerly as varied as they still are in modern German, but eventually the plural paradigm was simplified by reducing allomorphs to the now dominant /s/, /z/ and /əz/.

I borrow the term analogy to mean something similar, but not identical, to this use in historical linguistics. I am interested in the knowledge in the head of an individual, mature speaker of some specific language. Thus by "analogy" I mean the ability of such a speaker to generalize from specific items in a fully specified lexicon. This generalization can sometimes be

revealed through "external" evidence (e.g. language change, acquisition or experiments), but I assume that the ability to do so is part of linguistic competence, not just performance. My goal is to capture this view in a workable formal model.

By the way, some suggest that the very concept of "synchronic analogy" is misconceived, since analogy describes diachronic sound changes which arise through mechanisms outside of grammar proper (see e.g. Reiss 1998; Hale, Kissock and Reiss 1998). But I am not proposing my formalism of analogy as an explanation of diachronic phenomena; the impossibilty of analyzing language change with grammatical models unmediated by performance factors was recognized a long time ago in the generative literature (e.g. Kiparsky 1978). Indeed, as I point out later, I don't think that many cases of paradigmatic leveling really involve "analogy" in my sense at all. Regardless of what may be going on diachronically, what I call analogy certainly does seem to be a synchronic force, as we will see, and the empirical benefits that result from acknowledging this will speak for themselves.

## 2.    Analogy in English lexical phonology

Before discussing how to formalize analogy in OT and the many issues this raises, I simply want to convince my readers that all the hard work is empirically necessary:   lexical phonology involves generalizations that derive from existing lexical items, not from general rules or universal constraints.

The argument runs as follows.   Again and again, when lexical phonology is studied closely, it is found to have at least three properties indicative of the pairwise word comparisons that define analogy.  First, as everyone already acknowledges, lexical phonology is irregular:  it is impossible to predict why it applies precisely in the words that it does.  In traditional theories, this is considered an inconvenience rather than the fundamental clue for the true nature of lexical phonology.

Second, the classes of words affected by a particular lexical phonological pattern are not totally incoherent, but their cohesiveness can only be defined by <u>family resemblances</u> (Bybee and Slobin 1982).  The notion of family resemblances, first suggested by Wittgenstein (1953), and now forming a cornerstone of prototype theory (e.g. Rosch 1973), is that a category can be coherent even in the absence of a property that is shared by all members, simply by virtue of pairwise similarity among its members.  The scientists may have their categorical definitions for fish, but for real people a whale is a fish since it shares many properties with other fish.  If lexical phonology were driven by universal constraints, we would not expect to find pairwise comparisons between specific words to be very meaningful.  Since most phonologists have assumed that lexical phonology is driven in the same way as postlexical phonology, very little attention has been paid to the enormous amount of evidence for family resemblances in the lexicon (but see Bybee 1985, 1988, 1994, 1996).  In this paper I attempt to catalog some of the most obvious of these in some of the most famous patterns in lexical phonology, showing that the evidence for pairwise comparisons is simply overwhelming.

Third, lexical phonological patterns are <u>nondirectional</u>.  In derivational theories (e.g. Halle 1962, Chomsky and Halle 1968, Kiparsky 1982), the assumption was that morphologically complex words are derived from morphemes.  As we will see later, the OT approach to the

lexicon is quite similar to that of Chomsky and Halle (1968). In particular, in the OT literature the directional assumption is maintained by the use of inputs for morphologically complex words that consist of separate morphemes, and for some purposes the fundamental role of the base has been enshrined in principles like Base Identity (Kenstowicz 1995). The directional hypothesis makes two clear predictions: (a) all of the phonological properties of a derived form should be predictable from the phonological (and perhaps morphological) properties of its component parts, and (b) none of the phonological properties of a base form should be predictable from the phonological properties of any of the forms derived from it. Both of these predictions are false, as we will shortly see. This result is expected given my view of the lexicon as a list of whole words, and my view of lexical phonology as a set of pairwise comparisons between these words. Since no word is derived from the other, complex forms can take on new properties not found in the associated base forms, and base forms can borrow properties normally found only in the associated complex forms.

All of my examples for these points come from English phonology, by far the best-studied phonological system in the world. This will allow me to take advantage of all this previous work, especially since the rhetorical effect of the arguments is strengthened if the reader is already satisfied with the validity of the lexical generalizations I will be picking apart. I begin with highly irregular cases, then move to vowel alternations, continue with some consonant alternations, and finish with some discussion of how these patterns interact. In the course of the discussion, many interlocking facts are found to support the analogical approach and no other. The result is a view of English lexical phonology quite different from that in the standard literature, but much closer to reality.

## 2.1 Irregular inflection

I begin with a prototypical case of lexical phonology as analogy: the vowel alternations in irregular past tense inflection in English. I wish to show that (a) it is not random, showing clear phonological regularities; (b) nevertheless, it's a mess, defeating all previous formal approaches; (c) it betrays diagnostics of analogy. I will reserve a formal analysis for a later section.

I call this a prototypical case, since there is much agreement, even among strong proponents of the view that the linguistics of words can be handled by general rules and/or constraints, that irregular inflection cannot be so handled. For example, in a paper on "the psychological status of rules of grammar," Pinker and Prince (1992:233) suggest:

> ... the reason they [e.g., patterns involving irregular inflection -- JM] don't
> act like bona fide rules is that they are not rules at all, but epiphenomena of
> the way structured lexical entries are partially superimposed in memory.

Putting the claim into slightly more familiar jargon, patterns in irregular inflection arise not by rule, but by what Bybee and Slobin (1982) call schemas: relatively loose, nonderivational generalizations across form classes in a particular lexicon.

As an example, consider the following list of all irregular English verb stems with [ay] in the present form, arranged by the form of the vowel in the past tense (Bloch 1947).

(2)    [o]       bide-bode, drive-drove, ride-rode, rise-rose, shrive-shrove, smite-smote, stride-strode, strive-strove, thrive-throve, write-wrote
dive-dove [some dialects]
shine-shone [some dialects]

        [ɪ]       bite-bit, hide-hid, light-lit, slide-slid

        [ʌ]       strike-struck

        [aw]    bind-bound, find-found, grind-ground, wind-wound

        [ɔ]       buy-bought, fight-fought

        [u]       fly-flew

        [a]       shine-shone [some dialects]

Those who would claim that such cases are simply memorized by rote miss some rather clear phonological generalizations. One of these generalizations is absolute: irregular verbs containing the rime [aynd] always have past tense forms with the rime [awnd]. Other generalizations may not be absolute, but they are still systematic. For example, irregular verbs ending in [ayC], where C is a coronal stop, have a past tense form ending in [oC], [ɪC], or [ɔC], never the other four possibilities. Likewise, forms that show the alternations [ay]-[o] always have a coronal or simple voiced obstruent coda, never ending in [k] or [nd] and never being open. Any of these sorts of generalizations can potentially be extended by native speakers. This has happened historically, where as already mentioned, the past tense of *dive* became *dove* in some dialects (as well as *shone* changing pronunciation from [ʃan] to [ʃon]). Extensions of the generalizations have also been demonstrated in experiments (e.g. Bybee and Slobin 1982, Prasada and Pinker 1993) and documented in language development (Xu and Pinker 1995). Thus these alternations do indeed count as "linguistically significant," as many generative phonologists have recognized (e.g. Halle and Mohanan 1985, Halle and Marantz 1993, Jensen 1993).

Nevertheless, such generalizations cannot be formalized with anything more general than loose schemas, since the patterns are never truly absolute: any given phonological property of an irregular form is neither necessary nor sufficient to predict which pattern it will conform to. Even the "absolute" [aynd]-[awnd] alternation is revealed as idiosyncratic when more words are considered: the past tense of *mind* is *minded*, not *\*mound*. Thus any analysis of irregular inflection using general rules must resort to arbitrary diacritics to indicate which irregular forms undergo which rules. The same is true for standard OT, with the additional problem that these irregular alternations are not motivated by universal constraints of any obvious sort.

Of course, this doesn't mean that some scholars haven't tried to use general rules to analyze these vowel alternations (e.g. Halle and Mohanan 1985; Halle and Marantz 1993; Jensen 1993). For example, Halle and Mohanan (1985) suggest that the [ay]-[o] alternation is the result

of the serial application of two independently motivated rules. The first lowers vowels in irregular pairs such as *sit-sat* and *choose-chose*, and the second backs vowels in irregular pairs such as *dig-dug* and *get-got*. If we assume that the [ay] of *write* is really /i:/ (before it undergoes vowel shift), then *wrote* can be derived by first lowering /i/ to /e/, and then backing to /o/. As Jensen (1993) observes, however, there are more than a few problems with this approach. First note that these putative phonological rules are triggered by morphological context, not phonological context, which makes them more like morphological rules. But they are very curious morphological rules, since they both have exactly the same meaning ("PAST"). So why can't we just apply one rule, and get *write-*wret* or *write-*wrut* instead? On top of this, notice that the rules must apply in a specific order; the opposite order would change /i/ into /ʌ/ and then into /a/, deriving *write-*wraht*. What other kind of morphology comes in two extrinsically ordered steps? Jensen (1993) suggests that the morphs involved in the alternation can be reconceived as floating autosegments that have the same phonological effect as rules, something like tonal morphemes in African languages (e.g. *sit* + [+low] → *sat*), but he himself notes that this doesn't deal with the problems of redundancy (e.g. PAST in *wrote* is both [+low] and [+back]) and extrinsic ordering. And naturally, none of this attempts to explain why *write* is irregular but *right* isn't. You can't escape diacritics at some point in this sort of approach.

Thus so far we have argued that irregular inflection involves phonology, and yet phonological theories using only absolute categorical rules or constraints cannot describe it. The way out of the dilemma is indicated by an important observation about the pattern: the classes of verbs that show a particular alternations are defined by family resemblances (first noted by Bybee and Slobin 1982).

The relevance of this concept to the vowel alternations in irregular inflection is easy to express. In the following figure I list the base forms in the [ay]-[o] class in a way that shows how the class derives its coherence from pairwise comparisons.

(3)  a.   [lab,+cont,+voice]      drive, shrive, strive, thrive
     b.   [cor,+cont,+voice]      rise
     c.   [cor,-cont,+voice]      bide, ride, stride
     d.   [cor,-cont,-voice]      smite, write

The [ay]-[o] class is coherent because the rimes of the (a) subclass are identical, and because the rime of the (b) subclass differs from (a) in only one feature (Labial vs. Coronal), and because the rimes of the (c) subclass differ from (b) in only one feature ([+/-continuant]), and because the rimes of the (d) subclass differ from (c) in only one feature ([+/-voice]). In dialects in which *dive* is also in the [ay]-[o] class, it falls into the (a) subclass. In dialects in which *shine* is in the [ay]-[o] class, it forms its own subclass that differs from the (c) subclass by only one feature ([+/-nasal]).

The insight is that classes such as [ay]-[o] are built from the bottom up, by making links among individual lexical items, rather than defining the class top-down through a general phonological environment or an arbitrary diacritic. The same thing is true of the [ay]-[aw] class

([nd] is always present) and the [ay]-[ɪ] class (the coda is always a coronal stop; I'll discuss this class more fully later). The other classes are too small to say much of anything.

So far I have demonstrated that irregular inflection shows two properties of lexical phonology that are expected in my approach: irregularity and family resemblances. The third property, nondirectionality, can be seen if we look at irregular verb classes defined by the past tense forms. For example, the above list gives the misleading the impression that past tense forms ending in [ɔt] always alternate with [ayt] in the stem form. If true, this alternation could conceivably be described by a rule or constraint of some sort, e.g. ayt → ɔt / [_]$_X$, where X is some triggering factor. This approach would miss an important fact, however: the rime [ɔt] actually appears in past forms with a bewilderingly wide variety of stem forms, as shown below. A directional approach would require several rules, almost as many words are involved in the pattern (data from Bloch 1947).

(4)  a.   [ay]    buy-bought
          [ayt]   fight-fought
     b.   [ɪŋ]    bring-brought
          [ɪŋk]   think-thought
     c.   [ərk]   work-wrought
          [ik]    seek-sought
     d.   [i:tʃ]  beseech-besought, teach-taught
          [ætʃ]   catch-caught

The properties of the past tense forms are thus not necessarily derivable from properties of the associated stem forms. Note however that the X-[ɔt] class is itself not incoherent: the set of stem forms derive cohesion through family resemblances, as indicated by my arrangement in the above figure. (I note in passing that this observation is somewhat problematic for Bybee and Slobin 1982, since their "schemas" affect individual words and are not sensitive to word pairings; this is not a problem for my formalism, which can represent both analogies sensitive to word pairings and analogies that act like true schemas).

I think we are forced to the conclusion, therefore, that the vowel alternations in irregular inflection must be due to some sort of analogy, however this may be formalized. Of course, the pairwise comparisons that join members of an irregular class are still unpredictable; no speaker is compelled to say *dove* rather than *dived* merely by virtue of knowing *drove*. As I have shown, though, the correct response to such arbitrariness is not to resort to completely arbitrary diacritics. Rather, what is needed is a device that is just arbitrary enough. This, then, is what we will be trying to invent in later sections. Moreover, it should already be obvious that any device powerful enough to handle the messy vowel alternations of irregular inflection will be powerful enough to handle all of lexical phonology (and hopefully nothing more).

## 2.2    Irregular inflection and the Scottish Vowel Length Rule

As part of my strategy to lead the skeptical reader gradually from the traditional view of analogy as marginal to a view where it plays a central role in lexical phonology, I now turn to a case where irregular inflection interacts with a lexical pattern widely acknowledged to be "linguistically significant."  The interaction will be shown to be explainable only if the lexical pattern is described in the same way as the irregular inflection, which means, of course, with analogy.

The particular pattern occurs in Scottish English and affects vowel length, and is therefore known as the <u>Scottish Vowel Length Rule</u> (SVLR; see e.g. Aitken 1981; Allan 1985; McClure 1977; McMahon 1991; Scobbie, Hewlett and Turk 1999).  At least in some varieties, it also affects the quality of certain vowels.  In particular, the diphthong /ay/ alternates between a long form something like [aə] and a short form something like [ʌy].  The phonetic particulars don't concern me here (see J. Myers 1997 for some theoretical discussion); hence I will transcribe these alternants simply as [ay:] and [ay].  To the best of my knowledge, the interaction of SVLR with irregular inflection has only been clearly documented in the variety of Scottish English spoken in the village of Glenoe, Northern Ireland, about twenty miles north of Belfast (Gregg 1958, 1959, 1973, 1985; see J. Myers 1994 for an earlier discussion of the implications of this pattern).  All data will therefore come from the work of Gregg on this dialect unless otherwise noted.

In all varieties of Scottish English, the phonological conditions for SVLR are syllable structure, voicing and continuacy:  the long alternants are found in open syllables and before voiced continuant codas, while the short alternants are found before voiceless stop codas.

(5)    SVLR in Glenoe

a.    Relevance of open syllables:

| [tay:] | "tie" | [tayd] | "tide" |
| [fay:əl] | "phial" | [fayl] | "file" |

b.    Relevance of [+voice]:

| [fay:v] | "five" | [fayf] | "fife" |
| [pray:z] | "prize" | [prays] | "price" |

c.    Relevance of [+continuant]:

| [say:ð] | "scythe" | [bəsayd] | "beside" |

SVLR is indubitably lexical (Borowsky 1993; McMahon 1991).  First, in certain words the short form [ay] unexpectedly appears in "long" environments, thus representing

unpredictable lexical exceptions. The two examples in the Glenoe variety given in Gregg's work are listed below.

(6)     [gay]  "very"          {cf.    [gay:] "guy"}
        [ʃay]  "shy"           {cf.    [stay:] "(pig) sty"}

    Second, as has often been noted, morphological structure plays a role in SVLR. As illustrated below, the long alternant [ay:] can appear in a "short" environment if what makes the environment "short" can be analyzed as morphologically derived.

(7)     a.     [tay:] "tie"     [tay:d]    "tied"       {cf. [tayd]    "tide"}
        b.     [may:]"my"       [may:n] "mine (pron.)"   {cf. [mayn] "mine (n.)"}

    Note that, contrary to the analysis in Borowsky (1993), such violations of the SVLR pattern can occur even with the patently unproductive morphology found in *my-mine*, though presumably such cases are less common. The same point can be made for the dialects examined by Aitken (1981), which apparently show the pattern shown below (though McMahon 1991 seems doubtful). Observe that here both regular and irregular inflection violate the expected SVLR pattern.

(8)     [ro:d] "rowed"        [ro:d] "rode"          {cf. [rod]       "road"}

    So far we seem to have a rather "normal" example of lexical phonology. SVLR seems eminently expressible in standard phonological theory, and even appears to show signs of phonetic motivation (see J. Myers 1997), implying that it could be handled by universal OT constraints. On top of all this, SVLR is historically akin with the various vowel lengthening and shortening rules of standard English, and the vowel height alternations often seen with it form an essential part of the vowel shift alternations claimed to be so central in English segmental phonology (Chomsky and Halle 1968, Halle and Mohanan 1985).

    What makes SVLR problematic, at least in varieties like that found in Glenoe, is that it systematically fails to hold in specific classes of irregular forms. Defining these classes turns out to require precisely the sort of pairwise lexical comparisons discussed in the preceding section.

    Consider the pronunciations in Glenoe of the present forms of irregular verbs of the [ay]-[o] class that end in voiced continuants. Given SVLR, we expect the long alternant [ay:], but instead we get the short alternant [ay].

(9)     [drayv]        "drive"      [rayz]         "rise"
        [strayv]       "strive"     [θrayv]        "thrive"

    This exceptionality is systematic in three ways. First, as just observed, it affects all relevant verbs of the [ay]-[o] class, not just some of them. Second, it does not appear to affect all irregular verbs with [ay] in the present form, just those that alternate with [o] in the past

form.  The only relevant comparison form in Gregg's work is shown below, which shows the long alternant in an open syllable as expected.  (The SVLR doesn't apply before nasal stops, so the word *shine*, which alternates with *sh[a]ne*, is not relevant.)

(8)     [bay:]          "buy"

The third sign of systematicity is illustrated below:  SVLR isn't violated in any regular verbs containing [ay] (*dive* is regular in Scottish English).

(9)     [əray:v]      "arrive"      [day:v]      "dive"
        [pray:z]      "prise"       [rəvay:z]    "revise"

That this systematicity is indeed part of speakers' competence and not merely a historical quirk is demonstrated by the following amazing observation, reported in Gregg (1973, 1985):  the pronunciation of *strive* in Glenoe depends on whether or not a given speaker treats it as regular or irregular.  If for some speaker the past tense is *strove*, then *strive* is pronounced with the short alternant [ay].  If however for some speaker the past tense is *strived*, then *strive* is pronounced with the long alternant [ay:].

The problem, then, is this:  How can we possibly tie violations of SVLR to the property of being in the [ay]-[o] class?  The simplest approach would be simply to mark this class as exceptions to SVLR, but then we can raise the same objections we raised in the previous section.  Namely, such a diacritic could only appear on verbs with [o] in the past tense, which is at best redundant, and at worst nonexplanatory; we would prefer some way that would allow the [o] itself to do the necessary work.  But this is awkward to do in approaches that assume directionality, since the [o] is in the derived form, not the base form.  The Glenoe pattern thus represents a violation of the second prediction of the directional approach, since a phonological property of the base form is predicted by a phonological property of the derived form rather than the other way around.

To give alternatives to analogy their best shot, let's suppose that we allowed "backwards" derivation to happen in certain circumstances.  Even making this major concession to my view of lexical phonology, the best way to describe the blocking of SVLR would be to order it with some rule o → ay / [_]$_X$ in a counterfeeding relation, as illustrated by the following derivations, where *dive* is assumed to be regularly inflected.

(10)                     dive          drive
        UR               /dayv/        /drov/
        SVLR             day:v         --
        o → ay           --            drayv

In the heyday of derivational theories of phonology, such an analysis perhaps would have carried some weight, but the OT literature has long recognized the difficulty of expressing opaque interactions like this with constraint ranking.  Booij (1997) suggests that such ordering effects

should be handled by the universal ordering of lexical before postlexical phonology, but this is clearly not applicable here.  McCarthy (1998) provides a formalism for handling opacity in OT, called sympathy theory, but this depends on the use of universal markedness constraints, which o→ay clearly is not (I will have more to say about sympathy theory later).  There is thus no satisfying way for OT, as it is currently understood, to deal with the Glenoe pattern.  Whether the derivational approach sketched above is satisfying is a matter of taste, but I'll assume most of my readers share my own distaste for it.

Gregg himself points the way out of this dilemma:  analogy.  A large proportion of verbs in the [ay]-[o] class end in consonants that are stopped, voiceless, or both, which means that SVLR cannot apply.  SVLR is thus blocked in the other verbs by analogy to these.  To see this, I once again list the words in this class arranged into two subclasses:  those where SVLR could potentially apply (a), and those where it could not (b).

(11)    a.    drive, shrive, strive, thrive, rise
        b.    bide, ride, stride, smite, write

We can thus describe the violations of SVLR as Gregg does, with four-part proportional analogies of the form *rode:r[ay]d::drove:X.*  Words like *drive* are driven to have the same short vowel as in words like in *ride*, which have short vowels in accordance with SVLR.

This analysis is more satisfying than any of the alternatives for several reasons.  First, we have already seen from the preceding section that the correct of analysis of the [ay]-[o] class must use analogy; alternative analyses ignore this at the cost of descriptive and explanatory adequacy.  Second, Gregg's approach correctly predicts the existence of irregular verbs in the [ay]-[o] class where SVLR behaves normally.  Analogy could not work otherwise, but the alternative analyses do not make anything like this prediction; for non-analogical approaches, the behavior of *ride* is completely irrelevant to the behavior of *drive*.  Of course, in this one case, this could be a coincidence, but as we will see, lexical phonology always has such analogical "drivers."  Third, analogy doesn't have to happen in the same direction in every case.  In the historical literature, instances of "backwards" analogy like that in Glenoe, where derived forms influence base forms, are not all that rare (see e.g. Chapman 1995 for some instances in Swiss German dialects).

Finally, combined with the assumptions about productive morphology described in an earlier section, analogy provides a simple account for why *strive* ceases to violate SVLR for speakers who treat it as regularly inflected.  For these speakers, the word *strove* is not listed in the lexicon at all; regularly inflected forms like *strived* are usually generated outside the lexicon and cannot participate in analogies.  Hence no analogy like *rode:ride:strove:X* can be made, and SVLR works as usual.  Thus the difference of these speakers from the others is explained solely by dropping *strove* from their lexicons.  In the alternative approaches, however, we would have to make additional changes in the lexical representation of the word *strive*.

Recognizing the violations of SVLR as analogy immediately raises the question of the status of SVLR itself.  How could a general phonological pattern be blocked by analogy?  In derivational terms, how could a derivational process occur *before* a process of pairwise word

comparisons (as in the derivations in (10))? In OT terms, how could "analogy," whatever that is, outrank the general markedness constraints that presumably motivate SVLR? The solution I will pursue is that SVLR, like all of lexical phonology, is itself a form of analogy. Its interaction with irregular inflection is thus a conceivable possiblity rather than a total mystery, even if it doesn't happen in all varieties of Scottish English.

I will analyze this formally later, but intuitively, SVLR would involve pairwise comparisons like *tide-ride* and *prise-rise*. Given that [ay] is short in *tide*, this property would spread to *ride*, and then back again, mutually reinforcing the pattern. Given that [ay:] is long in *prise*, the same mutually-reinforcing spreading would occur in *prise* and *rise*. There could be as many such pairwise comparisons as there are words to pair up, resulting in a powerful network of mutually reinforcing lexical analogies.

The interaction with irregular inflection is then accounted for as follows. Overlapping with the SVLR network are a network of proportional analogies that enforce pairwise comparison between words like *ride* and *rise*. These two words are thus subject to a force that drives them to have the same vowel, but this force is diametrically opposed to the forces that give rise to SVLR. In principle, three things could happen: the vowel mismatch could be ignored, it could be resolved by making both short, or it could be resolved by making both long. Most varieties of Scottish English choose the first path, presumably because the SVLR pattern is so "strong" in some sense (this notion will be formalized later). Glenoe chooses the second path. It is probably not possible to be certain why one path was taken over the other in any particular instance, but what is explainable is why Glenoe's path is available to be taken at all.

Again, then, what is needed is a formalism of analogy that is explanatory and yet still arbitrary enough to allow the freedom for random cross-linguistic variation and idiosyncratic behavior within the lexicon. As we will see, my formalism does just this, and it does it without needing to actually use explicit "networks" as is done in connectionist models (e.g. Rumelhart and McClelland 1986).

## 2.3     Vowel alternations in semi-weak verbs

I have shown that vowel alternations in irregular inflection must be handled by analogy, not by general rules or constraints. This may seem convincing in the cases of highly irregular ablaut that I have discussed so far, but we must also consider vowel alternations in the so-called semi-weak verbs, i.e. verbs with irregular phonology that nevertheless appear to end with a suffix realized as [t] or [d]. The presence of this suffix seems to provide a phonological conditioning environment for the vowel alternation, and in fact, it is often argued that the pattern seen here can be described by precisely the same general rules that handle many other vowel alternations in English lexical phonology (e.g. Chomsky and Halle 1968; Halle and Mohanan 1985; S. S. Myers 1987; Rubach 1984, 1996; Yip 1987).

I will deal with these claims in two sections. Here I examine the pattern in irregular inflection, showing that the same logic that earlier argued for analogy in irregular inflection continues to work in this special case. Claims about vowel alternations in other morphological environments are examined in the next section.

Briefly, the claim in the above works is that vowels in semi-weak past tense forms undergo a vowel-shortening process triggered by the presence of a consonant cluster created by the suffix. The shortened vowels are then unable to undergo the rule that shifts vowel quality in long vowels. This is true even in semi-weak past tense forms that don't end in consonant clusters on the surface, since the shortening process occurs before the suffix is deleted by a cluster simplification process. Some forms also involve a voicing assimilation process affecting obstruents. Derivations showing this "classical" analysis are given here.

| (12) | "sleep" | "slept" | "bite" | "bit" |
|------|---------|---------|--------|-------|
| UR | sle:p | sle:p+t | bi:t | bi:t+t |
| Shortening | -- | slept | -- | bitt |
| Vowel shift, etc | sli:p | slɛpt | bayt | bɪt |

The pattern looks quite regular, but as I now demonstrate, this impression is solely due to judicious selection of examples. When all of the relevant examples are examined, it quickly becomes obvious that the pattern illustrated above is actually just one of many overlapping schema-like generalizations holding across subclasses of the semi-weak verbs. In addition to these irregularities, the shortening/vowel-shift pattern itself shows the two other diagnostics of analogy, family resemblances and nondirectionality.

In the tables below, I list all irregular verb stems given in Bloch (1947) whose past tense ends in /t/ or /d/, except those that seem to involve auxiliaries rather than verbs (*ought-ought*, *must-must*, *need-need*, *can-could*, *shall-should*, *will-would*, *may-might*) or show total suppletion (*go-went*). The other items in this standard source (also used by Halle and Mohanan 1985, among others) all seem to be quite familiar and typical irregular verbs, though the behavior of some varies somewhat across dialects. The tables arrange the examples into several categories chosen to show just how much is, and is not, accounted for by the classical analysis. To emphasize these points, I surround pairs that show the classical alternations with double borders. Pairs that are vacuously consistent with the classical analysis are surrounded with thick borders. The remaining pairs surrounded by thin-line borders violate the classical pattern and thus must be accounted for some other way.

(13)     Semi-weak verbs in English

| Final d or t in present form | | | | | | | |
| --- | --- | --- | --- | --- | --- | --- | --- |
| No change | | | Only C change | V change | | | |
| Short V | | Long V | | Stays long | Stays short | Shortens | |
| Final t | Final d | | | | | Vowel shift | Not vowel shift |
| bet-bet | bid-bid | beat-beat | bend-bent | eat-ate [some dialects] | get-got | bite-bit | fight-fought |
| burst-burst | gird-gird | | build-built | smite-smote | sit-sat | eat-ate [some dialects] | hold-held |
| cast-cast | rid-rid | | lend-lent | write-wrote | bid-bad | light-lit | |
| cost-cost | shed-shed | | rend-rent | bide-bode | stand-stood | meet-met | |
| cut-cut | spread-spread | | send-sent | bind-bound | | shoot-shot | |
| fit-fit | tread-tread | | spend-spent | find-found | | bleed-bled | |
| hit-hit | wed-wed | | | grind-ground | | breed-bred | |
| hurt-hurt | | | | ride-rode | | feed-fed | |
| knit-knit | | | | stride-strode | | hide-hid | |
| let-let | | | | wind-wound | | lead-led | |
| put-put | | | | | | plead-pled | |
| quit-quit | | | | | | read-read | |
| set-set | | | | | | slide-slid | |
| shut-shut | | | | | | speed-sped | |
| slit-slit | | | | | | | |
| spit-spit | | | | | | | |
| split-split | | | | | | | |
| sweat-sweat | | | | | | | |
| thrust-thrust | | | | | | | |
| wet-wet | | | | | | | |

| No final t or d in present form | | | | |
|---|---|---|---|---|
| No V change | | V change | | |
| Stays short | Stays long | Shortens | | Stays short |
| | | Vowel shift | Not vowel shift | |
| burn-burnt | spoil-spoilt | bereave-bereft | beseech-besought | bring-brought |
| learn-learnt | make-made | cleave-cleft | seek-sought | catch-caught |
| smell-smelt | | creep-crept | teach-taught | think-thought |
| spell-spelt | | deal-dealt | hear-heard | tell-told |
| spill-spilt | | dream-dreamt | buy-bought | sell-sold |
| have-had | | feel-felt | do-did | wreak-wrought |
| dwell-dwelt | | keep-kept | flee-fled | |
| | | kneel-knelt | say-said | |
| | | lean-leant [some dialects] | | |
| | | leap-leapt | | |
| | | leave-left | | |
| | | lose-lost | | |
| | | mean-meant | | |
| | | sleep-slept | | |
| | | sweep-swept | | |
| | | weep-wept | | |

The most fundamental observation to make is that this is a rather idiosyncratic data set. The proportion of pairs that provide positive evidence for the classical alternations is far from forming a majority (29% = 30/103), and even including the pairs that are vacuously consistent with the analysis (i.e. where the stem form already has a short vowel and there is no change in the past form), the total proportion of consistent pairs only reaches 68% (= 70/103). This leaves 33 items still unaccounted for, 32% of the total. These include direct violations of shortening (*beat-beat*, *spoil-spoilt*) and of vowel shift (*hold-held*, *hear-heard*).

Independent evidence for the existence of the triggering suffix is also rather hard to come by. A slight majority (62% = 62/103) of the pairs show no direct evidence for any [t] or [d] suffix, and of these, only six show indirect evidence through a consonant alternation (e.g. *bend-bent*). The form of the suffix is also unclear, because both [t] and [d] can show up in identical phonological environments, namely after sonorants.

(14)   a.   ∅~[t]:          burnt, leant, learnt, smelt, spelt, spilt, spoilt

　　　　　　[d]~[t]:          bent, built, lent, rent, sent, spent

　　　 b.   ∅~[d]:          sold, told, did, fled, said

　　　　　　[d]~[d]:          held, bound, found, ground, wound

Even if we assume that the suffix is normally [t], given that stem [d] can be replaced with [t] but never the reverse, it is still difficult to explain why the resulting cluster is simplified by deleting sometimes the suffix and sometimes the stem-final consonant.

(15)   a.   suffix → ∅:      bid, gird, rid, shed, spread, tread, wed,

　　　　　　　　　　　　 bound, found, ground, wound, ...

　　　 b.   stem C → ∅:     bent, built, lent, rent, sent, spent

Nevertheless, I should note that there does seem to be some "external" evidence for the semi-weak suffix, coming from the behavior of variable *-t/d* deletion, a process that optionally deletes [t] or [d] off the ends of words (e.g. *lift* may occasionally be pronounced *lif'* in fluent speech). This process, as I will have occasion to mention again later in this paper, is sensitive to morphology; thus [t] deletes far more often in monomorphemic *lift* than in regularly suffixed *laughed*. Interestingly, Guy and Boyd (1990) found that many English speakers also delete [t] or [d] less often in semi-weak verbs like *lost* than in monomorphemic forms, thus implying that such speakers have learned that *lost* falls into a distinct morphological class with a suffix that is less prone to deletion. However, this appears not to be the easiest thing for native speakers to learn; the youngest speakers in Guy and Boyd's (1990) study who treated semi-weak verbs differently in *-t/d* deletion were in their early 30's, and the average age was 49.

Of course, I do not argue that such idiosyncrasies are in themselves an argument against the classical analysis. Lexical phonology is expected to have idiosyncrasies, and as the quotation at the beginning of this paper indicates, scholars who merely point out exceptions are not considered to really be playing the game. Thus I now to turn to positive arguments for the role of analogy in the semi-weak verb patterns.

I begin with a quite basic observation: once you let synchronic analogy into lexical phonology, there is no principled way to contain it just to the "linguistically insignificant" cases. Suppose, for example, we try to harmonize the claim of general rules in the semi-weak class with the previous evidence for analogy in irregular inflection. In particular, suppose we make the claim that the 68% of the above verb pairs that are consistent with the shortening analysis are derived by general rules or constraints, and only the remaining 32% are handled by analogy.

The major problem with such a hybrid analysis is there are no criteria for choosing one alternation above all others as being "rule-governed" while the rest are something else entirely. True, the vowel alternations in pairs like *bite-bit* do form a plurality, and also play a role beyond irregular inflection, whereas vowel alternations like *write-wrote* don't extend very far. Yet even these "rule-governed" alternations don't extend indefinitely, failing to apply in pairs like *obese-obesity*, *reap-reaped*, and so forth. Do we want to say therefore that the only real phonology in English are the completely exceptionless processes? As Kiparsky (1975:195) pointed out,

"[phonological] productivity is traditionally and correctly viewed as a gradient phenomenon." We don't want to say that [ay]-[o] is not "real" while [i:]-[ε] is, but rather that the former is less productive than the latter, with both formally described using the same mechanisms. Since the former must be handled by analogy, as I have argued, then the latter must be as well, regardless of its significantly greater applicability.

Moreover, if the classical alternations found in the semi-weak verbs derive through analogy rather than a general rule, we expect them to show specific diagnostics of this, and they do. The first diagnostic to examine is the existence of phonological classes defined by family resemblances rather than the categorical presence/absence of particular features.

Consider the set of pairs that show positive evidence for the classical analysis (surrounded by double borders in the above tables). If the alternations here were due to general rules that are idiosyncratic through no fault of their own (i.e. because some words, like *write*, come with diacritics blocking the rules), then we'd expect to find in the stem forms all possible long vowels and all possible coda consonants. This isn't what we find. Instead, stem forms of the words that undergo the classical alternations fall into two quite distinct categories. If the stem form ends in a coronal obstruent, then this coronal is most often [d] (9 out of14), which always remains in the past tense form, and the stem vowels vary from [i:] (9) to [ay] (4) to [u:] (1). In sharp contrast, if the stem form does not end in a coronal obstruent, the final consonant in the past tense form is always [t] and the stem vowel is almost always [i:] (15 out of 16), with only one [u:].

What observations like these mean is that the class of semi-weak verbs that undergo the classical vowel alternations must be defined from the bottom up, through pairwise comparisons of particular lexical items. To illustrate this, in the following figure I've given the numbers of pairs that fall into various subclasses defined by stem consonants and vowels and whether their past forms show positive evidence of the classical alternations (double-line borders in above tables) or positive evidence of another alternation (single-line borders). Thus for example, if a stem form ends [i:d], its past form is more likely to end in the classical [εd] than if the stem ends in [ayd], whereas if it ends in [i:t], the chances of the past form ending in the classical [εt] are about even with the chances of a stem ending [ayt]. Similarly, if the stem ends [aynd], there is no chance that the past form will end in the classical [ɪnd]; if the stem ends [i:C] for C other than [t] or [d], the chances are high that the past form will end in the classical [εC], but not if the stem ends [V:C], where [V:] is any other long vowel.

(16)

| | | Stem form | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | t | | d | | | other C | |
| | | i: | ay | Vd | | Vnd | | i: | other long V |
| | | | | i: | ay | ay | other long V | | |
| Past form | Classical | 2 | 2 | 7 | 2 | 0 | 0 | 15 | 1 |
| | Other | 1 | 2 | 0 | 3 | 4 | 0 | 5 | 5 |

The class of stems that undergo the classical vowel alternations is thus defined by the pairwise comparisons indicated in the following list, where the rimes of the stems on each line differ from the previous solely by the given phonological properties.

(17)  a.                       deal, feel, kneel
        b.    [nasal]           mean, lean [some dialects]
        c.    [place]           dream
        d.    [voice, obstr] creep, keep, leap, sleep, sweep, weep
        e.    [voice, cont]  bereave, cleave, leave
        f.    [back, place]  lose
        g.    [voice, cont]  shoot
        h.    [back]           meet, eat [some dialects]
        i.    [low]            bite, light
        j.    [voice]          hide, slide
        k.    [low]            bleed, breed, feed, lead, plead, read, speed

This list is a bit misleading, however. A set of pairwise associations does not actually form a linear series; words like *bleed* (k), for example, are also similar to words like *meet* (h) and *deal* (a). Moreover, merely making such a list does not explain why these words have similar past tenses, since words like *write* are also similar to some items in this list, and yet have different past tenses.

To express in a more perspicuous fashion the pairwise associations that give rise to the "regular" alternations [iː]-[ɛ] and [ay]-[ɪ], and the "irregular" alternation [ay]-[o], I give a chart showing differences between pairs of words. First, I give a set of key feature values that distinguish the rimes in verbs of these classes. (Forms like *lose, shoot* and *shine* are left out to save space.)

(18)

|  | deal | mean | dream | keep | leave | meet | feed | bite | hide | drive | rise | ride | write |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| place | cor | cor | lab | lab | lab | cor | cor | cor | cor | lab | cor | cor | cor |
| son | + | + | + | - | - | - | - | - | - | - | - | - | - |
| nasal | - | + | + | - | - | - | - | - | - | - | - | - | - |
| voice | + | + | + | - | + | - | + | - | + | + | + | + | - |
| cont | - | - | - | - | + | - | - | - | - | + | + | - | - |
| low | - | - | - | - | - | - | - | + | + | + | + | + | + |

Using these features, we can then derive the following chart, where the same stem forms are listed across both dimensions. Numbers in the cells indicate how many features disagree in a given pair. Differences greater than or equal to 4 have been bolded. (See Frisch 1996 for a much more sophisticated way of calculating such similarity relations between lexical items.)

(19)

| deal | mean | dream | keep | leave | meet | feed | bite | hide | drive | rise | ride | write | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 1 | 2 | 3 | 3 | 2 | 1 | 3 | 2 | **4** | 3 | 2 | 3 | deal |
| | | 1 | 4 | 4 | 3 | 2 | 4 | 3 | **5** | **4** | 3 | **4** | mean |
| | | | 3 | 3 | 4 | 3 | **5** | **4** | **4** | **5** | **4** | **5** | dream |
| | | | | 2 | 1 | 2 | 2 | 3 | 3 | **4** | 3 | 2 | keep |
| | | | | | 3 | 2 | **4** | 3 | 1 | 2 | 3 | **4** | leave |
| | | | | | | 1 | 1 | 2 | **4** | 3 | 2 | 1 | meet |
| | | | | | | | 2 | 1 | 3 | 2 | 1 | 2 | feed |
| | | | | | | | | 1 | 3 | 2 | 1 | 0 | bite |
| | | | | | | | | | 2 | 1 | 0 | 1 | hide |
| | | | | | | | | | | 1 | 2 | 3 | drive |
| | | | | | | | | | | | 1 | 2 | rise |
| | | | | | | | | | | | | 1 | ride |
| | | | | | | | | | | | | | write |

The crucial point to observe is the greater amount of dissimilarity across the two classes (upper right quandrant of chart) in comparison to the dissimilarity within the "classical" class (upper left quandrant) and within the [ay]-[o] class (lower right quadrant). Within the classical class, only 19% (=7/36) of the pairings produce 4 or more differences, with only one pairing that has 5 differences, and within the [ay]-[o] class, there are no pairings with more than 3 differences. By contrast, 31% (=11/36) of the cross-category pairings have 4 or more feature differences, with three pairings having 5 differences. Statistical methods could make these sorts of observations more precise (though to do it properly pairings between all words should be considered, not just the subset illustrated here), but they are unnecessary to make the major point: semi-weak verb classes are defined by family resemblances, just like irregular inflection generally.

There is one final argument to make concerning the analogical nature of the classical alternations, and that concerns their nondirectional nature. Recall that nondirectionality can appear in two ways, namely when derived forms show influence of information other than what is contained in the base forms (e.g. *bring-brought*, *buy-bought* and *fight-fought*), and when base forms show influence of derived forms (e.g. the blocking of SVLR in Glenoe). The semi-weak verbs show both phenomena.

Among the 27 violations of the classical pattern that we have been discussing, three are particularly special: *do-did*, *say-said*, *flee-fled*. These are forms where the stem form does not end in a coronal obstruent, thus implying that a suffix has indeed been added, and where the output vowel is a short vowel that in other verbs can be described as derived via the shortening and vowel-shift processes. Yet in these words, the short vowel cannot be derived by these processes, since the stem forms are all open syllables; the suffix does not create a consonant cluster. Particularly frustrating is the pair *flee-fled*, since the vowel alternation is in fact exactly the same as in pairs like *bleed-bled*, but the open syllable in the stem means it cannot be analyzed the same way. Just as with the [øt] class, it appears then that the past tense forms of such verbs are under the influence of schemas that don't make reference to stem forms, e.g. *hid-did* and *bled-fled*.

Evidence for the influence of the derived (past) forms on the base (stem) forms is even stronger. Consider first the the stem-past pairs that are vacuously consistent with the classical

analysis because the stem forms already have short vowels (surrounded by thick borders in the above tables). A great majority of these stem forms end in [t] or [d] (86% = 37/43). This is unexpected in the rule-based analysis, since these shortened vowels must be underlying (there is of course no suffix on the stem forms), and so there is no reason in the classical analysis for the nature of the stem-final consonant to be at all relevant.

There is, however, a reason for this in an analogical analysis where past forms can influence present forms through pairwise comparisons. Since by definition all past forms in this set end in [t] or [d], these stem forms are similar to past forms. If there is a tendency for irregular past forms ending in [t] or [d] to have short vowels (a schema captured, albeit incorrectly, by the classical analysis), then this tendency can spread to the stem forms. The result is that underived stems ending in [t] or [d] already tend to have short vowels.

A closer examination just drives the point home. If we focus on the set of 27 semi-weak verbs with short vowels that show no changes between stem and past forms (e.g. *bet-bet, bid-bid*), we find that the range of vowels is extremely limited: 67% (=18/27) of them contain [ɛ] or [ɪ]. In the rule-driven analysis, these vowels must be underlying. Nevertheless, as we have seen, these are precisely the vowels that also appear most often in the past tense forms supposedly derived by shortening and vowel shift. In particular, semi-weak verb stems almost never contain the vowel [ey], the only example being *say-said*.

To reiterate, my point is not merely to undermine the classical analysis, and it is certainly not to claim that the patterns I've been discussing are not "real phonology" and should be ignored. On the contrary, my claims are positive. Just as many researchers have recognized, such patterns are genuine lexical phonology. However, they demonstrate many characteristics of analogy. The conclusion I wish to draw from such cases (and there are more to come) is that we need to give up on analyses of lexical phonology that depend on general rules or constraints, and instead develop a formal model of analogy.

## 2.4     Vowel alternations in derivational morphology

I now move beyond the ghetto of irregular inflection into the wide world of derivational morphology. The number of affected words naturally increases enormously, and it will no longer be possible for me to examine them all. Nevertheless, it is still straightforward to demonstrate the superiority of an analogical approach even in "regular" lexical phonology, regardless of how analogy should be formalized.

I begin by continuing my examination of vowel alternations. This not only allows me to build on what I have already discussed, but these alternations are correctly considered as playing the "central role" in English lexical phonology (in the words of Halle and Mohanan 1985:57), since they interact with so many other patterns, including stress and the consonantal alternations I'll consider later.

Since Chomsky and Halle (1968), the vowel alternations in derivational morphology are assumed to involve essentially the same processes as were claimed (incorrectly, as I've shown) to operate in the semi-weak verbs, namely a <u>vowel shift rule</u> that is sensitive to differences in vowel length (actually, Chomsky and Halle 1968 implicated vowel laxing, but viewing the issue as involving vowel duration has allowed researchers to imagine that the process is phonetically

natural; see especially S. Myers 1987). Jaeger (1986:78) provides a helpful chart showing the assumed vowel shift alternations as of Halle and Mohanan (1985) (the "official" alternations for the back vowels were somewhat different in Chomsky and Halle 1968). There are many difficulties with any attempt to describe these disparate vowel quality alternations with a single rule (see Halle and Mohanan 1985 for discussion of further necessary assumptions); I will mostly gloss over these important problems and concentrate on others.

(20)    [after (2) in Jaeger 1986:78]

|          |                      |
|----------|----------------------|
| [ay]-[ɪ] | divine-divinity      |
| [i:]-[ɛ] | serene-serenity      |
| [ey]-[æ] | sane-sanity          |
| [aw]-[ʌ] | profound-profundity  |
| [ju:]-[ʌ]| reduce-reduction     |
| [oi]-[ʌ] | destroy-destruction  |
| [u:]-[ɔ] | lose-lost            |
| [o]-[a]  | verbose-verbosity    |

        The major problem over the intervening years has lain in determining precisely what the conditioning environments are for the shortening and lengthening processes. In this section I remind the reader that there is no easy way to do this, the best possible analysis requiring a multiplicity of duration alternations that are frustratingly similar, but never similar enough to be reduced to a single process. I then show how analogy can actually make these alternations much more understandable.

## 2.4.1   Idiosyncrasies in the vowel alternations

        The most recent and complete analysis of vowel alternations in English is Rubach (1996). He reexamines the analyses of S. Myers (1987) and Yip (1987), both improvements over Chomsky and Halle (1968) and Halle and Mohanan (1985), and provides strong arguments that his own proposed analysis is better than all the previous ones. Therefore I will set the stage for my discussion by first outlining his analysis. All data come from these previous papers, from dictionaries, or from my own native mental lexicon. The goal here is primarily negative, namely to show that what is presented in the literature as a relatively straightforward process is actually just as complex and idiosyncratic as the vowel alternations in irregular inflection when examined more closely.

        Rubach (1996) implies that most vowel alternations in English are caused by a rule that shortens vowels in closed syllables. This (closed-)syllable shortening rule is claimed to account for all of the following alternation types.

(21)  a.      before consonant-initial suffixes:

sleep-slept, meet-met, describe-description

b.      before i-initial suffixes:

divine-divinity, mode-modify, Spain-Spanish

c.      before certain other vowels, suffixed or epenthetic:

grade-gradual, line-linear, table-tabular

d.      before morpheme-internal consonant clusters:

answer, country

This simple-sounding claim is achieved at a cost, however, since in order to make one rule handle all of these cases, many other additional assumptions are necessary, not all of which are independently motivated.  Moreover, even with all of these assumptions there still remain several similar-but-distinct rules (including some not discussed by Rubach 1996) and the usual lexical exceptions.

In a nutshell, Rubach's (1996) proposal is as follows.  The indicated vowels in words like *divinIty* and *tabUlar* are floating outside of syllable structure, meaning these words actually have forms like *divin-ty* and *tab-lar* from the point of view of closed-syllable shortening.  This suggestion is inspired by Yip (1987), who proposed that the indicated vowel in *divinIty* is underlyingly unspecified at the point when closed-syllable shortening applies, and is later filled in by default.  Rubach (1996) rejects the underspecification part of the analysis due to evidence that English only has one "default" vowel, and it is schwa, not /i/.  His use of floating vowels rather than entirely unspecified ones then allows him to analyze words like *tabUlar* the same way.  An alternative analysis by S. Myers (1987) involves resyllabification; note that the shortened vowels in *divínity* and *tábular* are both in stressed syllables before unstressed syllables, precisely the conditions under which Kahn (1976) and Borowsky (1986) argue that onsets are resyllabified as codas.  Rubach (1996) rejects this in the face of numerous arguments that true resyllabification only happens outside the class 1 morphology involved in these shortening processes, as well as comparisons such as that between *tone-tonic* (shortening) versus *tone-tonal* (no shortening), which imply that the crucial factor is the vowel of the suffix, not the stress pattern.  In short, even if one is skeptical of Rubach's floating vowels, there don't seem to be any other options to choose from.

To make Rubach's analysis handle all of the cases in (21), however, several other additional assumptions are necessary.  First, word-final consonants are assumed to be extraprosodic, meaning that syllables are only truly closed if preceding a cluster, as in *slept* or *description*.  This appears to be independently motivated from stress and other segmental rules (see e.g. Hayes 1982, Borowsky 1986).

Similarly, Rubach (1996) also needs to assume that if two coronal consonants appear at the end of a word, both are extraprosodic; this allows for an explanation of alternations like *child-children* and *pronounce-pronunciation*, since otherwise the vowels would be short in the nonsuffixed forms as well. This assumption is claimed to be independently motivated, but it then requires an additional (unmotivated) assumption to explain the vowel alternation in *meet-met*, since *met* is supposedly derived from /me:t+t/, which ends in a coronal cluster but still shortens the vowel. Moreover, it also negates a possible explanation for the lack of the classical alternation in pairs like *find-found*, which otherwise could be claimed to be exceptions to shortening (we will shortly see another way in which pairs like *find-found* are theoretically important).

The floating vowels also get Rubach into some trouble. First, he must assume that in pairs like *grade-gradUal*, which also involve a floating vowel as indicated, some property of the floating vowel prevents the stem-final consonant from being resyllabified as an onset; hence at the stage when shortening applies, *gradual* is syllabified *grad.u.al*, not *gra.du.al*, which would be expected if the /u/ were an ordinary vowel. Rubach (1996) accounts for this special property of floating vowels by exploiting the No Gap constraint (e.g. Archangeli and Pulleyblank 1994), which hitherto had only been used to prevent feature spreading from jumping across a valid target, and is otherwise entirely unmotivated in English.

Second, Rubach must assume that some suffixes that begin with /i/, e.g. *-ion* and *-ian*, do not contain floating vowels. Why most of them do (e.g. *-ic*, *-id*, *-ity*, *-ify*, *-ish*, etc), and only a few don't, rather than the other way around, is unclear. Moreover, the only way one can tell whether a given suffix has a floating vowel is whether or not it triggers the closed-syllable shortening rule.

Most seriously of all, there is a fatal empirical problem with any attempt to reduce all of the cases in (21) to a sort of cluster shortening, since true cluster shortening is sensitive to vowel quality. This can be seen by examining stems that contain surface [ey]. While they do shorten before clusters in the associated suffixed forms, the vowel quality does not change in the expected way, becoming [ɛ] rather than [æ].

(22)    abstain-abstention, detain-detention

Rubach (1996), following S. Myers (1987), includes such pairs as positive examples of cluster-induced shortening, ascribing the strange vowel quality change to lexical exceptionality to the vowel shift rule alone. However, this misses the generalization that cluster shortening alternations never involve [ey]; there are pairs like *thief-theft, wide-width*, but nothing like *\*cave-caft*. This gap is also found in the semi-weak verbs, as we already saw. The closest we got was *say-said*, which as in *abstain-abstention*, shortens the vowel and yet changes vowel quality the "wrong" way. This vowel quality gap is not found with the other forms of shortening that Rubach discusses (e.g. *cave-cavity*). Hence we must reject the most basic assumption of Rubach's analysis, namely that cluster shortening is the same process as the other cases he mentions. Instead, we need at least two separate rules: cluster shortening only affects [ay]-[ɪ]

and [i:]-[ɛ], while the other shortening processes (however they may be formalized) affect these, plus [ey]-[æ] as well.

If our goal is to describe all vowel alternations involving vowel shift, however, we cannot rely on these two rules alone. Another necessary rule, called i-shortening (Rubach 1984, 1996; Halle and Mohanan 1985) is illustrated by pairs like those below. Chomsky and Halle (1968) believed that such pairs could be handled the same way as the above cases, since the triggering suffix always begins with /i/, but as Rubach (1984) first showed, this is incorrect. First, the shortening process here behaves differently with respect to a rule that voices /s/ (to be discussed later). While a stem-final /s/ is not voiced in derived forms involving the shortening suffixes discussed so far, as in the examples in (a), it is voiced when the suffix is *-ion* or *-ian*, as shown in (b). Regardless of how this is analyzed (e.g. with rule ordering), the two shortening processes must be distinct.

(23)    a.    verbose-verbosity
        b.    precise-precision, precise-precisian

Second, with suffixes like *-ion*, *-ian* and others, the only vowel quality alternation that is consistently found is [ay]-[ɪ], as shown in (a) below. The surface stem vowel [ey] never alternates at all, as in (b), while the surface vowel [i:] only alternates sometimes, as in (c)-(d).

(24)    a.    decide-decision, precise-precision, revise-revision,
              ignite-ignition, recognize-recognition, Cyprus-Cypriot,
              hide-hideous, reptile-reptilian, Christ-Christian

        b.    evade-evasion, relate-relation

        c.    discreet-discretion, succeed-succession

        d.    delete-deletion, cohesive-cohesion

As Rubach (1996) points out, the alternations in (c) can be handled by normal cluster shortening if one assumes that here the suffix is actually *-tion* rather than *-ion*, which would also explain why the stem-final consonant becomes voiceless in pairs like *succeed-succession*; in (d) the suffix is truly *-ion*. (Of course this doesn't explain why the *-tion* allomorph is never used when the surface stem vowel is [ey] or [ay].)

The alternations in (a) therefore require a different process, called i-shortening because it only affects the [ay]-[ɪ] vowel alternation. The conditioning environment seems to be a suffix starting with the sequence /iV/.

This is not to be confused with another independent process, often called CiV-lengthening, which is necessary to handle alternations where, in direct contradiction of the above rule, a vowel is *lengthened* before suffix beginning with /iV/. That these alternations involve lengthening rather than shortening is shown not only by the mix of environments where the short

alternants appear, but also by the stress pattern; if the second vowel in a word like *Canada* were underlyingly long, it would attract stress. There are the usual technical difficulties with the formalism of CiV-lengthening; e.g. see Rubach (1996:215, 232fn22, the latter observing that something must be done to explain why the suffix *-ion* never triggers CiV-lengthening).

(25)  a.    comedy-comedian, college-collegian
      b.    Canada-Canadian, regal-regalia, mendacity-mendacious
      c.    Jefferson-Jeffersonian
      d.    Malthus-Malthusian

Notice that all of the examples above target all vowels except /i/, exactly the opposite of i-shortening. As illustrated below, when a high front vowel appears in the appropriate place, suffixes like *-ian* do not consistently lengthen it (a); one case where lengthening does occur is in (b). Rubach (1996) correctly notes that allowing CiV-lengthening to affect all vowels except /i/ is dissatisfying, but allowing it to handle cases like (b) as he does just turns words like those in (a) into something like lexical exceptions.

(26)  a.    Darwin-Darwinian, family-familiar, reptile-reptilian
      b.    Paris-Parisian [some dialects]

Actually, Rubach's handling of words like the above requires something far worse than lexical exceptions. The existence of overlapping vowel-shortening and vowel-lengthening processes allows for the possibility that both could apply in a single word, and Rubach (1996) claims that just such a possiblity occurs. The short [ɪ] in derived forms like those in (a) above, Rubach claims, are due to a derivation whereby the underlying vowel is first lengthened by CiV-lengthening (if not already long) and then shortened again by closed-syllable shortening.

Evidence for this disturbing analysis comes from British Received Pronunciation, where in *Parisian*, the "s" is voiced as in all dialects, but the second vowel is short. Since the stem-final /s/ is voiceless, it must have been voiced by a rule, which, as we will see later, is said to apply only after long vowels, which in turn implies that at some point in the derivation the second vowel must have been long. The result is the following series of steps. (Of course in dialects where the second vowel surfaces as long, this word now becomes a lexical exception to i-shortening.)

(27)  ["Parisian" derivation after (33) in Rubach 1996:217]

|                   | "Parisian" | "Darwinian" |
|-------------------|-----------|-------------|
| UR                | pærisiæn  | darwiniæn   |
| CiV-lengthening   | pæri:siæn | darwi:niæn  |
| s-voicing         | pæri:ziæn | --          |
| i-shortening      | pæriziæn  | darwiniæn   |

Not only do these derivations involve opaque rule interaction, but are in fact instances of the so-called Duke-of-York gambit, where one rule undoes the effect of an earlier one (Pullum

1976). This kind of interaction stumps even sympathy theory, the extension of OT that can handle many kinds of opaque interactions (McCarthy 1998). Rubach (1996) himself rejects an analysis elsewhere in his paper because it would require the Duke-of-York gambit (p. 223). Yet as Rubach points out, this seems to be the only way to handle the pronunciation of *Parisian* without allowing many lexical exceptions or barring CiV-lengthening from applying to high front vowels. (Note that the argument relies on the assumption that s-voicing is a reliable diagnostic of anything; in reality, as I will show later, s-voicing pattern is probably the least rule-like pattern in all of the "standard lexical phonology" canon.)

If Rubach's attempts to generalize CiV-lengthening to /i/ are rejected, we are left with two rules that are almost exactly complementary: i-shortening shortens /i/ before suffixes like *-ian*, while CiV-lengthening lengthens all other vowels in the same context. This correlation with vowel quality does not seem to be entirely random. In particular, as first pointed out to me by Mike Hammond, there is a natural phonetic correlation between vowel height and vowel duration, with lower vowels being longer than higher vowels. In fact, such factors actually seem to play an active role in some lexical stress patterns in Finnish (Anttila 1997). One may thus be tempted to say that i-shortening and CiV-lengthening are actually just two aspects of a single process that tries to align vowel quality and vowel duration in a maximally "harmonic" fashion. For example, by making use of the concept of harmonic alignment (Prince and Smolensky 1993), we might use the following universal rankings:

(28)  a.  *high/long >> *mid/long >> *low/long
      b.  *low/short >> *mid/short >> *high/short

The problems with this analysis become clear as soon as we actually try it out, as I do in the following tableau for the word *reptilian*. The analysis correctly rules out the candidate where the long [i:] surfaces, but it can't eliminate a vowel-shifted [ay], which is not really a high vowel. Since the relation between shortening and vowel shift is opaque (counterbleeding), a complete OT analysis would need to use something unpleasant like sympathy theory (McCarthy 1998). Moreover, the analysis makes no use of the suffix *-ian*, falsely predicting shortening or lengthening in unsuffixed words as well. In fact, since the conditions for shortening and lengthening are identical other than for the vowel quality, it's difficult to see how the form of the suffix could be made to play any "natural" role. I therefore conclude that the temptation to unify i-shortening and CiV-lengthening must be resisted. I will have more to say about this frustrating conclusion later.

(29)

| rept/i:/lian | *high/long | *low/short | *low/long | *high/short |
|---|---|---|---|---|
| rept[i:]lian | * | | | |
| rept[ay]lian | | | | |
| rept[ɪ]lian | | | | * |

But I am still not done cataloging processes affecting vowel duration in English lexical phonology. Another is found in some syllables before a stress. The rule of <u>prestress destressing</u> (e.g. Liberman and Prince 1977) removes stress on the prestress syllable and this leads to its being shortened (with the details depending on who's telling the story; cf. Jensen 1993 vs. Rubach 1996). In any story, the shortening cannot be due to closed-syllable shortening, since the vowel targeted by prestress destressing ends up in an open syllable in the derived form, and since the following vowel is stressed, it isn't plausible to say that underspecification, floating vowels, or resyllabification are involved. Moreover, in spite of the fact that the shortening occurs in the antepenultimate syllable, it cannot be handled by Chomsky and Halle's (1968) original rule of trisyllabic laxing (shortening), since they specifically forbid this rule from applying when the penultimate syllable is stressed.

(30)    a.      divine-divination
         b.      reveal-revelation, cease-cessation
         c.      defame-defamation, migrate-migratory
         d.      provoke-provocation

Yet another process affecting vowel length in English lexical phonology is not discussed in Rubach (1996), but it is in many other places, including Chomsky and Halle (1968), Halle and Mohanan (1985), and Borowsky (1986). This is the apparent compensatory lengthening of /i/ when /g/ is deleted before a tautosyllabic nasal, as illustrated in the following alternations, which I'll call <u>g-lengthening</u>. As can be seen by comparing (a) with (b) below, it only affects the [ay]-[ɪ] alternation.

(31)    a.      malign-malignant, sign-signal, paradigm-paradigmatic
         b.      phlegm-phlegmatic, diaphragm-diaphragmatic

The above six processes, namely cluster shortening, closed-syllable shortening, i-shortening, CiV-lengthening, and g-lengthening, are not the only ones affecting vowel duration in English lexical phonology; they are just the ones that have received names in the literature. Here I briefly describe a few other unnamed processes.

First consider the alternations shown below. Here shortening occurs in the antepenultimate syllable even though the following vowel is already found in the stem, which means that Rubach (1996) is unable to account for them with any of his rules. Yip's (1987) underspecification analysis was unable to handle these cases, and Rubach simply follows her comment that this pattern is "unproductive," which is somewhat disingenuous given that this phonological alternation involves precisely the same vowels found in the alternations that their analyses can handle, including irregular inflection and "pairs" like *hide-hideous*. Intriguingly, in the otherwise discredited trisyllabic shortening rule of Chomsky and Halle (1968), these alternations are expected, since shortening occurs in antepenultimate position before an unstressed syllable. Chomsky and Halle's original rule is hard to understand as "natural,"

however, and since the stress doesn't shift, even S. Myers's (1987) resyllabification analysis can only handle cases like these with special pleading.

(32)  nature-natural, nation-national, femur-femoral, semen-seminal

Another pattern not worthy of a name is seen in the following pairs.  Again, the vowels are shortened in antepenultimate syllables, which may seem systematic, but it happens in spite of the fact that the vowel contexts are the same in stem and derived forms.  Moreover, the vowel quality alternations ([ɪ]~[ɛ] in British Received Pronunciation) are not what is expected from the normal vowel shift pattern.  Rubach (1996) dismisses these as allomorphy.

(33)  relate-relative, preside-president, revere-reverend

Still other pairs show alternations (or non-alternations) that are unexpected by one theory or another, with nobody coming out a clear winner according to Rubach, such as the following.

(34)  a.    omen-ominous, beast-bestial
      b.    nasal-nasalize, velar-velarize

Finally, there are of course also lexical exceptions, plain and simple, such as the following, where shortening does not occur before suffixes that normally trigger shortening.  (Other lexical exceptions will be discussed more fully later.)

(35)  base-basic, note-notify, scene-scenic, obese-obesity

To summarize so far, if possible, the "best" analysis of vowel alternations in English requires many shakily motivated assumptions of the type Rubach (1996) and previous researchers have made, plus at least six independent processes:  cluster shortening, closed-syllable shortening, i-shortening, CiV-lengthening, prestress shortening, and g-lengthening.  Of course, these are just the vowel alternations involving the canonical vowel-shift patterns; still other explanations are necessary for alternations like *nature-natural*, *abstain-abstention*, *relate-relative*, and so forth.  In addition, all of these processes have unexplainable lexical exceptions.

In this section my method has been deconstructive, reminding the reader just how idiosyncratic the English vowel alternations really are.  I now turn to positive arguments, showing why these alternations are better understood as built from the ground up, from pairwise matching of the lexical items themselves.

## 2.4.2 Family resemblances and alternations

My first positive argument is that accepting analogy into our analysis means that we can openly acknowledge the similarity of the six distinct processes, rather than merely footnoting this fact in frustration since they can't be collapsed into a single general rule.  That is, the words

pairs involved in these alternations show family resemblances with each other, thereby forming a coherent class.

Indeed, the existence of these family resemblances is what allowed S. Myers (1987), Yip (1987) and Rubach (1996) to simplify the original Chomsky and Halle (1968) analyses to the extent that they did. In Chomsky and Halle (1968), what Rubach (1996) tries to analyze with a single process of closed-syllable shortening was ascribed to three distinct rules, one applying before surface clusters (e.g. *sleep-slept*), one applying in antepenultimate syllables (e.g. *serene-serenity*), and one applying before specific monosyllabic suffixes like *-ish* (e.g. *Spain-Spanish*). As I showed in the previous section, the second two generalizations can be collapsed into one using any of a number of powerful technical devices unavailable in 1968 (e.g. resyllabification, underspecification, or floating vowels), and although the first generalization cannot in fact be collapsed with the others, it is easy to be fooled into thinking that it can, as S. Myers (1987), Yip (1987) and Rubach (1996) were. In short, these three processes appear very similar. It is of course conceivable that the future will bring some dramatic new theoretical breakthrough that will allow phonologists to collapse the remaining five patterns as well, but I prefer to draw a different lesson: lexical phonology is in its very substance a redundantly overlapping set of similar but not identical patterns.

In fact, the six patterns that have actually been given names in the literature are probably just the tip of the iceberg. Recall for example trisyllabic shortening. As observed in the previous section, this abandoned rule of Chomsky and Halle (1968) continues to live on as a sort of ghost, describing several alternation classes that stymie the more sophisticated analyses of S. Myers (1987), Yip (1987) and Rubach (1996). That is, there does seem to be a schema that coincides roughly with Chomsky and Halle's original rule, but not only does it not behave properly enough to deserve the status of a real rule, but it also redundantly overlaps with other patterns. The curious reader is encouraged to study the footnotes in Chomsky and Halle (1968) and other surveys of English phonology for other semiregular alternation patterns that never managed to reach official status in the literature.

Focusing now just on the six processes recognized in current work, a particularly helpful way to see their similarities, as well as their frustrating differences, is to arrange them as in the following figure, which shows the front vowels affected by the various processes (back vowels are ignored, since they are even messier). The check marks indicate whether a given process applies with the given alternation.

(36)     [after a figure in J. Myers 1993]

|  | /ay/-/ɪ/ | /iː/-/ɛ/ | /ey/-/æ/ |
|---|---|---|---|
| i-shortening | √ |  |  |
| Cluster shortening | √ | √ |  |
| Other cases of "closed-syllable shortening" | √ | √ | √ |
| prestress shortening | √ | √ | √ |
| CiV-lengthening |  | √ | √ |
| g-lengthening | √ |  |  |

As we have seen, one major reason we cannot ascribe these six patterns to a single process is that they affect different vowels (closed-syllable shortening and prestress shortening cannot be collapsed since the latter occurs in undeniably open syllables). Even the complementary i-shortening and CiV-lengthening cannot be collapsed. Recall that this conclusion was drawn in spite of the existence of a phonetic motivation for preferential shortening in higher vowels. A further argument against a direct role of such phonetic motivations in these lexical vowel alternations is provided by g-lengthening, which directly contradicts this "natural" tendency. Even the alternations that conform to the height-duration correlation differ in unpredictable ways as to which specific vowels are affected.

However, although we cannot collapse the six patterns into one, we can express their similarities through family resemblances. I illustrate this in my now standard fashion, as a list of pairs where each pair differs minimally from the preceding one as indicated by the given phonological properties of the rightmost word of the pair. Again, the actual pairings wouldn't be linear like this, and would involve all of the relevant words in the lexicon. Notice that to show how CiV-lengthening is related to the other patterns, I reversed the order of the pair to derived-base. No reversal was necessary for the g-lengthening pair since here the short vowel appears in the morphologically derived form, as in all of the shortening patterns. These two observations suggest that the patterns are actually nondirectional.

(37)  a.  _____ gNσσ  design-designation
         [+high]

      b.  _____ C[Cor]σ  describe-description
         [+high]

      c.  _____ [Cor]σ  contrite-contrition
         [+high]

      d.  _____ [Cor]σ  discreet-discretion
         [-hi,-lo]

      e.  _____ Cσσ  serene-serenity
         [+stress]      divine-divinity

      f.  _____ Cσσ  divine-divination
         [-stress]      defame-defamation

      g.  _____ C]  regalia-regal
         [-stress]

I make no claims that such a list explains anything by itself. The primary point is to show that the six patterns are similar to each other in essentially the same way as irregular verb stems in an ablaut class. It is important to recognize that it didn't have to be this way. It is completely conceivable, given the devices available in both derivational theories and in OT, that English could have had a single process that affects vowel duration the same way across the entire lexicon, with the vowel shift alternations behaving properly in every single word. The fact that this is not truly the case is a surprise given the formalisms currently available, even though it is not a surprise given our intuitions as phonologists: lexical phonology, as we all know, doesn't behave so nicely. Repairing this enormous mismatch between what our theories and our scholarly instincts tell us is one of the major goals of this paper.

Consideration of another alternative English provides further insights into what the theory of lexical phonology should be like. Imagine an English with six processes all involving the classical vowel-shift alternations, and yet which are otherwise as completely unlike each other as is possible. The fact that English is not like this either is also unsurprising, but the usual explanation is that variation in rules is limited by universal principles, which in OT terms are the universal markedness constraints. Yet if these rules are truly lexical, meaning they will have lexical exceptions, idiosyncratic behavior and the rest, markedness considerations will remain as frustratingly useless as they are in the case of the six actual patterns I've been discussing. All may be unmarked (by hypothesis), but they will still be uncollapsible (by hypothesis). Moreover, since they are so different, an analysis that depends on pairwise comparisons would not work as well as it actually does in real English. The conclusion I draw from this little thought experiment is that markedness factors alone may be insufficient to account for similarity across distinct lexical patterns (though as already noted, the issue of markedness requires further discussion, which will come later). Analogical enforcement of similarity between words may serve to create just the kind of similar-but-distinct families of patterns that are actually found in real English.

### 2.4.3  Family resemblances and exceptions

The second positive argument I wish to make here is that family resemblances also play a role in defining classes of words that fail to take part in expected vowel alternations. I discuss two pieces of evidence for this.

The first concerns some exceptions to CiV-lengthening listed below, mentioned in a footnote in Rubach (1996).

(38)    Italian, valiant, battalion, Maxwellian

Following Chomsky and Halle (1968), Rubach (1996) notes that vowels before [l] are "typically" short. He connects this with observations in Rubach (1984) that a post-vowel-shift shortening process is triggered by [ɾ] for some American speakers, as in the following pair.

(39)    grammar-gramm[ɛ]rian          {cf. gramm[e:]rian in other dialects}

The analysis that Rubach (1996) seems to be hinting at is another Duke-of-York gambit, where CiV-lengthening first lengthens these vowels, and then some pre-liquid shortening process shortens them again. Moreover, since the vowel qualities are different in short vowels before [l] and [r], we would need to split pre-liquid shortening into two parts. The result would be rather questionable derivations like the following.

| (40) | "Italian" | "grammarian" |
|---|---|---|
| UR | It/æ/lian | gramm/æ/rian |
| CiV lengthening | It/æ:/lian | gramm/æ:/rian |
| l-shortening | It/æ/lian | -- |
| Vowel shift | -- | gramm/e:/rian |
| r-shortening | -- | gramm/e/rian |

Compared to this, an analogical approach seems preferable: stems with vowels before [l] are involved in their own schema that happens to override the schema named CiV-lengthening. It is impossible to say for sure why the [l] schema overrides the lengthening schema anymore than we can say why the analogy in irregular inflection in Glenoe Scots overrides the general Scottish Vowel Length Rule. All we can say is that if it didn't, we would never have noticed it. The fact that it can, however, is expected in an analogical approach.

An even more dramatic example is found in another footnote in Rubach (1996). Following Chomsky and Halle (1968) and Kiparsky (1982), Rubach (1996) argues that what he analyzes as closed-syllable shortening also applies in monomorphemic words, and in fact his own analysis provides an explanation for famous exceptions like *nightingale*: the antepenultimate long vowel is in an open syllable. Of course, he still must give a list of truly exceptional words like the following, where long vowels appear in closed syllables. My reasons for listing them in this peculiar way will be made clear in a moment.

(41)  a.  ancient
      b.  angel, danger
      c.  chamber, cambric, Cambridge
      d.  maintain, dainty, fountain, mountain
      e.  council, wainscot
      f.     peascot
      g.  boulder, shoulder
      h.  deictic, deixis, seismic

Citing Borowsky (1986), Rubach (1996) notes that most of these exceptions involve homorganic nasal/liquid-obstruent clusters, implying that they could perhaps be handled by a rule that counteracts closed-syllable shortening. In a standard approach to lexical phonology, however, "most" is not good enough: no rule can possibly suffice to undo the effects of shortening in all and only these words. A putative rule would also have the curious property of applying only in monomorphemic words, something judged impossible in all theories of generative phonology (but see the next section for another example).

It seems instead that these exceptions form a class by virtue of family resemblances. I've indicated this in my usual way above, within the artificial limitation of a linear presentation (so words on lines (f) and (g) are both similar to words on line (e)). It is true that the words on line (h) aren't very similar to the ones above them, but surely it's significant that they're similar to each other.

What is particularly interesting about this list of exceptions is that there is an entire additional morphological class like them, a point apparently missed by Borowsky (1986) and Rubach (1996). This is the class of irregular verbs ending in [nd], which I discussed earlier and repeat below.

(42)     bind-bound, find-found, grind-ground, wind-wound

These verbs contain a nasal-obstruent cluster and also fail to undergo vowel shift in the past tense. Verbs like these have a good excuse for not undergoing these processes: they're irregular, and can do whatever they like. The monomorphemic words and these irregular verbs thus mutually support each other in their resistence to the general cluster-shortening schema.

Thus just like the Scottish Vowel Length Rule, closed-syllable shortening is blocked precisely where a competing analogical pattern is stronger than it within a portion of the lexicon. It is therefore difficult to understand how shortening could itself be anything other than analogy.

### 2.4.4 Vowel alternations: summary

In this section I have reviewed some of the various vowel alternations in English lexical phonology, and have demonstrated some important properties that reveal their true nature as analogy. First, they are far more idiosyncratic than one would expect given the devices available in current formalism. Second, they are less phonetically "natural" than they have been made out in the literature since S. Myers (1987). Third, they are more similar to each other than would be expected if they truly had entirely distinct ontologies in the grammar. Fourth, classes of words that represent exceptions to the patterns are also defined by natural classes. Finally, the vowel alternations in derivational morphology parallel alternations in irregular inflection, thus forming analogies like *hide:hid::hide:hideous*, *say:said::abstain:abstention* and *found::council*. If the vowel alternations in irregular inflection must be handled by analogy, therefore, then so must they all.

### 2.5     Consonantal alternations

Since I have finished with the vowels, all that's left in English phonology are the consonants and stress. Obviously I can't discuss all of this in this already overlong paper. Hence I will take a far less thorough approach in the following discussion, limiting myself primarily to brief observations on just two processes, s-voicing and velar softening. Again, it is not difficult to show that they show all the expected properties of analogy.

## 2.5.1   Analogy in s-voicing

The pattern of s-voicing (discussed, among other places, in Chomsky and Halle 1968, Rubach 1984, Halle and Mohanan 1985, and Borowsky 1986) is standardly described as holding in the context indicated in (a) below, with standard illustrative examples given in (b). (Some of the derived forms also show palatalization which occurs before /i/ or /y/ or whatever it is that the suffixes *-ion* and *-ian* begin with.)

(43)   a.   v: __ v

   b.   Paris-Parisian
        recluse-reclusion
        sign-design
        assist-resist

The reality, as usual, is far more complicated than this simple description would imply (see e.g. J. Myers 1993, 1994 for earlier deconstructions). There are actually several other types of word pairs in English that involve alternations between [s] and [z]. As with the vowel alternation processes, they cannot all be described with a single rule, and yet the conditioning environments show striking family resemblances. These other environments where [z] appears, each with some examples, are listed below in an order that shows how similar they are when compared in a pairwise fashion. To emphasize these similarities, I've included the standard examples again at the top.

(44)   a.   v: __ v

        Parisian       [cf.   Paris]
        reclusion      [cf.   recluse]
        design         [cf.   sign, consign]

   b.   v __ v́

        dissólve       [cf.   sólve, dìssolútion]

   c.   vk __ v́

        exíst          [cf.   áxis]
        exámine        [cf.   éxecùte]

d.    r __ v

dispersion    [cf.    disperse]
coercion      [cf.    coerce] [/s/ in some dialects]
persist       [cf.    insist]  [/s/ in some dialects]

e.    r __ ]

Mars          [cf.    Martian]

Observe first of all, that like the vowel alternation patterns, these patterns are not only similar yet distinct, but also appear to be vaguely phonetically natural:  fricatives often voice intervocalically or next to sonorant consonants.  Again, of course, this observation gets us nowhere; these patterns have too many unique properties to follow automatically from general markedness constraints.

Instead, these patterns display properties expected of analogy.  Note first the pattern in (44c).  This is taken directly from Chomsky and Halle (1968:228-9), who make the interesting observation that it only holds of monomorphemic words.  That is, orthographic "x" is not voiced, even when in the proper phonological environment, if it is separated from the following vowel by a morpheme boundary.  Here are the examples they give.  (The word *éxit* must be counted as forming its own class.)

(45)   a.    exíst, exámine, auxílliary, exásperate
       b.    hexámeter, toxícity, annéxation

In other words, pattern (44c) is a morpheme structure constraint (MSC) rather than a rule (I'll mention such constraints again later in the paper).  Since Kiparsky (1982), phonologists have not been afraid to assume that MSCs are describable with the same devices that handle alternations; for Kiparsky (1982), these were (noncyclic) rules, while in OT everything is constraints.  In neither approach can we understand how MSCs could be blocked from applying in morphologically complex words as well.  By contrast, in an analogical approach that also includes reference to morphological productivity, the mystery is solved.  True, the morphology involved in (45b) is hardly very productive, but the fact that there is morphology at all means that it is easier to separate the /ks/ from the following vowel in (45b) than in (45a).  If the analogy that gave rise to s-voicing in words like *exist* is weak enough, it could fail to apply in words like *hexameter* for the same reason that "standard" s-voicing applies with class 1 morphology (e.g. *Parisian*) but not with more productive class 2 morphology (e.g. *policing*), the reason being that analogy has less of an effect on forms that can be derived outside of the lexicon.

Another diagnostic of analogy, nondirectionality, is shown in pattern (44e).  Note that the "s" in *Mars* is voiced after [r], just as in words like *dispersion*, except here the voicing occurs in the nonderived form.  An analysis of this that appeals to allomorphy would ignore the phonological relevance of this [r].  The [z]-[s] alternation in *Mars-Martian* also cannot be handled

by a devoicing rule, since the suffix *-ian* doesn't have this effect in words like *Parisian*, even in Received Pronunciation. Like the almost-mirror-image lengthening and shortening patterns in vowel alternations, the s-voicing in *Mars* implies that phonological generalizations actually don't apply in particular directions.

A different kind of evidence for family resemblances and nondirectionality in s-voicing is illustrated by pairs like the following.

(46)   a.      relate-relation
       b.      evade-evasion
       c.      equate-equation

The pairs in (46a) and (46b) show the typical pattern:  if the stem contains a long vowel, the form derived through *-ion* suffixation will maintain the voicing of the stem-final consonant. The change from stop to fricative cannot be handled by s-voicing, of course; this is the responsibility of another process, often called spirantization (e.g. Chomsky and Halle 1968, Rubach 1984, Halle and Mohanan 1985, Borowsky 1986). In these analyses, standard s-voicing is prevented from applying in pairs like (46a) because s-voicing is extrinsically (and opaquely) ordered before spirantization, at which point the /t/ is not yet an /s/.

Of interest here is the pair in (46c), which has a property in the derived form (voicing) that cannot be predicted from the base form. Our only choices are to view this as a sort of "anti-exception" (it undergoes s-voicing when it shouldn't), as a case of rule-reordering from opaque to transparent that is caught in mid-lexical-diffusion (see other examples in Kiparsky 1978, Robinson 1976, 1977 and references therein), or as a case of analogy with pairs like *evade-evasion*. Which of these three approaches best accounts for the data?

The answer is analogy. The other two explanations don't require that pairs like *evade-evasion* exist, but the analogical explanation depends on them. In fact, its predictions are still more specific:  the *evade-evasion*-type pattern should show a family resemblance with the standard s-voicing pattern, and this family resemblance should be at least as strong as, if not stronger than, with pairs like *relate-relation*. Both of these predictions are confirmed by the evidence. First, words ending in *-asion* appear both in spirantization alternations (e.g. *evade-evasion*, *include-inclusion*), and in s-voicing alternations (e.g. *precise-precision*, *recluse-reclusion*). The pair *equate-equation* thus stands in the middle of these two patterns:  it is like *evade-evasion* in ending in [-continuant] in the base form, and like *precise-precision* in ending in [-voice] in the base form. Second, the *evade-evasion* pattern is stronger than the *relate-relation* pattern in the sense that words ending in *-asion* always involve the suffix *-ion* affixed to a stem ending in a coronal obstruent, whereas words ending in *-ation* very often involve the allomorph *-ation* (e.g. *reveal-revelation*; see Aronoff 1976 for fuller discussion of the *-ion* allomorphs). This makes pairs like *relate-relation* less typical than pairs like *evade-evasion*; *equate-equation* becomes somewhat more typical (i.e. "optimal" within a specific lexicon) by voicing in the suffixed form. This analysis will be formalized later.

The final point I will make about the analogical nature of s-voicing also concerns family resemblances and nondirectionality. I have just noted that words ending in *-sion* can be derived in two ways according to the official rules:  through spirantization or through s-voicing. However,

there are also alternations where *-sion* cannot be derived by any standard rule at all, as illustrated below. Aronoff (1976) describes these as *-ion* allomorphy, but interestingly, in all of these cases the base form contains [r] in the final syllable coda. The derived forms are thus describable with the same schemas listed above, except that this time the /z/ (palatalized by another process) cannot actually be derived from /s/. Hence the derived forms not only contain something unpredictable from the base forms, but in accordance with analogy, they are similar to other derived forms.

(47)   a.      Alternates with [t] in the context r __ v (pattern (44d))

            diversion          [cf.    divert]

      b.      Alternates with [dʒ] in the context r __ v (pattern (44d))

            submersion        [cf.    submerge]

      c.      Alternates with [r] in the context v: __ v (pattern (44a))

            cohesion          [cf.    cohere]

In short, the evidence that s-voicing is analogy is so overwhelming, that there really isn't anything else to say about it.

## 2.5.2   Analogy in velar softening

The final pattern of English lexical phonology I will consider is velar softening (e.g. Chomsky and Halle 1968, Rubach 1984, Halle and Mohanan 1985, Borowsky 1986). Once again, its behavior is indicative of analogy.

Velar softening describes the consonantal alternations illustrated in pairs like those below, where stem-final [k] and [g] alternate with [s] and [dʒ], respectively, before /i/ and /e/.

(48)   a.      critic-criticism, matrix-matrices
      b.      analogue-analogy

Crucially, in the standard analysis, the vowel triggers /i/ and /e/ are in the form they have before vowel shift has applied, thus accounting for "overapplications" and "underapplications" like those shown below. These opaque interactions with vowel shift can be handled in a derivational theory as shown in (c).

(49)   a.      criticize      [velar softening occurs before surface [a]]
      b.      medicate     [velar softening fails to occur before surface [e]]

|  | "criticize" | "medicate" |
|---|---|---|
| c. | | |
| UR | /ki:/ | /kæ:/ |
| velar softening | /si:/ | -- |
| vowel shift | /say/ | /key/ |

Velar softening is also standardly argued to apply in some morphemes that do not alternate, namely *-cess*, *-cept*, *-gest*, and the like. There are two arguments. The first is that positing an underlying /k/ or /g/ in these cases explains why [k] or [g] sometimes show up on the surface.

(50)  a.  accept vs. assist, success vs. suspect
      b.  suggest

The second argument is that the alveolar fricative in these morphemes is never voiced as in (a) below, thus creating surface violations of s-voicing. This can be handled by ordering s-voicing before velar softening, as illustrated by the derivations in (b).

(51)  a.  recite, recede, receive, recess

|  | "recede" | "resent" |
|---|---|---|
| b. | | |
| UR | /k/ | /s/ |
| s-voicing | -- | /z/ |
| velar softening | /s/ | -- |

Independent evidence of this putative ordering comes from words like the following, which again show the results of velar softening but not s-voicing.

(52)  opaque-opacity, Greek-Grecian

Such then for the empirical motivations for velar softening. How should it best be analyzed? We have already seen how cumbersome the formalism for lexical patterns can be, and velar softening takes this to a new height. Not only is it difficult to express the *k~s* and *g~ dʒ* alternations with a single rule, but the *k~s* alternation involves two featural changes at the same time (place and continuance), a problem enough for Chomsky and Halle (1968), but even more serious for autosegmental analyses. As with most lexical rules, there are hints of phonetic naturalness floating just out of reach of the formalism; thus /i/ and /e/ are front vowels, therefore "coronal" in some feature geometry proposals, and intervocalic spirantization is quite common. The specific changes that occur in velar softening, cannot be expressed in a natural way; hence most authors, e.g. Kiparsky (1982) and Halle and Mohanan (1985) don't even bother trying to formalize the change, giving the rule simply as k→s. In addition, there are further problems with the formalization of velar softening involving the OCP; see J. Myers (1993) for some discussion.

Let us then consider an alternative analysis, where velar softening is due to a set of exemplar-driven analogies rather than a formal rule. One important piece of evidence can be seen either as nondirectionality or as family resemblances run wild: virtually all pairs showing the velar softening alternation involve just one morpheme, *-ic*. Of the twenty-three pairs showing *k~s* alternations given in the literature (i.e. in Chomsky and Halle 1968:168, 219, 230; Rubach 1984:26, 27; Halle and Mohanan 1985:79; Borowsky 1986:128, 129), only four do not involve this morpheme. A more thorough dictionary search (J. Myers 1993) confirms this impression. The special affinity of velar softening for *-ic* is also illustrated by coinages like *witticism*, which treats *-icism* forms a single unit (Ohala 1986a).

The fact that velar softening has a special affinity for a single morpheme could perhaps be handled with allomorphic rules of the sort used by Aronoff (1976) or Booij (1997), but this would miss two facts. First, precisely the same alternations are found beyond this morpheme, though far less commonly. That is, parsimony demands that either velar softening is always allomorphy or else never is; since the first choice is out, we must choose the latter.

Second, even in words that do not seem to have the morpheme *-ic*, velar softening alternations are far more common if the base stem ends with the phoneme sequence [ɪk] than would be expected by chance. This second point is illustrated by the following figures which list all alternating pairs (found by J. Myers 1993 in a thorough dictionary search) whose base forms end in [k] but not the morpheme *-ic*, and whose derived forms end in the velar-softening-triggering suffixes *-ine, -i, -y, -ist, -ism, -ize, -ian, -ity* and *-es* (as in *matrices*). The examples are divided into two groups: those where the affected stem ends in the submorphemic phoneme sequence [ɪk], and all others. The proportion of stems in these lists that end in [ɪk] (42% = 22/54) is far higher than what one would expect by chance, given the variety of vowel phonemes that could have preceded the /k/ in these stems. Moreover, it is clear that the majority of the [ɪk]-final base forms all end in the same morpheme, *-ix*, and the majority of other forms end in *-x*, which of course already contains an [s].

(53)    Stem ends in the submorphemic phoneme sequence [ɪk]

        [k]-[s] alternations

        a.      colchicum-colchicine, duplicate-duplicity, lubricate-lubricity, medicate-medicine, mendicant-mendicity, vortical-vorticity

        b.      calix-calices, calyx-calyces, cervix-cervices, coadjutrix-coadjutrices, curatrix-curatrices, cyclix-cyclices, directrix-directrices, executrix-executrices, generatrix-generatrices, helix-helices, initiatrix-initiatrices, matrix-matrices, radix-radices, spadix-spadices, tractrix-tractrices, victrix-victrices

Stem does not end in the phoneme sequence [ɪk]

[k]-[s] alternations

c.     abacus-abaci/abacist, Australopithecus-Australopithecine, caducous-caducity, cercus-cerci, coccus-cocci, focus-foci, Greek-Grecism/Grecize, locus-loci, opaque-opacity, pharmacology-pharmacist/pharmacy, reciprocal-reciprocity, Spartacus-Spartacist

d.     apex-apices, borax-boraces, cimex-cimices, codex-codices, crux-cruces, fecal-feces, hallux-halluces, haruspex-haruspices, hyrax-hyraces, ibex-ibices, index-indices, latex-latices, lux-luces, pontifex-pontifices, scolex-scolices, vortex-vortices

[k]-[ʃ] alternations

e.     Greek-Grecian, Marcus-Marcian

Although J. Myers (1993) did not do a thorough search of the [g]-[dʒ] alternations, a similar point emerges from the examples found in the literature, listed in the following figure. Namely, far more alternations involve a preceding [ɪ] than would be expected by chance. Notice also that velar softening seems to be less robust without this preceding [ɪ], so that it's obligatory in *rigid* but optional in *fungi*. Moreover, in one pair, *intellect-intelligent*, this [ɪ] only appears in the derived form, a nondirectional effect typical of analogy.

(54)     Compiled from all examples in Chomsky and Halle (1968:168, 219, 230); Rubach (1984:26, 27); Halle and Mohanan (1985:79); Borowsky (1986:128, 129)

Velar softening with preceding [ɪ]

intellect-intelligent, larynx-larynges, prodigal-prodigy, rigor-rigid

Velar softening preceded by other vowels

analogue-analogy/analogous/analogize {also analo[g]ous}, dialog-dialogist {also dialo[g]ist}, fungus-fungi/fungicide {also fun[g]i, fun[g]icide}, pedagog-pedagogy, regal-regicide

Thus once again, close examination finds that a familiar rule of English lexical phonology is not only far more idiosyncratic than it is typically made out to be, but shows diagnostics indicative of analogy.

## 2.6     Pattern interactions in lexical phonology

One well-known property of lexical patterns is that they interact.  In particular, patterns like vowel shift, s-voicing and velar softening interact in a way that can most easily be described with rule ordering.  Many of the interactions of lexical patterns are opaque, and so are impossible to express in standard OT; sympathy theory (McCarthy 1998) can technically handle it, but no one can say that opaque interactions fit organically into OT.  Moreover, the "rule ordering" interactions in English lexical phonology appear to be extrinsic, that is, part of the language-particular grammar of English rather than following from instrinsic properties of the patterns themselves.  How can analogy, working as it does on a word-by-word basis, possibly capture these kinds of interactions?

The answer actually turns out to be quite simple, indeed perhaps too simple for some theorists.  Consider first the interaction between velar softening and vowel shift.  This interaction is both non-surface-true, since velar softening can underapply (e.g. *medicate*), and non-surface-apparent, since velar softening can overapply (e.g. *criticize*).  One obvious solution to this problem is to reformulate velar softening so it has precisely the behavior desired, e.g. stipulating that it can apply before the suffix *-ize* but not before the suffix *-ate*.  This kind of solution is explicitly rejected in the generative literature, however, going back at least to Halle (1962).  Given the goal of providing a maximally efficient grammar, extrinsically ordered derivations (or perhaps even the cumbersome devices of sympathy theory in OT) are preferable to complicating rules by repeating information that is already handled by independently necessary rules.

In an analogical view of lexical phonology, however, such a solution is precisely the desirable one, since in this view lexical phonology consists of nothing more than many thousands of overlapping "mini-rules" that are specific to particular pairs of words.  We have already seen many examples of such mini-rules, from the tiniest in irregular inflection up to the widely extended ones in vowel alternations.  By now it should not be shocking to suggest that lexical patterns can overlap to an enormous extent; the vowel alternation patterns are so similar to each other, for example, that it has been very difficult, even after thirty years of work, to figure out which ones go together and which ones need to be kept distinct.

Moreover, in the case of some opaque interactions, we can actually say more than this. We have already seen cases where analogical patterns compete with each other in their influence on words (e.g. *relate-relation* vs. *evade-evasion* competing for control of *equate-equation*).  The winners in such competitions often seem to be the analogical patterns which are more widely attested across the lexicon, i.e. which are instantiated as alternations in more pairs of words.  In some cases, as with *equation*, the result can in fact be incompatible with any general rule ordering analysis.

Opaque interactions like that between velar softening and s-voicing involve much the same sorts of direct competitions.  Thus a word like *Grecian* contains an intervocalic coronal fricative, with the first vowel long, precisely the conditions for s-voicing.  Why doesn't s-voicing apply?  An analogical analysis would say that velar softening beats out s-voicing in *Grecian* for the same reason *evasion* beats out *relation* in influencing *equation*.

There are two good reasons why velar softening is expected to override s-voicing.  First, "standard" s-voicing requires that the preceding vowel be long.  Yet, as we have seen, the vast

majority of velar softening alternations occur with stems whose final syllable contains the short vowel [ɪ]. This means that derived words that conform to the velar softening pattern tend not be similar to derived words that conform to the s-voicing pattern, and vice versa. In general, then, the patterns can't interact. Words like *Grecian* that potentially can be influenced by either pattern are very rare, and they should therefore tend to conform to whichever pattern is stronger overall.

Second, velar softening is stronger because it is far better attested in the English lexicon than is s-voicing. The following figure from J. Myers (1993) gives estimates, based on dictionary searches, of the number of base words involved in alternations that provide positive evidence for s-voicing and for velar softening. Velar softening examples outnumber s-voicing examples over fifteen-to-one. If velar softening says that the root-final consonant in *Grecian* should be voiceless, s-voicing doesn't have much of a chance to disagree. (When I formalize analogy in a later section, I'll show how the sheer force of numbers automatically translates into the notion of analogical "strength.")

(55)                                         number of alternating word pairs

      s-voicing:                     21
      velar softening:             320

Given that the analogical approach to opaque interactions is so noxious to assumptions that have been fundamental in generative phonology since its founding, it is worthwhile to consider independent evidence that it is on the right track. One clear prediction of this approach is that opaque interactions can only be found within the lexicon (via competing analogical patterns, or redundantly overlapping analogical patterns), or at the fuzzy interface between lexical and postlexical phonology, where some postlexical process creates or destroys the environment for an analogical pattern which of course behaves as if the postlexical process didn't exist. In other words, opaque interactions should never be found solely within postlexical phonology alone. This prediction has thus far held up to careful scrutiny. A thorough search of the literature on English (J. Myers 1992, 1993) found no unambiguous evidence for opaque interactions among postlexical patterns.

The most famous example of rule ordering appears at first to falsify my claim, but it actually doesn't. This is the interaction of Canadian raising and flapping, first discussed by Halle (1962), based on data in Joos (1942). Briefly, Canadian raising raises diphthongs before voiceless consonants, as in *write*, while flapping changes intervocalic voiceless consonants to voiced flaps, as in *writer* and *rider*. Joos (1942) noted two distinct ways these processes can interact for different speakers. In both "dialects," words without flaps like *write* and *ride* had different diphthongs, but in words with flaps there was a difference: in one dialect *writer* and *rider* had the same raised diphthong, while in the other only *writer* had the raised diphthong. Halle (1962) showed how this could be handled by extrinsically ordering raising and flapping; only when raising preceded flapping would *writer* and *rider* end up with different pronunciations. This ordering is opaque, since it makes the raising in *writer* non-surface-apparent. In fact, the other

dialect, in which *writer* and *rider* were pronounced identically, has ceased to exist (Chambers 1973), leading some to wonder if it had ever existed at all (Kaye 1990).

One implicit assumption in some discussions of this interaction is that both processes are postlexical, but this appears not to be true.  First, Canadian raising has some lexical exceptions; Chambers (1973) cites words like *Cyclops* that don't undergo raising while similar words like *micron* do; Vance (1987) provides further examples in a Canadian-raising-like process found in the northeastern USA that has the same "opaque" interaction with flapping.  Second, like a typical lexical process, and unlike a typical postlexical process, raising is phonetically categorical; raised diphthongs are raised by the same amount regardless of details of the phonetic environment (J. Myers 1997; see later in the paper for further discussion of categoricality and its relevance for understanding lexical phonology).  Third, even flapping may be lexicalized to some extent; see later in the paper for mention of the so-called Withgott effect (Withgott 1982, Steriade 1996).

Thus what appears to the case with this famous interaction is that at the time Joos (1942) reported his observations, Canadian raising was just beginning to be lexicalized.  For speakers who had not yet lexicalized it, raising and flapping interacted in a transparent fashion, as is expected given usual OT constraint interactions.  Only when raising became lexicalized for some speakers, thus turning into analogy in my view, did the opaque interaction emerge.

In short, pattern interaction can be opaque only if at least one of the patterns is actually analogy.  Most of the time this interaction mimics the order in which the patterns became part of the language, or more precisely in my view, became lexicalized; see Halle (1962) for an early discussion of this observation in the generative literature and Hayes (1986) for a more recent discussion.  But since within a speaker's competence there is of course no knowledge of history, the ordering is actively maintained, as well as corrupted, by analogy alone.  The interaction of "strong" velar softening and "weak" s-voicing is an example of analogical maintenance of an "ordering" relation; cases like *equate-equation* (or the many other similar instances of "local ordering" in e.g. S. Anderson 1974; Bley-Vroman 1975; Cole 1990; Hooper 1976; Kiparsky 1973, 1978; Rice 1980; Robinson 1976, 1977) provide examples of analogical "corruption."

## 2.7　English lexical phonology:  summary

In this section I have demonstrated an objective fact:  lexical phonology acts like analogy. The evidence, like the patterns themselves, is convoluted, but as promised, it all boils down to three general observations.  The first is negative:  general rules do not work, since there are always cases where rules underapply, overapply, or do something else entirely.  It is also impossible to find just a small set of maximally distinct rules to handle all of the most important cases; inevitably the necessary rules will overlap to a great extent.  The second observation is that "standard" lexical phonology, just like irregular inflection, involves family resemblances.  These can be seen in the overlapping patterns themselves (e.g. the many similar vowel alternation patterns), and they also play a role in defining classes of words that undergo particular alternations (e.g. most stems affected by velar softening end in [ɪk]) and classes of words that don't (e.g. most monomorphemic exceptions to shortening contain NC clusters).  Family resemblances imply pairwise comparisons of individual lexical items, and this implies analogy.

The third observation is that alternations are nondirectional: derived forms can contain more than the sum of their parts (e.g. words ending in -*sion* form a family in and of themselves, although the associated stems forms may all be quite different), and base forms can be influenced by the associated derived forms (e.g. "backwards" analogy in Glenoe Scots, or the fact that semi-weak verb stems that contain short vowels and end in [t] or [d] tend to contain precisely the same short vowels that are "derived" when [t] or [d] is suffixed).

In addition to these arguments in support of analogy, along the way I have also considered potential arguments against it, in particular alternative paths that English could have taken. The analogical approach would be falsified (or at least seriously threatened) if any one of the following had turned out to be the case: if vowel shift were as pervasive as it actually is, interacting with s-voicing and velar softening, yet were exceptionless or describable with a single simple rule; if vowel alternations could be described with a set of totally distinct rules, rather than a set showing strong family resemblances; if systematic violations of the Scottish Vowel Length Rule like *drive-drove* existed without pairs like *ride-rode* also existing; if overapplications of s-voicing like *equate-equation* existed without words like *evade-evasion* also existing; if opaque interactions exist in postlexical phonology, or within lexical phonology without independently motivated evidence for differences in strength of the interacting patterns (e.g. s-voicing and velar softening). These and several more claims were tested, and all came up on the side of the analogical approach.

Moreover, I don't believe that the success of the analogical approach in English is due to something unusual about English. I see no reason to doubt that claims very similar to the ones I have made here also hold in every other language in the world, even in languages with virtually no lexical phonology at all (see e.g. Wang's 1998 study of Mandarin phonotactics).

Before concluding this section, I should clarify an important methodological point implicit in the preceding discussion: an analogical approach doesn't negate all previous work on lexical phonology, but rather builds on it. Recognizing that lexical phonology is actually represented in the grammar by analogy, which involves overlapping sets of pairwise comparisons, does not entail that we must give up descriptions based on general rules or constraints. Not only can't we expect phonologists to search for generalizations pair by pair (though with computers this task is not as difficult as it once was), but such a method may mistake a more general pattern for an extremely idiosyncratic one. Thus for example, in spite of my harsh criticism, I believe that the attempts by Rubach (1996) and others to find the most general descriptions possible for the vowel alternations form a highly important and necessary body of work.

Nevertheless, phonologists must learn to recognize such descriptions as but one step in a larger research program, since general rules are just a shorthand way of describing many overlapping exemplar-driven analogies. There may be many reasons not to want to go beyond this shorthand in any particular project, but a desire to truly understand lexical phonology cannot be one of them.

Having established the empirical reality of synchronic analogy, then, the job now is to devise a formalism that can capture this notion naturally, explicitly and in a restrictive fashion.

### 3.	Formalizing analogy

In this section I show that formalizing analogy is possible. I first show why OT is useful in this regard and yet why OT has still not done it. Then I do it.

Actually, precise formalisms already exist for the expression of what I call analogy, including connectionism (e.g. Rumelhart and McClelland 1986 and numerous followups), work in the approach pioneered by Skousen (1989), and more recent work such as Frisch (1996) (which strictly speaking only handles morpheme structure constraints). In addition, Kirchner (1999) sketches the outlines of an approach that attempts to join formalist work in OT with experimental work in connectionist modeling. However, such formalisms have yet to go "mainstream" in linguistics, the reasons being partly sociological, partly philosophical and partly empirical. The sociological problems include selling such quantitative approaches to math-phobic linguists. The philosophical problems concern the different kinds of theoretical explanations preferred by linguists compared to other cognitive scientists (see e.g. Miller 1990). I can't hope to touch these sociological and philosophical issues in this paper.

The empirical objections to quantitative analogical models are more important: no one has yet demonstrated that they are capable of handling all the phonological patterns that generative formalisms handle so well. I think the problem here is that scholars who have recognized the role of analogy in lexical phonology have been so put off by available generative formalisms that they have felt obligated to go out and try to reinvent the wheel, and therefore at this early stage their work still has some bugs. An alternative strategy, I suggest, is to try to build on the honest toil of generations of generative linguists. I hope to provide some first steps in this paper.

### 3.1	Can analogy really be formalized?

Some may suppose that my quest is doomed from the start. By its very nature, analogy is idiosyncratic. Even if we put an idiosyncratic lexicon into the model, and even if we adopt principles that prevent "crazy" analogies like *ear:hear::eye:*heye* (Kiparsky 1988), there is still no completely consistent algorithm for predicting precisely what analogies will be made. Analogy involves pairs that match in "phonological similarity" and pairs that are "related" some other way, and there is simply no way to automatically pick just the pairs we want. For instance, the past tense of *dive* may be *dove* by analogy with *drive-drove*, but then why don't we also have *arrive-*arrove*, or why doesn't *drove* become *\*drived* by analogy with *arrived*?

Attempts to look further afield for an automatic analogical algorithm have proven fruitless. Could the key be lexical frequency? Perhaps *arrive* fails to pattern with *drive* because *arrive* doesn't have a high enough token frequency; after all, frequency has been shown to be an important factor in diachronic analogy (e.g. Phillips 1984). But then how frequent, precisely, is frequent enough (see also Chapman 1995)? And why are there cases where lexical frequency seems to have no effect on diachronic lexical changes whatsoever (e.g. Yaeger-Dror and Kemp 1992)? I will later discuss the role of frequency in analogy, but it must be admitted that this is still rather poorly understood.

What about semantic factors? Semantics plays an important role in analogy, but the role is, if anything, even more complex than frequency. For example, Yaeger-Dror and Kemp (1992)

found that an on-going lexical diffusion in Montreal French was unaffected by frequency or etymology, but was affected by lexical semantics of a curious sort: words kept the older pronunciation if they referred to the "good old days." Chapman (1995) found that the likelihood of an analogical change occurring in a set of Swiss German dialects was related to the degree of semantic relatedness between the alternating forms. The same complexity is found with morphological factors: "backwards" analogy, where the derived form influences the underived form, are not uncommon (Chapman 1995). It even occurs in English dialects, as we saw above in Glenoe Scots.

I believe that the correct response to these difficult issues for a generative phonologist interested in analogy is to ignore them (at least for now). That is, we should feel no obligation to explain how the relevant word pairs are chosen when making an analogy, except to require that phonological similarity and perhaps some non-phonological relationship be involved. We don't even have to require that the non-phonological relationship be morphological, though apparently it often is. As we will see, in my formalism, the pairing of lexical items in the analogies is indicated extrinsically, as a part of the grammar. Arbitrary pairs are chosen to make it work, just as arbitrary constraint rankings are chosen to make OT work. This approach captures a reasonable insight: native speakers can only generalize a pattern like velar softening if pairs like *critic-criticism* are indeed seen as "pairs," and failing to see them as pairs does not mean they are not really native speakers. Similarly, if somebody recognizes the pair *critic-criticism* but also decides that *witty-witticism* form a "similar" pair, thereby leading to the induction of a novel idiolectal generalization, then surely this strange "knowledge" should be part of this person's grammar. My point is that exactly what leads people to posit certain analogies and not others is indeed a mystery, but once the analogies are posited, grammatical analysis is still possible. Analogy cannot be completely housebroken, but it can be tamed. I propose to show how.

### 3.2    Optimality Theory and the lexicon

OT is often claimed to be a revolutionary new way of dealing with phonological problems, and in the case of lexical phonology, I will ultimately suggest that the hype is deserved. OT has several properties that allow it to express our desired insights far more easily than earlier formalisms. First, OT rejects the mechanism of derivations in favor of constraints on outputs, which means in principle that we no longer have to claim that the [s] in *criticism* derives from a /k/; instead, the pattern can be expressed on the surface, where it appears to belong.

Second, a natural consequence of a theory of output constraints is that the input cannot in principle be constrained at all, a doctrine enshrined as Richness of the Base (e.g. Prince and Smolensky 1993, Smolensky 1996). This doesn't mean that we can ignore the input entirely, as we would want to in a truly exemplar-driven lexical phonology, but it does allow us to stuff it with details that derivational theories would have dismissed as too "surfacy"; an extreme example of this is found in the OT offshoot known as phonetically-driven OT (e.g. Flemming 1995; Hayes 1995; Jun 1995; Kirchner 1997; Silverman 1996; Steriade 1996).

Third, since constraints in OT are violable, we no longer have to be puzzled about cases where velar softening and other exception-ridden lexical generalizations fail to hold, as long as we can come up with a good excuse for their misbehavior.

Finally, OT is a descendent of (though not reducible to) connectionism (Smolensky 1986, 1995b; Prince and Smolensky 1997), and connectionism offers an explicit and rather successful method for eliminating the rule-list dichotomy (e.g. Rumelhart and McClelland 1986).

In spite to these revolutionary properties, however, the approach OT has taken towards lexical issues has unfortunately been strictly traditional. This is perhaps because OT describes phonology with universal constraints rather than language-specific rules as the theory of Lexical Phonology did. The fact that actual lexical phonology is notoriously language-specific, phonetically unnatural, and so forth, is not inherently interesting to OT researchers, who understandably want to show off the theory's strengths rather than its weaknesses. This is a shame, since it means we can't exploit all those beneficial properties of OT to handle lexical phonology in a more accurate way.

In essence, OT deals with the lexicon the same way that Chomsky and Halle (1968) did: the lexicon is a repository of arbitrary information, and all the interesting work is done by the "grammar proper" (i.e. the constraints). The new claim is that the lexicon and grammar interact via a set of universal <u>faithfulness constraints</u> that enforce identity of various aspects of the input and output. Since the faithfulness constraints just say "be identical," leaving the specific effects to whatever arbitrary information is in the lexicon, the lexicon is thus crucial for explaining why all words are not reduced to some maximally unmarked state, e.g. [ta].

There are important consequences of this approach, however, that can lead to new insights into how OT might handle lexical phonology. First, Hale and Reiss (1998) have argued that the problem of simultaneously learning a lexicon and a constraint hierarchy means that learners must start by hypothesizing that all faithfulness constraints are ranked at the top. By contrast, if the child began with markedness constraints at the top, as assumed by Tesar and Smolensky (1998) (see also Prince and Smolensky 1997), then instances of neutralization would make it impossible for the child to learn the lexicon. Hale and Reiss's argument, if valid, thus implies that the child's default hypothesis is that there is nothing to know about lexical phonology except the lexicon itself (i.e. the set of faithfulness constraints), and so any generalizations that may be posited are derived from surface forms.

Another consequence of OT's approach towards the lexicon concerns the role of marked lexical patterns in phonological competence. Imagine three languages, L1, L2 and L3, each containing only monosyllabic words. In L1, all words are open syllables, thus obeying NOCODA. Lexicon Optimization (Prince and Smolensky 1993; see later in paper) will therefore lead the child to an analysis where all the words are represented as open in the lexicon, and it won't matter how NOCODA is ranked with FAITH in the grammar. In L2, all words are closed syllables. Since there is no universal markedness constraint enforcing the presence of codas, the constraint ranking in L2 must be FAITH >> NOCODA. Finally, in L3, all of the words are open syllables except for a small number of words that have codas. If these exceptional words aren't somehow compartmentalized (e.g. with a co-phonology), OT has no choice but to claim that the grammars of L2 and L3 are identical (i.e. FAITH >> NOCODA). Yet surely it's an important fact about a language whether it routinely violates a universal markedness constraint or almost never does.

The problems OT faces with respect to the lexicon are due to its acceptance of the rule-list dichotomy, which is strange given its antecedents in connectionism. As Smolensky (1995b)

and Prince and Smolensky (1997) observe, OT acts like a constraint-satisfaction network (except for the fact that OT constraints stand in a strict dominance relation, whereas in connectionism gangs of lower-ranked constraints can sometimes override higher-ranked ones). What is missing in Prince and Smolensky's observation is the fact that in connectionist models of the lexicon, the constraints can be imposed by specific lexical items, not just by universal ("hard-wired") properties of the units or architecture. To allow OT to handle the lexicon as well as connectionism does, therefore, we must allow for idiosyncratic, lexicon-specific constraints.

Fortunately, just such things are already allowed; they are sometimes called parochial constraints. Already in the earliest work on OT, McCarthy and Prince (1993a) used constraints that refered to specific lexical items, e.g. the Tagalog infix *um*, which has the idiosyncratic property of prefixing rather than suffixing. The approach has been extended in works such as Benua (1995, 1997a,b) to restrict constraints to specific lexical classes, e.g. class 1 versus class 2 morphology in English, and has been taken to its logical extreme in work by Hammond (1995, 1997), Russell (1995) and Golston (1996). Hammond (1995), for instance, following a suggestion in Kiparsky (1982), proposes that lexical items are themselves constraints; e.g. *cat* is represented by the constraint CAT = [kæt]. In the insightful approach of Golston (1996), lexical forms are represented as a set of violations of independently necessary markedness constraints; thus the lexical representation of *cat* would include violations of *DORSAL and NOCODA but not of ONSET.

Constraints like this will naturally prove essential in my formalization of analogy, but since I don't want to overwhelm the reader with totally unfamiliar notation, my parochial constraints will always be of the same type: identity constraints that refer to specific words. For example, the constraints associated with the lexical representation of some word $W$ will be the following input-output identity constraints, where $F_1$, ... $F_n$ are all the phonological properties necessary to describe the surface form of $W$.

(56)    $W$:  IDENT-IO($W$;$F_1$), ... IDENT-IO($W$;$F_n$)

Notice that there is no claim that $W$ is monomorphemic; Hammond's, Russell's and Golston's approaches all maintain the assumption that the lexicon contains nothing but morphemes, never whole complex words, but I have already given reasons for rejecting this assumption.

It's also important to note that by "phonological properties" I mean anything phonological; they could be phonetic features, entire segments or prosodic structure. They are binary since a lexical item is assumed to either have a property (e.g. +F) or not (-F); input underspecification doesn't work given Richness of the Base. Unfortunately, this choice of formalism means that I can't go all the way in my attempt to incorporate the lexicon into the constraint hierarchy, since a lexical representation of $W$ as [+F] or [-F] is still necessary to determine what precise effect the constraint IDENT-IO($W$;F) will have. The formalisms of Hammond (1995) and Golston (1996) do not have this problem, but I have found them too hard to make do what I want; resolving the issue is left to future work.

The key idea remains, however: this kind of parochial constraint serves as a way of "exploding" faithfulness. This is just what is needed to distinguish the languages L2 and L3 discussed above. These two languages would now be represented as having distinct grammars (with Lexicon Optimization automatically cleaning up the lexicon):

(57)   L2:   {IDENT-IO($W_1$,coda), ... IDENT-IO($W_n$,coda)} >> NOCODA
       L3:   {IDENT-IO($W_1$,coda), ... IDENT-IO($W_i$,coda)} >> NOCODA
             >> {IDENT-IO($W_{i+1}$,coda), ... IDENT-IO($W_n$,coda)}

One immediate consequence of parochial constraints is that they can be extrinsically ranked. What might this ranking mean? Hammond (1997) suggests that it may correlate with lexical frequency, since lexical differences in frequency, as already noted, play a nontrivial role in lexical phonology. I will discuss the dramatic implications of this interesting suggestion in a later section.

Input-output faithfulness and parochial constraints are not the only means available in the current OT literature for dealing with the lexicon. More recently, the concept of faithfulness has been generalized in <u>correspondence theory</u> (e.g. McCarthy and Prince 1995b; Kenstowicz 1995; Benua 1995, 1997a,b), which allows faithfulness relations to hold between different parts of an output (e.g. reduplicant and base) or even between different lexical items entirely. The formalism of output-output (OO) correspondence allows us to express a basic ingredient of analogy: comparison of one lexical item with another. For this very reason, perhaps, OO-correspondence has something of a bad reputation in some circles within the OT community (see e.g. Booij 1997; Hale, Kissock and Reiss 1998). The problem is that it is difficult to know how to restrict OO-faithfulness to just the right lexical items (though see Benua 1997b for the most thorough attempt to date), so it is all too tempting for the linguist simply to choose the ones that make the analysis work.

This objection doesn't hold, however, if one views lexical phonology as derived from the lexicon rather than the other way around: parochial OO-constraints can be just as arbitrary as needed to describe a particular lexicon. I therefore suggest that included in the set of universal constraints is a family of OO-faithfulness constraints that are parameterized to hold of all word pairs in a given lexicon. Proposed universal constraints should be motivated, and here the parochial OO-faithfulness family derives its motivation from an inherent drive to reduce "memory load," represented as "energy minimization" or "relaxation" in constraint-satisfaction connectionist models of the lexicon (e.g. Hopfield 1982).

Thus given a lexicon with the four words *a*, *b*, *c*, and *d*, and the feature F, we would automatically have the following six constraints. (Note that the italicized letters label words, not their phonological forms.)

(58)   IDENT-OO(*a*,*b*;F)          IDENT-OO(*b*,*c*;F)
       IDENT-OO(*a*,*c*;F)          IDENT-OO(*b*,*d*;F)
       IDENT-OO(*a*,*d*;F)          IDENT-OO(*c*,*d*;F)

This approach "explodes" the sort of OO-faithfulness constraints found in the OT literature.  Like all constraints, each of these can be extrinsically ranked with respect to other constraints in the grammar.  The fact that this ranking is extrinsic thus provides a principled response to the above criticism of correspondence theory:  of course linguists (and presumably language-acquirers) are free to choose whatever ranking makes the analysis work!  For example, if in some language some word *b* is forced to become similar to *a*, but another word *c* is not forced to become similar to *a*, this can be expressed through the following extrinsic ranking:

(59)    IDENT-IO(*a*;F) >> IDENT-OO(*a*,*b*;F) >> {IDENT-IO(*b*;F), IDENT-IO(*c*;F)}
        >> IDENT-OO(*a*,*c*;F)

There are some technical issues to finesse here.  Correpondence theory proposes that faithfulness holds between corresponding parts of related forms.  Thus the formal definition of correspondence goes something like this (McCarthy and Prince 1995b):

(60)    Given two strings $S_1$ and $S_2$, related to one another by some linguistic process, *Correspondence* is a relation *f* from any subset of elements of $S_1$ to $S_2$.  Any element a of $S_1$ and any element of $S_2$ are *correspondents* of one another if b is the image of a under Correspondence; that is, b=*f*(a).

How can this possibly work if the two forms are totally unrelated words as I suggest, say *chicken* and *egg*?  Well, notice that in the above definition, the phrase "linguistic process" is undefined (and also peculiar, given OT's nonderivational stance).  Who's to say that storing both *chicken* and *egg* in a single lexicon shouldn't itself count as a linguistic process?  Moreover, the tacit assumption that $S_1$ and $S_2$ are in an asymmetrical relationship that can be determined by universal principles (e.g. the Base Identity principle of Kenstowicz 1995, 1996) cannot be maintained in the face of evidence of stems coming to conform to reduplicants (McCarthy and Prince 1995b) and similar phenomena in analogy that we saw above.  Thus *chicken* is trying to become *egg* and *egg* is trying to become *chicken*, and there's no general way to predict which, if either, will win; the winner is described solely by the extrinsic ranking of the relevant parochial OO-constraints and IO-constraints.

As for the question of what should count as "correspondents" in *chicken* and *egg*, notice that the definition doesn't tell us any more than Correspondence is some relation (a "relation" being something like a function, but less restricted).  If OT researchers are comfortable with choosing whatever correspondents make the analysis work without formalizing precisely how this happens, then that's good news for me:  the search for universal principles of Correspondence relations can be done by somebody else while we forge ahead under the assumption that they will eventually be found.  If, however, there is an unspoken consensus that correspondents are defined by a metric calibrated via morpheme edges, then I may be in more trouble, if I truly want to say that morphologically complex words are wholes without any internal structure.  Well, then, I won't say that.  Words like *legal* and *illegality* are both stored in memory, but *illegality* may be stored as *il[legal]ity* (as noted earlier, the "external" evidence on such questions go both ways).  Then the two words can undergo OO-correspondence using the

indicated edges to line them up properly. However, I would still want to be able to line up unrelated words by word edges alone in order to use analogy across paradigms (see below).

To summarize, I propose to provide a more realistic analysis of lexical phonology by combining a word-based approach to morphology with two formal devices made possible by OT: OO-faithfulness and parochial constraints. Details of how these can be used to formalize analogy will be given shortly.

### 3.3 Previous formalizations of analogy in OT

At one time, when analogy was discussed in the generative literature at all, it was usually just to dismiss it as an epiphenomenon of a putative drive towards grammatical simplification, combined with noisy performance factors in language acquisition (e.g. Kiparsky 1978, 1988). Recently, however, it has been suggested that the OT device of OO-correspondence is just what is needed to express the essential insight that analogy involves forcing identity to other lexical items (see e.g. Kenstowicz 1995, 1996; Steriade 1996). Unfortunately, though I think they are on the right track, such suggestions are not sufficient to handle lexical phonology in the way that I envision.

One major problem with how analogy has been formalized in OT so far is that the formalisms can only deal with paradigmatic leveling, not with four-part proportional analogy (as pointed out by Reiss 1998). The reason is that the OO-correspondence constraints that are used are not parochial, thus requiring that they be restricted to pairs of items that can be universally defined. It is claimed that analogy results from OO-correspondence holding between morphologically related forms, in particular a base form and words derived from it. This means the approach can handle cases like the leveling of English plural forms, but not of the many cases like irregular inflection. For my purposes, what is particularly problematic is that it cannot handle analogy and "regular" lexical phonology in the same way. After all, the prototypical lexical phonological pattern is described in terms of multi-part proportions. Just giving a single pair like *electric-electricity* is not enough, since the *k~s* alternation may be a fluke. Only if other pairs are added, e.g. *opaque-opacity*, can we say that we actually have a pattern. Thus it is crucial that we be able to use cross-paradigmatic analogy, not just paradigmatic leveling.

Another problem with the current approach to analogy in the OT literature is the assumption that paradigmatic leveling is really always "analogy" of the sort that I mean, i.e. enforcing similarity between stored lexical items. As Steriade (1996) observes, the commonness of paradigmatic leveling is directly correlated with the productivity of the associated morphology. As I have argued, however, "morphological productivity" is just another name for "likelihood of being generated outside of the lexicon." Hence there is no way for analogical effects, which by definition occur between items represented in an actual lexicon, to affect items *more often* the *less likely* they are found in the lexicon. The correlation goes exactly the wrong way.

What does explain paradigmatic leveling, then? For one clue, consider an analysis in Benua (1997a,b) of the observation that in the word *condemnable*, the supposedly underlying cluster /mn/ is simplified to /m/ in spite of the presence of a following vowel which should "protect" the /n/ by parsing it as a syllable onset (thus bleeding cluster simplification). The

analysis assumes an OO-correspondence constraint, DEP-OO$_2$, which prevents a form derived through class 2 morphology (e.g. *-able*) from including more material than the surface form of the base. DEP-OO$_1$ is a parallel constraint parameterized for class 1 morphology (e.g. *-ation*), which here has no effect.

(61)     [simplified from Benua 1997a]

| /kandɛ**mn**/ /kandɛ**mn** + ʌbl/ | *mn]s | DEP-OO$_2$ | MAX-IO | DEP-OO$_1$ |
|---|---|---|---|---|
| kʌn.dɛ**mn** kʌn.dɛ**m.n**ʌ.bl] | * | | | |
| kʌn.dɛ**m** kʌn.dɛ**m.n**ʌ.bl] | | * | * | |
| ☞     kʌn.dɛ**m** kʌn.dɛ.**m**ʌ.bl | | | * * | |

In other words, what the analysis works so hard to explain is actually a non-event: suffixing *-able* doesn't cause any difference from the surface form of the word *condemn*. In my view of the lexicon, where the input form of *condemn* already ends in [m], not /mn/, this is expected. As a productive process, *-able* suffixation can produce words outside of the lexicon, and as such *condemnable* is not subject to analogical forces. By contrast, *-ation* suffixion is not productive, meaning that *condemnation* is in fact stored in the lexicon, thus allowing for the analogical spreading of [mn] from other stored words like *autumnal*.

Admittedly, there are more complex cases. For example, Steriade (1996) discusses a pattern in English whereby suffixation can shift stress without causing the distribution of aspiration to follow suit (the so-called Withgott [1982] effect), e.g. *mìli[t$^h$]àry-mìli[t$^h$]arístic*. She analyzes this by ranking OO-faithfulness constraints (parameterized to aspiration) above markedness constraints for aspiration and stress. I admit that there is no obvious way to ascribe this to the lack of analogy in productive morphology, since the stress does shift, presumably by analogy, and gladly accept that OO-faithfulness may work to produce some cases of paradigmatic leveling. However, note that this case involves class 1 morphology, which is famously less productive than class 2 morphology.

My central point thus stands: real analogy, formalized with OO-correspondence, only occurs across lexical items in a specific lexicon, thus occuring less often the more productive the associated morphology. Analyses such as that of Benua (1997a), sketched above, are therefore misguided. Of course, this now leads us to wonder why analogy doesn't force identity between *condemn* and *condemnation*, if both are stored in the lexicon. Here I agree with Benua's insights: this is blocked by ranking OO-faithfulness constraints for less productive morphology (DEP-OO$_1$, in her analysis) below IO-faithfulness constraints (in her case, MAX-IO; in my case, something like IDENT-IO(*condemnation*,[mn])). That is, the word *condemnation* just doesn't want to become similar to *condemn*.

In short, I am proposing to restrict the use of OO-correspondence to word pairs that are actually listed in the lexicon, i.e. involving less productive morphology, and restrictions, as all linguists know, are quite desirable things.

### 3.4     The proposal

Generally speaking, my formalization of analogy simply translates the traditional concept of analogy into OT terms. Simplying somewhat, the traditional concept of proportional analogy involves the following implication. Note that again F and G represent phonological properties of any sort, not just phonetic features; they could describe entire segments or prosodic structure.

(62)     Given words *a*, *b*, *c*, *d*:

|       |       |                                                          |
|-------|-------|----------------------------------------------------------|
| *if*  | i.    | *a* is related to *b*, and *c* is related to *d*,        |
| *and* | ii.   | *a* shares phonological property F with *c*,             |
| *then*| iii.  | then *b* shares phonological property G with *d*.        |

The goal is to explain why *i-ii* are necessary but not sufficient conditions for *iii*. I'll begin by formalizing step *iii* in OT, and then propose the minimum needed to make it depend on *i-ii*.

In order to capture step *i* under the assumption of a fully specified lexicon, I will use parochial faithfulness constraints of the form described above. To make the discussion specific, I assume that word *d* is originally different from *b*, and the analogy forces *d* to match *b* rather than the other way around. The lexicon thus starts off as follows.

(63)     *a*=[+F], *b*=[+G], *c*=[+F], *d*=[-G]

IDENT-IO(*a*;F)     IDENT-IO(*b*;G)     IDENT-IO(*c*;F)     IDENT-IO(*d*;G)

The formalization of step *iii* can be accomplished simply with the following parochial OO-faithfulness constraint.

(64)     IDENT-OO(*b,d*;F):  *b* and *d* must share the feature value for G.

Since what we want to have happen is that *d* comes to be realized as [+G], we must rank IDENT-OO(*b,d*;G) above IDENT-IO(*d*,G). In the remaining discussions, I will call *d* the "weak" word in the analogy. It doesn't matter where we rank the other constraints as long as they're also higher than IDENT-IO(*d*;G). A tableau is shown below. Candidates for all four words are shown together, since in my view of lexical phonology, what is evaluated is an entire lexicon, not an individual item (this parallels what occurs in the constraint-satisfaction networks which inspired OT). For now we only consider the four outputs that involve variations in words *b* and *d*.

(65)  {IDENT-IO($a$;F), IDENT-IO($b$;G), IDENT-IO($c$;F)} >> IDENT-OO($b,d$;G)
    >> IDENT-IO($d$;G)

| [+F]$_a$, [+G]$_b$, [+F]$_c$, [-G]$_d$ | IDENT-IO($a$;F) | IDENT-IO($b$;G) | IDENT-IO($c$;F) | IDENT-OO($b,d$;G) | IDENT-IO($d$;G) |
|---|---|---|---|---|---|
| ☞ [+F]$_a$, [+G]$_b$, [+F]$_c$, [+G]$_d$ | | | | | * |
| [+F]$_a$, [+G]$_b$, [+F]$_c$, [-G]$_d$ | | | | * | |
| [+F]$_a$, [-G]$_b$, [+F]$_c$, [+G]$_d$ | | * | | * | * |
| [+F]$_a$, [-G]$_b$, [+F]$_c$, [-G]$_d$ | | * | | | |

Before we criticize this analysis for what it doesn't do, let's first appreciate it for what it does. First, it captures the insight that analogy involves idiosyncratic comparisons between lexical items. This is accomplished with the constraint IDENT-OO($b,d$;G), which is idiosyncratic in two ways: by its reference to specific words $b$ and $d$, and by its relatively high ranking (comparable constraints like IDENT-OO($b,c$;G) don't have any effect because they are stipulated to have low rank in this grammar). Second, it accounts for the equally idiosyncratic fact that word $b$ influences word $d$, and not vice versa. This is handled by the extrinsic ranking IDENT-IO($b$;G) >> IDENT-IO($d$;G). Third, the input does very little work, which is what we want if the input (i.e. lexicon) is identical to the constraints (i.e. grammar). In particular, if $d$ is represented in the leicon as [+G] rather than [-G], the analysis will result in exactly the same surface representations, as a moment's thought will make clear. Finally, the formalism does all this without increasing the power of OT any more than has already been done in the literature with the introduction of parochial constraints and output-output correspondence.

Now for the bad news. We have stipulated step *iii*, rather than having it depend on steps *i* and *ii*.

There are a number of possible responses. One is to say "So what?" There's already so much stipulation, so what's a bit more? As we saw when we examined real cases, however, this would not be a satisfactory move. Some aspects of any particular instance of analogy are not predictable, it is true, but some aspects are regular, and we are obligated to acknowledge this. Specifically, it seems crucial that the words form "pairs" *a-b* and *c-d* (step *i*) and that *a* and *c* are phonologically similar (step *ii*). These may not be sufficient conditions for analogical extensions (step *iii*), but they are necessary. The response I will make, therefore, is to stipulate only what needs to be stipulated, and do the rest through inherent properties of the model.

The crucial observation to make is that steps *i* and *ii* involve a logical conjunction (i.e. *AND*). This may ring a bell: in the grab bag of formal extensions to OT is the notion of <u>constraint conjunction</u> (see Crowhurst and Hewitt 1997; Smolensky 1995a; and references therein). One typical piece of evidence for conjoined constraints is the observation that morphemes in Diyari must both begin and end with some foot. In order to describe this within

Generalized Alignment (McCarthy and Prince 1993b), two alignment constraints must be conjoined: a word is only well-formed if it obeys both INITIAL-FOOT and FINAL-FOOT at the same time. Evidence for constraint conjunction doesn't depend just on prosody, however, and the formal device of combining constraints with the Boolean operator *AND* makes the empirically verified prediction that constraints may also be derived through Boolean disjunction (*OR*) and implication (*IF-THEN*) (Crowhurst and Hewitt 1997). Crowhurst and Hewitt (1997) suggest that such <u>macro-constraints</u> are generated on a language-particular basis.

For my purposes, it turns out that simple constraint conjunction (*AND*, symbolized with ∧) is sufficient. How does constraint conjunction help with the problem at hand? Recall that we want to show how step *iii* is related to steps *i* and *ii*, but both step *i* (non-phonological relationships in pairs *a-b* and *c-d*) and step *ii* (phonological similarity between *a* and *c*, and between *b* and *d*) are idiosyncratic. That is, no universal principle can tell us that *a* and *b* must form a pair, and no universal principle can tell us that *a* and *c* are phonologically similar enough to force *d* to match *b*. The solution is thus to conjoin two parochial IDENT-OO constraints. The fact that two constraints have been idiosyncratically conjoined accomplishes step *i*; the specific words idiosyncratically referenced in each IDENT-OO constraint accomplishes step *ii*.

The analysis thus works as follows. In addition to the IDENT-IO constraints for each word in a lexicon, we include IDENT-OO constraints for all word pairs, as described earlier. The fact that our language idiosyncratically obeys the four-part analogy *a:b::c:d* is then expressed by the addition of the following conjoined constraint, which I will call, for lack of a better term, an <u>analogical macro-constraint</u> (abbreviated OO∧OO-constraint).

(66)    IDENT-OO(*a,c*;F) ∧ IDENT-OO(*b,d*;G)

As with Crowhurst and Hewitt (1997), I assume that the positing of macro-constraints occurs on a language-particular basis; the existence of the component parochial constraints is a necessary, but not sufficient condition for analogy. This accounts for the fact that analogical extentions do not occur easily.

However, it should be noted that my use of constraint conjunction differs from Crowhurst and Hewitt (1997) in that they claim that constraints can only be conjoined if they share the same focus (e.g. "foot" in the case INITIAL-FOOT and FINAL-FOOT). This cannot be a requirement for analogical macro-constraints, of course, thus forcing us to examine the basis upon which Crowhurst and Hewitt make their claim. Fortunately, this basis provides nothing to worry about, since it boils down to an "intuition that things could not sensibly be otherwise" (p. 13 in ms). If my analysis manages to justify itself empirically, this focus restriction will thus have to be rejected.

In any case, the fact that word *d* is the one that gets changed, and not the others, is still expressed by ranking IDENT-IO(*d*;G) below the analogical macro-constraint. A tableau illustrating this analysis is given below, this time including all sixteen possible lexicons.

(67)  {IDENT-IO($a$;F), IDENT-IO($b$;G), IDENT-IO($c$;F)}
   >> IDENT-OO($a,c$;F)∧IDENT-OO($b,d$;G) >> IDENT-IO($d$;G)

| [+F]$_a$, [+G]$_b$, [+F]$_c$, [-G]$_d$ | IDENT-IO($a$;F) | IDENT-IO($b$;G) | IDENT-IO($c$;F) | IDENT-OO($a,c$;F) ∧ IDENT-OO($b,d$;G) | IDENT-IO($d$;G) |
|---|---|---|---|---|---|
| ☞ [+F]$_a$, [+G]$_b$, [+F]$_c$, [+G]$_d$ | | | | | * |
| [+F]$_a$, [+G]$_b$, [+F]$_c$, [-G]$_d$ | | | | * | |
| [+F]$_a$, [+G]$_b$, [-F]$_c$, [+G]$_d$ | | | * | * | * |
| [+F]$_a$, [+G]$_b$, [-F]$_c$, [-G]$_d$ | | | * | * | |
| [+F]$_a$, [-G]$_b$, [+F]$_c$, [+G]$_d$ | | * | | * | * |
| [+F]$_a$, [-G]$_b$, [+F]$_c$, [-G]$_d$ | | * | | | |
| [+F]$_a$, [-G]$_b$, [-F]$_c$, [+G]$_d$ | | * | * | * | * |
| [+F]$_a$, [-G]$_b$, [-F]$_c$, [-G]$_d$ | | * | * | * | |
| [-F]$_a$, [+G]$_b$, [+F]$_c$, [+G]$_d$ | * | | | * | * |
| [-F]$_a$, [+G]$_b$, [+F]$_c$, [-G]$_d$ | * | | | * | |
| [-F]$_a$, [+G]$_b$, [-F]$_c$, [+G]$_d$ | * | | * | | * |
| [-F]$_a$, [+G]$_b$, [-F]$_c$, [-G]$_d$ | * | | * | * | |
| [-F]$_a$, [-G]$_b$, [+F]$_c$, [+G]$_d$ | * | * | | * | * |
| [-F]$_a$, [-G]$_b$, [+F]$_c$, [-G]$_d$ | * | * | | * | |
| [-F]$_a$, [-G]$_b$, [-F]$_c$, [+G]$_d$ | * | * | * | * | * |
| [-F]$_a$, [-G]$_b$, [-F]$_c$, [-G]$_d$ | * | * | * | | |

   To see that similarity between words $a$ and $c$ is crucial for this outcome, I provide another enormous tableau, this time under the assumption that the lexical representations of $a$ and $c$ are not phonologically similar (i.e. $a$=[+F] but $c$=[-F]).  Now the optimal lexicon is the one where nothing changes.

(68)    [same ranking as above]

| [+F]_a, [+G]_b, [-F]_c, [-G]_d | IDENT-IO(a;F) | IDENT-IO(b;G) | IDENT-IO(c;F) | IDENT-OO(a,c;F) ∧ IDENT-OO(b,d;G) | IDENT-IO(d;G) |
|---|---|---|---|---|---|
| [+F]_a, [+G]_b, [+F]_c, [+G]_d | | | * | | * |
| [+F]_a, [+G]_b, [+F]_c, [-G]_d | | | * | * | |
| [+F]_a, [+G]_b, [-F]_c, [+G]_d | | | | * | * |
| ☞ [+F]_a, [+G]_b, [-F]_c, [-G]_d | | | | * | |
| [+F]_a, [-G]_b, [+F]_c, [+G]_d | | * | * | * | * |
| [+F]_a, [-G]_b, [+F]_c, [-G]_d | | * | * | | |
| [+F]_a, [-G]_b, [-F]_c, [+G]_d | | * | | * | * |
| [+F]_a, [-G]_b, [-F]_c, [-G]_d | | * | | * | |
| [-F]_a, [+G]_b, [+F]_c, [+G]_d | * | | * | * | * |
| [-F]_a, [+G]_b, [+F]_c, [-G]_d | * | | * | * | |
| [-F]_a, [+G]_b, [-F]_c, [+G]_d | * | | | | * |
| [-F]_a, [+G]_b, [-F]_c, [-G]_d | * | | | * | |
| [-F]_a, [-G]_b, [+F]_c, [+G]_d | * | * | * | * | * |
| [-F]_a, [-G]_b, [+F]_c, [-G]_d | * | * | * | * | |
| [-F]_a, [-G]_b, [-F]_c, [+G]_d | * | * | | * | * |
| [-F]_a, [-G]_b, [-F]_c, [-G]_d | * | * | | | |

Before concluding this section, I should emphasize that the IDENT-OO constraints that are necessary for this analysis are not simply passively waiting for the chance to be conjoined before they can do anything. Lexical phonology can also be forced by non-conjoined IDENT-OO constraints acting alone, and this can give rise to what has been descriptively called morpheme structure constraints (MSCs) (though their idiosyncratic nature is more similar to Bybee and Slobin's 1982 concept of the schema). Of course, many apparent cases of MSCs have been shown to involve general markedness constraints (e.g. on syllable structure), and thus are not necessarily lexical. There are, however, constraints like those on English words containing the

string sCVC which truly appear to be lexical rather deriving from general markedness (Davis 1991).

As with lexical phonology generally, MSCs are often more lexically idiosyncratic than they may appear at first glance. Thus for example, Frisch (1996) has shown that in real lexicons, the Obligatory Contour Principle (OCP) does not behave as if imposed from above, like a universal markedness constraint, but rather OCP effects emerge from below, with the particular content of the lexicon, and differing frequencies of different patterns, playing critical roles. In my formalism, such effects could be described by the high ranking of OO-faithfulness constraints of the form IDENT-OO($a,b$;F), or if one property of a word influences another property of the same word, conjoined constraints of the form IDENT-OO($a,b$;F)∧IDENT-OO($a,b$;G). Details of the analysis would work quite similarly to the schematic example given above, but I will not pursue them here. By the way, it has been argued that MSCs should be handled by the same formal mechanisms that handle alternations (Kiparsky 1982), and that's precisely what my formalism does: a non-conjoined OO-constraint can affect both derived and nonderived words.

Moreover, often it is necessary for an analogical macro-constraint to be composed of more than two parochial OO-constraints. For example, a "weak" form may only be affected if one pair in the proportional analogy match in two phonological properties, not just one. Examples of such cases will be illustrated below. However, since the creation of OO∧OO-constraints occurs on a language-particular basis, it is costly, and the number of components in a macro-constraint will be naturally limited.

In short, then, it is possible to formalize analogy in OT without making any dramatic new additions to the devices already available in the literature.

### 3.5     Analogical macro-constraints and the power of numbers

One dramatic way in which my approach differs from previous work on lexical phonology is that I claim that general lexical patterns like velar softening are not represented anywhere in the grammar. Instead, all that a speaker knows is a set of pairs like *critic-criticism*, from which analogical macro-constraints may algorithmically, though only occasionally, be derived. By contrast, even word-based morphologists use rules, albeit redundant ones, to describe lexical knowledge that appears to hold of many words, not just a random few. My proposal thus may seem too weak to do anything, since it seems to merely restate the data: knowing *critic-criticism* means knowing *critic-criticism*, and there's not much else to say.

At the same time, my proposal may appear to be too powerful. Allowing IDENT-OO constraints to hold between all pairs of words in a lexicon creates an immense explosion in the predicted factorial typology. The number of IDENT-OO constraints will increase roughly as the square of the lexicon size (if a lexicon has $n$ items, there will be $n(n-1)/2$ pairings), although this problem might be relieved somewhat if the power of the Correspondence relation can be restricted (see earlier discussion). Conjunctive analogical macro-constraints only make things worse. For instance, as observed earlier, velar softening describes at least 320 alternations by a conservative count (J. Myers 1993). If every alternation were paired with every other in separate analogical macro-constraints, that would potentially be 51,040 such constraints floating around somewhere in the grammar of English. Allowing all of them to be extrinsically ranked

with respect to each other would predict 51,040! alternative Englishes (the "!" represents both shock and the factorial operation $n!=n(n-1)(n-2)...$). This is surely significantly more than the number of times somebody has said something is more numerous than all the particles in the universe.

Interestingly, the above two problems are closely related, and one solves the other: the factorial explosion actually explains how a set of exemplar-driven analogies can end up having far-reaching consequences across an entire lexicon. In essence, the argument runs as follows. Because of extrinsic ranking of the IDENT-IO constraints, most of the potential IDENT-OO constraints don't do anything, which means they can't be learned, posited, or ranked by acquirers, and so we can forget about them. Only when at least one IDENT-OO constraint is ranked above a relevant IDENT-IO constraint will there be any effect, and the likelihood of this happening is directly related to the number of relevant IDENT-OO constraints that can be posited. This in turn means that the more similarities there already are in the lexicon (e.g. pairs showing velar softening alternations), the more rankings there will be where IDENT-OO constraints can actually act to spread these similarities.

Let me take these points again a bit more slowly by going through a schematic example. Suppose we have two language families L1 and L2, each containing only six words. The language families differ slightly in the contents of their lexicons, as indicated below: L1 has only one pair showing a +F/+G relation (*a-b*), while L2 has two (*a-b* and *e-f*).

(69)   L1:     $a = [+F]$          $c = [+F]$          $e = [+F]$
                   $b = [+G]$         $d = [-G]$         $f = \textbf{[-G]}$

        L2:     $a = [+F]$          $c = [+F]$          $e = [+F]$
                   $b = [+G]$         $d = [-G]$         $f = \textbf{[+G]}$

Suppose further that as before, word *d* is "weak," meaning that if any of the six is changed by analogy, it should be *d*. The goals are to show that the factorial typology within each language family is restricted in practice, and also that in L2, which contains the same analogical pattern twice (i.e. *a:b::c:d* and *e:f::c:d*), *d* will be more likely to change than in L1.

If my formal devices are given complete freedom, the generation of grammars will proceed as follows. Each language family will have six IDENT-IO constraints parameterized to the relevant features, which I will abbreviate as IO-*a*, IO-*b*, ..., IO-*f*. To focus the discussion, I set aside the OO constraints that hold between words in different rows in the above figure (i.e. the F row and the G row); a constraint of the form IDENT-OO(*a,b*;G) will be difficult to interpret in these schematic language families given that I've given the value of property G for word *b* but not for word *a*. This brings the number of relevant OO constraints down to 6 (=3 combinations per row × 2 rows). This number in turn means that each language family can potentially have up to 15 (=6×5/2) analogical macro-constraints, which I will abbreviate as *ac-ae, ac-ce, ..., bf-df*, bringing the grand total of constraints for each language to 27 (=6+6+15). The number of possible constraint hierarchies is thus approximately $1.09 \times 10^{28}$ (=27!), quite a large number, but not anything that OT isn't used to. Finally, to simplify the discussion, let's agree that neither L1

nor L2 has simple MSCs. That is, all IO constraints are ranked higher than all non-conjoined OO constraints. Now we can begin to explore the consequences.

First, since by hypothesis *d* is "weak," IO-*d* must be ranked below the IO constraints for the other five words. As for the OO∧OO constraints, we have already seen that they only have an effect when ranked directly above the IO constraint of the lowest ranked word. Thus there's no point considering OO∧OO constraints that don't make reference to *d*. This leaves us with only the following nine OO∧OO constraints:

(70)  *bd-ab*      *bd-ac*      *bd-bc*
      *df-ab*      *df-ac*      *df-bc*
      *df-bf*      *df-bd*      *bd-bf*

Now let us compare the typological patterns that emerge in the L1 and L2 language families if these nine constraints are allowed to be freely ranked with respect to each other and IO-*d*. We obviously can't list all possible rankings, since there are $10! = 3,628,800$ of them, but we don't have to. Anttila (1997) demonstrates with many examples that if one is comparing only two possible outcomes (as we are, namely *d*=[-G] or *d*=[+G]), then the probability of the desired outcome (here, *d*=[+G]) can be found in accordance with the following algorithm (a formal proof is left to the interested student).

(71)  $c$ = number of freely ranked constraints
      $n$ = number of constraints violated by desired candidate X
      $m$ = total number of constraint violations by both candidates
      $p$ = proportion of rankings in which X is optimal

      If $n = c$, then $p = 0$.
      If $n < c$, then $p = 1-n/m$.

To make this algorithm intuitive, examine the following tableau, and imagine permuting the three constraints $C_1$, $C_2$, $C_3$ in all 6 (=3!) possible ways. Candidate $X_1$ is optimal under two of the six rankings, i.e. in a proportion of 1/3, just as the above formula predicts.

(72)

| X | C1 | C2 | C3 |
|---|----|----|----|
| $X_1$ | * | | * |
| $X_2$ | | * | |

To indicate how our ten freely ranked constraints judge the crucial distinction (*d*=[-G] vs. *d*=[+G]) in the the two language families, I give the following schematic tableaux. Lexical representations for words *a*, *c*, and *e* are all [+F] in both language families, and *d* is [-G]. The only difference between L1 and L2 is the representation of word *f*, which is [-G] in L1 and [+G] in L2.

Thus for the L1 family, the tableau schema is as shown below. This implies that given free ranking of these ten constraints, 46% (=6/13=1-7/13) of the possible languages in the L1 family will have the surface form *d*=[+G]. That is, a language in L1 will show an analogical effect less often than expected purely by chance, meaning that the analogy is "suppressed" by the initial lexical representation of *d*.

(73)　L1:　　*b*=[+G], *d*=[-G], *f*=[-G]

| | *bd* *ac* | *bd* *ae* | *bd* *ce* | *df* *ac* | *df* *ae* | *df* *ce* | *bd* *bf* | *bd* *df* | *df* *bf* | IO-*d* |
|---|---|---|---|---|---|---|---|---|---|---|
| *d*=[+G] | | | | * | * | * | * | * | * | * |
| *d*=[-G] | * | * | * | | | | * | * | * | |

By contrast, in the L2 family, applying the algorithm to the following tableau schema results in a proportion of 90% (=9/10=1-1/10) languages where *d*=[+G]. Though the specific numbers are an artifact of the many simplifying assumptions we made, the important thing is that the proportion for L2 is far larger than that for L1, as desired.

(74)　L2:　　*b*=[+G], *d*=[-G], *f*=[+G]

| | *bd* *ac* | *bd* *ae* | *bd* *ce* | *df* *ac* | *df* *ae* | *df* *ce* | *bd* *bf* | *bd* *df* | *df* *bf* | IO-*d* |
|---|---|---|---|---|---|---|---|---|---|---|
| *d*=[+G] | | | | | | | | | | * |
| *d*=[-G] | * | * | * | * | * | * | * | * | * | |

This result seems likely to generalize to larger lexicons, variations in number of items showing an analogical pattern, number of competing analogical patterns, inclusion of non-conjoined OO-constraints, inclusion of OO-constraints parameterized to other features, and so on (more work for the interested student). The lesson is that even under the limiting assumption that the generation of analogical macro-constraints is completely free, a "weak" word is more likely to be affected by patterns that are more widely attested across the lexicon. Thus it is indeed possible to express the intuition that the "strength" of a lexical phonological pattern is positively correlated with the number of lexical items that conform to the pattern, without ever explicitly representing the pattern itself anywhere in the grammar.

## 3.6　　Applications

To illustrate my formalism with real data, I will focus on four cases discussed in section 2: [ay]-[o] vs. [ay]-[ɪ] alternations in irregular inflection, the interaction of irregular inflection and the Scottish Vowel Length Rule, exceptions to vowel shortening, and the interaction of s-voicing with other patterns.

### 3.6.1   Irregular inflection

The basic insight that I want to capture in irregular inflection is this:   vowel alternation patterns such as [ay]-[o] are not maintained because of a general rule that is instantiated in pairs like *drive-drove*, but rather because of pairs like *drive-drove* themselves.

The analogy *drive:drove::dive:X* would be handled with the following ranking of OO∧OO-constraint and IO-constraint.  Note that as usual, the italicized words represent words, not their phonological forms; thus "*dove*" represents "past form of *dive*".  Note also that the OO∧OO-constraint here contains three components, in order to describe the fact that it is not the vowel or consonant alone that make *drive* and *dive* similar, but rather both.

(75)   a.       IDENT-OO(*drive,dive*;[ay]) ∧ IDENT-OO(*drive,dive*;[v])
                 ∧ IDENT-OO(*drove,dove*;[o])

       b.       IDENT-IO(*dove*;vowel features)

       c.       OO∧OO-constraint >> IO-*dove*

A tableau showing this ranking in action is given below.  I've abbreviated the name of the analogical macro-constraint to save space; this convention is followed throughout all of the following examples.  Moreover, since I assume in all of the following examples that the IO-constraint for the "weak" word is ranked below those for all other words, I have left out IO-constraints in most of the following analyses, and have included only the candidate lexicons where the "weak" word(s) can appear in alternate forms (bolded).  These simplifications mean that there is no need to give the inputs in their usual location in the upper-left corner of the tableaux.

(76)

|  | OO-(*drive,dive*;ay)∧(*drive,dive*;v)∧(*drove,dove*;o) |
|---|---|
| [drayv]<br>[drov]<br>[dayv]<br>**[dayv]** | * |
| [drayv]<br>[drov]<br>[dayv]<br>☞    **[dov]** |  |

In dialects where the past tense of *dive* already is *dove,* the above analogy has no effect.  Likewise, in dialects where the past tense of *dive* is *dived*, the past tense form of *dive* is not found in the lexicon at all, or at least is not easily accessible, given the extremely high

productivity of regular past tense morphology. Hence no parochial constraint referring to *dove* will exist, and analogy will usually not occur either.

However, the psychological reality of such analogies can occasionally reveal itself with children unclear on what is in the lexicon and what is not, and even with adults who are forced under experimental conditions to pronounce or make judgments on nonce forms. For example, in Prasada and Pinker (1993), subjects were asked to judge or produce past tense forms for nonsense forms like *spling*. In one task, a significant proportion of productions included *splung*, thus following the typical [ɪŋ]-[ʌŋ] pattern, exemplified in many real pairs like *cling-clung*. Prasada and Pinker (1993) didn't include any nonce forms with [ay], but we can safely assume that people given a form like *thive* would occasionally produce *thove* as a past tense. In my analysis, this would have involved positing the following ad hoc OO∧OO-constraint.

(77)    IDENT-OO(*drive,thive*;[ay]) ∧ IDENT-OO(*drive,thive*;[v])
        ∧ IDENT-OO(*drove,thive-PAST*;[o])

Analogy is not guaranteed to occur. If the speaker happened not to have posited any of the component OO-constraints, or didn't happen to conjoin them into a macro-constraint, the output *thived* would be produced instead. This is typical of analogy. Even in Prasada and Pinker's (1993) study, *spling* induced the production of *splung* only 33% of the time, and Xu and Pinker (1995) found that children extend patterns in irregular inflection less commonly than had been thought. This may not be surprising for those who doubt that the patterns in irregular inflection are phonological at all, but the same holds even for rather robust lexical patterns like velar softening. For instance, Ohala (1974) found that speakers only applied velar softening in novel forms like *toxicism* in about a quarter of their responses. This result is entirely expected in my approach, since the creation of the necessary analogical macro-constraints is not done automatically, but rather is a costly business carried out on a word-by-word basis.

The analogical macro-constraints enforcing the [ay]-[o] alternation are not the only ones active in the irregular verbs, of course, and in fact a word may be targeted by contradictory analogies at the same time. For example, *smite* is similar both to *write* (*wrote*) and *light* (*lit*). The fact that the past tense form of *smite* is *smote* rather than \**smit* is due to the fact that *smote* is actually listed in the lexicon and/or to the fact that the OO∧OO-constraint for the analogy *write-smite* is extrinsically ranked higher than the OO∧OO-constraint for the analogy *light-smite*, as shown below.

(78)    IDENT-OO(*write,smite*;[ay]) ∧ IDENT-OO(*write,smite*;[t])
        ∧ IDENT-OO(*wrote,smite-PAST*;[o])
        >>
        IDENT-OO(*light,smite*;[ay]) ∧ IDENT-OO(*light,smite*;[t])
        ∧ IDENT-OO(*light,smite-PAST*;[ɪ])

(79)

| | OO-<br>(*write,smite*;ay)<br>∧<br>(*write,smite*;t)<br>∧<br>(*wrote,smote*;o) | OO-<br>(*light,smite*;ay)<br>∧<br>(*light,smite*;t)<br>∧<br>(*light,smote*;ɪ) |
|---|:---:|:---:|
| [rayt]<br>[rot]<br>[layt]<br>[lɪt]<br>[smayt]<br>**[smɪt]** | * | |
| [rayt]<br>[rot]<br>[layt]<br>[lɪt]<br>[smayt]<br>☞   **[smot]** | | * |

As with all extrinsic rankings, there needn't be any particular reason for the ranking of these analogical macro-constraints; it's simply part of one's knowledge of English grammar. Thus one important aspect of analogy's "arbitrariness" can be handled simply by using an "arbitrary" device already necessary in OT. Shortly, however, we will see that the ranking of analogical macro-constraints sometimes may follow general principles as well.

### 3.6.2   Irregular inflection and the Scottish Vowel Length Rule

Another important case to examine is the blocking of SVLR in the stem forms of a specific class of irregular verbs in Glenoe. As I showed in section 2, this stumps all rule-based analyses, but it can be handled quite naturally in my formalism.

Essentially the analysis builds on the insight of Gregg (1958, 1959, 1973, 1985) that SVLR is blocked in *drive*, which contains short [ay] rather than long [ay:], by proportional analogies of the form *rode:ride::drove:drive*, where *ride* contains a short vowel [ay] since it precedes a stop. This blocking analogy be expressed in my formalism in the following OO∧OO-constraint.

(80)    IDENT-OO(*rode,drove*;[o]) ∧ IDENT-OO(*rode,drove*;[v])
    ∧ IDENT-OO(*ride,drive*;[ay])

However, SVLR should itself viewed as an analogy, playing a role in two-part proportions such as *five:drive*, as expressed in the following OO∧OO-constraint. The word *drive* is thus under pressure from two competing analogies, one pushing it to have a short vowel like *ride*, and the other to have a long vowel like *five*.

(81)    IDENT-OO(*five,drive*;[v]) ∧ IDENT-OO(*five,drive*;[ay:])

The conflict could be resolved in many ways. Perhaps the IO-constraint for *drive* will outrank both of these OO∧OO-constraints; in this case, nothing happens. Perhaps the *five-drive* constraint will outrank the *ride-drive* constraint; in this case *drive* obeys the SVLR, as in most varieties of Scottish English. In Glenoe, however, the *ride-drive* constraint happens to outrank the *five-drive* constraint and others like it. What then happens is illustrated in the following tableau.

(82)

|  | OO-<br>(*rode,drove*;o)<br>∧<br>(*rode,dive*;v)<br>∧<br>(*ride,drive*;ay) | OO-<br>(*five,drive*;v)<br>∧<br>(*five,drive*;ay:) |
|---|:---:|:---:|
| [fay:v]<br>[rayd]<br>[rod]<br>**[dray:v]**<br>[drov] | * |  |
| [fay:v]<br>[rayd]<br>[rod]<br>☞ **[drayv]**<br>[drov] |  | * |

Of course, the short vowel in *ride* that drives the shortening of *drive* is itself driven by analogies, e.g. with *tide*. A more complete analysis would thus look something as in the following tableau.

(83)

| | OO-<br>(*tide,ride*;d)<br>∧<br>(*tide,ride*;ay) | OO-<br>(*rode,drove*;o)<br>∧<br>(*ride,drive*;ay) | OO-<br>(*five,drive*;v)<br>∧<br>(*five,drive*;ay:) |
|---|:---:|:---:|:---:|
| [tayd]<br>[fay:v]<br>**[ray:d]**<br>[rod]<br>**[dray:v]**<br>[drov] | * | | |
| [tayd]<br>[fay:v]<br>**[ray:d]**<br>[rod]<br>**[drayv]**<br>[drov] | * | * | * |
| [tayd]<br>[fay:v]<br>**[rayd]**<br>[rod]<br>**[dray:v]**<br>[drov] | | * | |
| [tayd]<br>[fay:v]<br>**[rayd]**<br>☞ [rod]<br>**[drayv]**<br>[drov] | | | * |

Now recall the dialect variation in the past tense form of *strive*, where speakers who have regularized this verb also apply SVLR to it. This results from the fact that regular verbs do not have past tense forms stored in the lexicon (at least not readily accessible). Hence such speakers cannot posit analogical macro-constraints that contain the past tense form of *strive*. With such constraints absent, the analogical macro-constraints affecting the present tense forms alone, such as IDENT-OO(*five,strive*;[v]) ∧ IDENT-OO(*five,strive*;[ay:]), hold sway, and only SVLR is obeyed, as shown in the following tableau.

(84)     Lexical assumption: *strove* not in lexicon

|  | OO-<br>(*five,strive*;v)<br>∧<br>(*five,strive*;ay:) |
|---|---|
| [fay:v]<br>[rayd]<br>[rod]<br>☞ **[stray:v]** |  |
| [fay:v]<br>[rayd]<br>[rod]<br>**[strayv]** | * |

One quirk of the Glenoe pattern, the nondirectional nature of the influence of the past form on the present form, poses no special problem for my analysis. However, it must be admitted that "backwards" analogies like this are not the typical case. Chapman (1995), noting the existence of "backwards" analogy in some Swiss German dialects, cites Tiersma's (1982) notion of "local markedness," which here means that a derived form may occasionally trigger analogy in a base form if the derived form is of higher lexical frequency. If parochial constraints are ranked in order of lexical frequency, with those for higher frequency words first (see below for evidence), this would have the effect of normally ranking derived forms lower, thus making them "weaker" (i.e. more subject to analogy-induced changes). "Backwards" analogy, as in Glenoe, is relatively less common because it can only occur if the appropriate analogical macro-constraints are ranked in violation of this general frequency trend, or if the derived forms are significantly more frequent than the bases, contrary to the usual case.

### 3.6.3   Exceptions to shortening

The analogical analysis of vowel alternations works basically the same way as in the previous cases, except that far more pairwise comparisons would be involved. Here I will focus just on one aspect of the complex overlapping patterns, namely the exceptions to closed-syllable shortening found in monomorphemic words like *council*.

This situation can be conceived of as essentially identical to the exceptions to SVLR in irregular inflection: two analogies compete for control of the vowel in *council*, and only one of them wins. The details are given in the following figures.

(85)    Competing schemas targeting *council*:

    a.    Vowel shortening:    IDENT-OO(*country,council*;[nC])
                                    ∧ IDENT-OO(*country,ancient*;[ʌ])

    b.    No shortening:    IDENT-OO(*found,council*;[nC])
                                      ∧ IDENT-OO(*found,council*;[aw])

    c.    OO∧OO-(b) >> OO∧OO-(a)

(86)

|  | OO-<br>(*found,council*;[nC])<br>∧<br>(*found,council*;[aw]) | OO-<br>(*country,council*;[nC])<br>∧<br>(*country,council*;[ʌ]) |
|---|---|---|
| [fawnd]<br>[kʌntri]<br>☞ **[kawnsəl]** |  | * |
| [fawnd]<br>[kʌntri]<br>**[kʌnsəl]** | * |  |

       Similar tableaux can be given to describe the other analogical properties of the vowel alternations, for example the nondirectional extension of the short-vowel+t/d pattern from semi-weak past forms like *met* to stem forms like *bet*.

### 3.6.4   Interactions with s-voicing

       The interactions of s-voicing with other lexical patterns also involve ranking analogical macro-constraints, but this time the rankings seem to follow general principles rather than merely being stipulated.  I first look at the overapplication of s-voicing in *equate-equation*, and then at the interaction between s-voicing and velar softening.

       Why is it *equation* that conforms to the *evasion* analogy rather than *relation*?  To some extent this is arbitrary, but there is an important difference between *equation* and *relation* that may provide a partial explanation: *equation* has a much lower lexical frequency than *relation*.  If parochial IO-constraints are ranked in order of decreasing frequency, as I have suggested above (and more evidence is to come), then less-common words like *equation* are expected to be "weaker" (i.e. more subject to analogies).  Phillips (1984) notes that precisely this seems to be the case diachronically:  less frequent words are more subject to analogical change.  The competition between the *evasion-equation* and *relation-equation* analogies is thus more likely to

be resolved by changing *equation* than by changing *relation*. This is illustrated in the following tableau, where the ranking of IO-*relation* >> IO-*equation* is intrinsic rather than stipulated.

(87)  IO-*relation*          [higher frequency]
   >> {OO-(*evasion,relation*;[ʒ]), OO-(*evasion,equation*;[ʒ])}
   >> IO-*equation*          [lower frequency]

| eva[ʒ]ion **rela[ʃ]ion** **equa[ʃ]ion** | IO-*relation* | OO- (*evasion, relation*;[ʒ]) | OO- (*evasion, equation*;[ʒ]) | IO-*equation* |
|---|---|---|---|---|
| eva[ʒ]ion **rela[ʒ]ion** **equa[ʒ]ion** | * | | | * |
| eva[ʒ]ion **rela[ʒ]ion** **equa[ʃ]ion** | * | | * | |
| eva[ʒ]ion ☞ **rela[ʃ]ion** **equa[ʒ]ion** | | * | | * |
| eva[ʒ]ion **rela[ʃ]ion** **equa[ʃ]ion** | | * | * | |

The interaction between s-voicing and velar softening also involves a general ranking principle, but one of a somewhat different sort. To show this, below I first give the constraints and ranking relevant to an analysis of the word *Grecian*.

(88)  Competing schemes targeting *Grecian*:

   a.   s-voicing:                    IDENT-OO(*Parisian,Grecian*;V_V)
                                      ∧ IDENT-OO(*Parisian,Grecian*;[ʒ])

   b.   velar softening:              IDENT-OO(*Phoenician,Grecian*;[ən])
                                      ∧ IDENT-OO(*Phoenician,Grecian*;[ʃ])

   c.   OO∧OO-(b) >> OO∧OO-(a)

(89)

| | OO-<br>(*Phoenician,Grecian*;[ən])<br>∧<br>(*Phoenician,Grecian*;[ʃ]) | OO-<br>(*Parisian,Grecian*;V_V)<br>∧<br>(*Parisian,Grecian*;[ʒ]) |
|---|---|---|
| [pəri:ʒən]<br>[fəni:ʃən]<br>**[gri:kən]** | * | * |
| [pəri:ʒən]<br>[fəni:ʃən]<br>**[gri:ʒən]** | * | |
| [pəri:ʒən]<br>[fəni:ʃən]<br>☞ **[gri:ʃən]** | | * |

As with the previous cases, this ranking is in principle reversible. It is not inconceivable that some future generation will begin pronouncing *Grecian* with a voiced rather than voiceless consonant by analogy with words like *Parisian*, just as *equation* has come to have a voiced consonant by analogy with words like *evasion*. It seems plausible to suppose, however, that the ranking in the above tableau is more natural than the reverse. As I showed earlier, the sheer number of analogical macro-constraints with a given pattern has a strong effect on the likelihood that the pattern will win a competition with another. Since there are far more words showing velar softening than s-voicing, *Grecian* will appear in far more analogical macro-constraints with words like *Phoenician* than with words like *Parisian*. If such macro-constraints are created and ranked basically at random (subject to factors like lexical frequency), it will be much more likely that a velar softening OO∧OO-constraint targeting *Grecian* will outrank an s-voicing OO∧OO-constraint targeting *Grecian* than the other way around. My formalism thus explains why analogical strength tends to correlate with how robustly a given pattern is attested in the lexicon.

### 3.7 Formalism: summary

My aim in section 2 was to show that analogy is an empirically attested phenomenon. In section 3, I have shown how it may be formalized in OT. To do this, I have used no assumptions or devices that are not already found somewhere in the OT literature. The three most important contributions from OT that make my analysis possible are the extrinsic ranking of violable constraints, output-output correspondence, and parochial constraints (conjoined constraints just make the formalism more direct). The result is a formalism that handles analogy essentially the same way as in a constraint-satisfaction connectionist network: all words are trying to become identical to all others, but due to differences in constraint strength (similar to connection weights in connectionism), some words have more influence than others. There are surely important differences in how my formalism behaves relative to genuine connectionism, especially in quantitative predictions, but these aren't my main interest. The central achievement

of this section, to my mind, is that I have shown that it is possible to take advantage of the special qualities of OT, found in its connectionist forebears but ignored in the OT literature itself, which allow for a view to the lexicon that is empirically (and psycholinguistically) far more accurate than was ever possible in derivational theories.

## 4. Lexical phonology and markedness constraints

In spite of its successes, my approach to lexical phonology may seem to have a very serious flaw, a flaw that it shares with any approach treating lexical phonology as qualitatively different from "true" phonology (e.g. Hooper 1976; Stampe 1973/1979; Ohala 1990). Namely, it doesn't explain why lexical phonological patterns often (perhaps even usually) look a lot like "true" phonology, involving phonetic features, local assimilation, and so forth. After all, such similarities are what led the early generativists (e.g. Halle 1962, Chomsky and Halle 1968) to give up the systematic phonemic level as theoretically useless. In an OT framework, the objection can be expressed in terms of constraint types: my approach to lexical phonology relies solely on faithfulness constraints, with markedness constraints playing no role whatsoever.

The objection has a compelling surface plausibility. For example, lexical palatalization in English, which among other things changes /s/ to /ʃ/ before /y/ (e.g. *confe[s]-confe[ʃ]ion*), seems essentially the same pattern as the postlexical palatalization that applies across word boundaries (e.g. *confe[ʃ] your sins*). Ignoring these sorts of observations allows for the possiblity of wide varieties of "unnatural" lexical patterns that are in fact unattested, extremely rare or highly restricted. My approach is thus not explanatory in two senses: (a) it doesn't provide a constrained description of phonological patterns cross-linguistically, and (b) it ignores what Chomsky and Halle (1968) called the "intrinsic content" (i.e. phonetic motivation) of lexical phonology.

In this section I respond to these objections, and in the course of doing so, provide empirical evidence supporting my markedness-constraint-free approach to lexical phonology. Moreover, I show that the explanation for unmarkedness in the lexicon cannot be given within a competence model, but instead we must include performance factors, in particular acquisition, in a larger, modular theory of phonology.

## 4.1 Lexical markedness constraints

The first comment I need to make concerning markedness and the lexicon is that I don't rule out the possiblity of general markedness constraints that only apply within the lexicon. Such constraints would be motivated by universally valid cognitive principles of lexical access and storage. The OCP (perhaps exploded into parochial constraints) is probably an example, both because Frisch's (1996) lexically-driven OCP model works so well, and because, as Kiparsky (1986) observed, dissimilation processes always seem to be lexical, never postlexical. Shortly, in fact, I will have occasion to use an OCP constraint directly within a lexical phonology analysis.

What I deny is the lexicon-internal role of constraints motivated by lexicon-external factors. Putting my claim this way makes the study of OT constraints very interesting. For

instance, nobody now knows whether NOCODA is motivated by lexical access factors or by motor control factors. If it proves to be the latter, I predict that it cannot play a direct role in lexical phonology (appearances to the contrary); if the former, then it can. Thus "internal" data on phonological competence could help teach us something about how phonology is processed. In the following sections I will give examples of the kind of evidence that could address such questions.

## 4.2  Lexicon-external factors and lexical phonology

Evidence that has been known for a long time, but whose dangerous consequences tend to be neglected, strongly suggests that phonetically motivated markedness constraints cannot play a direct role in lexical phonology, just as my analysis implies. Lexical phonology differs from phonetics (whether universal or language-particular) in being categorical, i.e. describable with combinations of a finite set of discrete units rather than with continuous, gradient scales (see e.g. Cohn 1990; Fowler 1992; Keating 1985, 1990; Zsiga 1993). For example, using a variety of phonetic measures, Zsiga (1993) demonstrated that lexical palatalization in English completely neutralizes "underlying" /ʃ/ (e.g. *mesh*) and "derived" /ʃ/ (e.g. *confession*), while this is not the case with postlexical palatalization (e.g. *mesh* vs. *confess your*). This phenomenon goes beyond mere violations of structure preservation in the postlexical phonology, which in principle could be represented categorically (see e.g. Kiparsky's 1985 discussion of nasal stops in Catalan), since the postlexical palatal in English is simply not a categorical entity: it begins /s/-like and ends /ʃ/ -like, as if it were derived through gestural overlap in the articulation (e.g. Browman and Goldstein 1992). The lexical /ʃ/ in words like *mesh* and *confession*, by contrast, appears to be an articulatory target specified in the lexicon. Thus lexical and postlexical palatalization may look superficially similar, but their grammatical description must be completely different. Zsiga (1993), for instance, describes lexical palatalization as autosegmental spreading of discrete distinctive features, and postlexical palatalization as gradient coarticulation.

Thus it appears that lexical phonology can always be described with categorical units, while postlexical phonology or phonetics cannot. This is consistent with the analogical approach to lexical phonology, since from Aristotle to modern connectionists, it has been recognized that analogy cannot be carried out except with the use of some feature system (otherwise the notion of "similarity" is impossible to define). I thus predict rather than stipulate lexical categoricality.

I hasten to point out, however, that I don't require that these lexical features come from a small universal set. It has been shown that lexical phonology can involve derived distinctive features (Tsay 1994) or even phonetic properties that are universally nondistinctive (e.g. Flemming 1995, Hayes 1995, Jun 1995, Kirchner 1995, Silverman 1996, Steriade 1996; Bybee 1996). Yet it has not been found that lexical patterns cannot be described categorically with phonetic features of some sort, even if they are ad hoc. If lexical representations are actually non-categorical in nature, which seems likely, it is still possible to derive ad hoc lexical features when needed, as Kirchner (1997) has demonstrated within an OT formalism.

The upshot of this is that no theory of lexical phonology can be explanatory in both senses (a) and (b) given at the beginning of section 4, i.e. by describing "cross-linguistically common and well-established processes" solely in terms of "very simple combinations of the

descriptive parameters of the model" (McCarthy 1988:84), which are themselves motivated by "natural" factors such as phonetics. This is because the formalism for lexical phonology must be categorical, and this means that there is no way to encode gradient phonetic motivations directly into the formalism (see McCawley 1973/1979 for an early recognition of this). For example, the phonetically motivated categorical notation of autosegmental phonology is guaranteed to fail with lexical patterns motivated by factors other than coarticulation (encoded as spreading), and there are many such factors to choose from. Vowel shortening, for instance, appears to be the phonetic motivation for the familiar (and phonetically categorical) phenomenon of Canadian Raising, and yet shortening cannot be given a causal role in the grammar unless we falsely claim that it is also categorical (J. Myers 1997).

How, then, can we account for the strong similarities between lexical and postlexical patterns, such as English palatalization? The logical response to this dilemma has always been available, namely to view the theory of phonological competence as just one module in a theory of phonological description that also includes modules for articulatory physiology, speech perception, language acquisition, history, and so on (see e.g. S. Anderson 1981). For example, a reasonable explanation for why velar softening is systematic in English is that it was systematic in Latin, and it was systematic in Latin because the articulatory physics that was ultimately responsible for it was systematic. If Hale and Reiss (1998) are right, there is a natural explanation for why history is so important to lexical phonology: language acquirers begin by ranking all faithfulness constraints at the top, thus simply memorizing the adult surface forms in all of their pedigreed glory.

Some phonologists may be conditioned to feel that such a modular approach is passing the buck, but it's not. First, as I have argued, we have no choice. Second, if we remember that the primary goal of generative linguistics is to describe the language-specific knowledge of individual speakers in terms of innate knowledge (Chomsky 1965), then my proposal does exactly that: knowledge of lexical phonology results from an innate analogical mechanism interacting with a specific lexicon, which is itself expressed via innate constraint families. Why should we expect the competence model to explain any more without any outside help?

## 4.3  Markedness and lexical frequency

The previous section reviewed familiar arguments for a modular approach to unmarkedness in lexical phonology. In this section I'd like to provide a relatively unfamiliar one involving lexical frequency. I find this argument particularly interesting, since it seems to provide the beginnings of a formal proof both for the modular theory of lexical phonology and for the relevance of language acquisition in this modular theory.

Consider the relation of lexical frequency to the ranking of parochial and markedness constraints. As noted earlier, many scholars have noted the effect of frequency in lexical phonology (Bybee 1996; Fidelholtz 1975; Frisch 1996; Hammond 1997; Kaisse 1985; Myers and Guy 1997; Phillips 1984). In a sense, this phenomenon just represents a special case of the well-known penchant of lexical phonology to have exceptions, except here the exceptionality is gradient and correlated with frequency. Understanding this phenomenon, however, requires deep changes in the understanding of how markedness is represented in lexical phonology.

Hammond (1997) provides an interesting OT analysis of such a case, namely the English rhythm rule (see e.g. Hayes 1984). The finding he wishes to describe is the following: in an informal experiment he conducted, the likelihood of a native speaker obeying the English rhythm rule was positively correlated with the lexical frequency of the target word. Higher frequency words like *antíque* tended to shift stress under phrasal stress clash, i.e. *ántique bóok*, while lower frequency words like *arcáne* tended not to, i.e. *arcáne sórt*. Hammond first points out that such a systematic pattern should be handled in the grammar, and yet due to its reference to arbitrary properties of a specific lexicon, the only way to do this is with parochial constraints. To get the particular effect of frequency, he ranks the parochial constraints associated with lower frequency words above the universal markedness constraint *CLASH, and higher frequency words below it. Using my conventions, his analysis would appear as in the following tableau (note that in selecting an optimal lexicon, non-parochial constraints gradiently accumulate violations word by word).

(90)    [after Hammond (1997)]       IO-rare >> *CLASH >> IO-common

| antíque bóok<br>arcáne sórt | IDENT-IO(*arcane*,stress) | *CLASH | IDENT-IO(*antique*,stress) |
|---|---|---|---|
| antíque bóok<br>arcáne sórt | | *<br>* | |
| antíque bóok<br>árcane sórt | * | * | |
| ☞ ántique bóok<br>arcáne sórt | | * | * |
| ántique bóok<br>árcane sórt | * | | * |

There is a fatal flaw in this analysis, however: the parochial constraints are ranked from lowest to highest frequency. This cannot possibly be right. Instead, as we have already seen, if parochial IO-constraints are ranked by frequency, they should be ranked from highest to lowest. The evidence for this came from two observations: analogies tend to target derived forms rather than base forms (which are usually more frequent), and analogies tend to target lower-frequency words like *equation* more often than higher-frequency words like *relation*. In my formalism, these observations require a ranking of IO-constraints opposite to that of Hammond (1997).

But the arguments against Hammond's analysis are not merely theory-internal, but also conceptual and psycholinguistic. Conceptually, note that only the ranking at the top of a constraint hierarchy is well-defined; as one moves towards the bottom, the effects of alternate rankings are swamped by ever more competing constraints. The frequency hierarchy of words in a lexicon is also only well-defined at the top: every language has a most frequent word, but none has a least frequent word, given morphological productivity, recent borrowings, spontaneous coinages, speech errors, and so forth.

Hammond's analysis is also problematic in psycholinguistic terms. His claim is that rare words, which have repeatedly been shown to be harder to access from the lexicon than common

words (e.g. Forster and Chambers 1973; Whaley 1978), somehow have a greater power to resist lexicon-external markedness forces. If anything, we should expect exactly the opposite to be the case.

There is also an empirical problem with Hammond's approach: frequency effects sometimes interact with markedness in precisely the opposite way found with the rhythm rule. For example, Phillips (1984) reports that in a study of native English speakers of Georgia (USA), prevocalic /y/ was more likely to be missing after alveolars in lower frequency words like *nude* (y-less rate of 74.4%) than in higher frequency words like *new* (y-less rate of 43.0%). In OT terms, the relevant markedness constraint would be some variety of the OCP, say *COR-COR. This can be easily handled in an analysis like that used by Hammond, but only if we reverse the ranking of the parochial IO-constraints.

(91)    IO-common >> *COR-COR >> IO-rare

| [nyu], [nyud] | IDENT-IO(*new*,y) | *COR-COR | IDENT-IO(*nude*,y) |
|---|---|---|---|
| [nyu] [nyud] | | * * | |
| ☞ [nyu] [nud] | | * | * |
| [nu] [nyud] | * | * | |
| [nu] [nud] | * | | * |

This is not to say that Hammond's observations are a fluke. Frequency and unmarkedness correlate positively very often, and indeed this seems to be the more common kind of correlation: Bybee (1996), Fidelholtz (1975), Kaisse (1985), Myers and Guy (1997) and Phillips (1984) all describe cases where phonetic markedness factors affect higher frequency words more readily than lower frequency words. Fidelholtz (1975), for instance, shows that English vowel reduction is found more often in higher frequency words like *astronomy* and *mistake* than in lower frequency words like *gastronomy* and *mistook*.

In an attempt to harmonize such conflicting patterns of frequency-markedness correlations, Phillips (1984) suggests that generalizations that affect higher frequency words more often than lower frequency words (e.g. the rhythm rule) are always motivated by physiology, while generalizations that affect lower frequency words more (e.g. the distribution of [yu] vs. [u]) are motivated by non-physiological factors acting on "underlying forms" (i.e. lexical representations). In my terms, cases like the rhythm rule involve lexicon-external markedness constraints, while cases like [yu]-[u] involve lexical markedness constraints, which as I have already argued, includes the OCP. Thus the tableau in (91) basically shows the right analysis for the Georgian data, with a lexical markedness constraint embedded in the middle of a ranking of IO-constraints. The tableau in (87) for *relation* and *equation* is a similar case. Both require that the IO-constraints be ranked from highest to lowest frequency.

But then how can the common correlation of high frequency with unmarkedness be handled within the competence module of phonological theory? If I am right in ranking parochial IO-constraints in order of decreasing rather than increasing frequency (and I have already given

five arguments for this), then the answer is that they can't. Therefore they must be motivated by something outside the competence module itself.

As it happens, from the very beginning the OT literature has assumed the very "something" that is needed: <u>Lexicon Optimization</u> (Prince and Smolensky 1993, Smolensky 1996). This principle of language acquisition is given below.

(92)    [Prince and Smolensky 1993:192]
Suppose that several different inputs $I_1$, $I_2$, ..., $I_n$ when parsed by a grammar G lead to corresponding outputs $O_1$, $O_2$, ..., $O_n$, all of which are realized as the same phonetic form $\Phi$ -- these inputs are all *phonetically equivalent* with respect to G. Now one of these outputs must be the most harmonic, by virtue of incurring the least significant violation marks: suppose this optimal one is labelled $O_k$. Then the learner should choose, as the underlying form for $\Phi$, the input $I_k$.

In essence, this principle says that for a given word, if there are multiple inputs that could map to the actual output, the acquirer will posit the most optimal output as the input. As its name clearly suggests, this is a device that serves to make the lexicon more unmarked. If we simply add the reasonable assumption that children do the work of positing inputs more efficiently, or earlier, or more often, when the words are of higher frequency, we automatically end up with an explanation for why lexical frequency is positively correlated with unmarkedness. In the adult, then, higher frequency words are typically represented in the lexicon with unmarked properties already built in, while lower frequency words are more likely to contain marked properties in their lexical representations. (See Bybee 1996 for strikingly similar views couched in a totally different conceptual framework.)

But we have not solved just a minor mystery regarding frequency effects. If knowledge of lexical phonology is knowledge of lexical items, the above argument means that there is a natural force that causes patterns in lexical phonology (even patterns that are widely attested throughout the lexicon like palatalization of nasal place assimilation) to be unmarked. Crucially, though, this force must be in performance (acquisition), not competence.

## 4.4  Emergence of the unmarked and emergence of the marked

My final comment is both a followup to the previous discussion and a reply to another potential criticism of my view of lexical phonology. At issue is what is known in the OT literature as <u>emergence of the unmarked</u>, the phenomenon whereby markedness constraints that usually have no effect in some language (because of the higher ranking of faithfulness constraints) suddenly demonstrate their existence in morphologically complex words where faithfulness cannot apply, such as reduplication (McCarthy and Prince 1995a) or truncation (Benua 1995). For example, Benua (1995), following observations of Poser (1990) and Itô (1990), notes that in Tokyo Japanese, there are many monomoraic words (e.g. /ki/ "tree"), including at least one monomoraic personal name /ti/. When this name undergoes a hypocoristic process, which normally shortens names to two moras (e.g. /midori/ becomes /mido-čan/ or /mii-čan/), the

name /ti/ is *lengthened* to two moras:  /tii-čan/.  Thus the bimoraic "minimal word," which results from a high ranking of markedness constraints like FTBIN, only emerges in derived forms that in some sense do not have an "input."  The name /ti/ is normally pronounced /ti/ because IDENT-IO(V-length) outranks FTBIN, whereas in the derived hypocoristic, IDENT-IO(V-length) is irrelevant, allowing FTBIN to have full effect.

Interestingly, Hammond (1997) has found something like "emergence of the unmarked" in the English rhythm rule as well.  In his experiment, morphologically derived words (e.g. *unknown*) were highly likely to undergo the rhythm rule (e.g. *únknown chíef*), regardless of the lexical frequency of the words.  He comments that this follows naturally from the assumption that derived words like *unknown* are not listed in the lexicon, and thus have no associated parochial constraints to outrank the universal markedness constraint *CLASH.

This seems to be a problem for my model for two reasons.  First, since the emergence of the unmarked specifically occurs in morphologically complex words, it must be lexical in some sense.  The fact that unmarkedness can emerge in morphology therefore implies that it must be represented somewhere in the grammar, not merely in the lexical items themselves.

Second, it appears that unmarked patterns can emerge even in highly productive morphology.  The degree of productivity of some the morphology discussed in some of the literature is not clear (e.g. McCarthy and Prince 1995a, Benua 1995), but the morphological structures used in Hammond's experiments are certainly productive:  twelve of his fifteen morphologically complex words involved compounding or class 2 affixation, both quite productive processes.  The problem for my model is that morphological productivity is defined by the likelihood of applying on-line, which therefore correlates with the likelihood of generating a novel word.  Hence words formed with productive morphology tend to be of lower frequency, on average, than monomorphemic words.  How can a single process allow low frequency simple words to be marked, and yet allow unmarkedness to emerge in low frequency complex words?

As with the formalism of productive allomorphy, more work needs to be done to deal with these problems, but some observations can already be safely made.  These demonstrate that the emergence of the unmarked is not the extremely free phenomenon that we would expect if markedness constraints were fully represented in the grammar of lexical phonology.

First, it appears that the emergence of the unmarked not only *can* occur with highly productive morphology, but in fact this is a *necessary* condition for it.  As just noted, Hammond's conclusion was based on fifteen complex words, almost all generated by highly productive morphology.  The famous "broken" plurals in Arabic (e.g. McCarthy and Prince 1990, 1995c) provide a contrasting example.  They involve prosodic morphology of the same basic type as in the truncation phenomena analyzed by Benua (1995), except that faithfulness appears even less applicable, since the "input" to the Arabic broken plural is nothing more than an unpronounceable sequence of consonants.  Yet the unmarked does not emerge in the broken plurals.  That is, given the constraints that make an iambic foot the unmarked form for nouns, we would expect all nouns to have this form.  Not only is this not the case, but there are even instances of "anti-iambs" that must be explained away (e.g. /kaatib/ "writing, scribe" is derived from /katab/ "wrote" by a special vowel-lengthening process, which begs the question of why this process doesn't obey "emergence of the unmarked" either).  The explanation for the lack of emergent unmarkedness in the broken plural probably lies in its lesser productivity:  canonical

noun structure in Arabic is routinely violated by recent loan words like /tilifuun/ "telephone", and Omar (1973) notes that children seem to prefer the more regular suffixing "sound" plurals to the broken plurals.

The second observation is that even in highly productive morphology, not all kinds of unmarkedness spontaneously emerge. For example, consider the variable -t/d deletion process in English alluded to earlier in this paper (e.g. where *lift* optionally becomes *lif'* in fluent speech; see Guy 1991a,b, Bybee 1996, Myers and Guy 1997, among many other places). The deletion can be analyzed through obedience to a universal markedness constraint against coda clusters (i.e. *COMPLEX). It is thus prosodically motivated like the English rhythm rule, and it is also sensitive to lexical frequency in the same way as the rhythm rule: in monomorphemic forms it occurs more readily in higher frequency words like *past* than in lower frequency words like *priest* (Bybee 1996, Myers and Guy 1997). Yet the effect of morphology is the opposite of the cases discussed above: -t/d deletion occurs far more commonly in underived words than in regularly inflected past tense forms (e.g. *laughed* becoming *laugh'*). Another example of the <u>emergence of the marked</u> is found with geminates in English. As is well known, geminates are forbidden in monomorphemic forms and in forms derived through (less productive) class 1 morphology, e.g. *innate*, but not in forms derived through (more productive) class 2 morphology, e.g. *unnatural* (Borowsky 1986; Goldsmith 1990). If we assume that geminates are marked, motivating a universal constraint like *GEMINATE, we again have a case where the more marked forms emerge only with productive morphology.

I note in passing that such cases pose yet another problem for Hammond's (1997) analysis. He assumes that the reason *CLASH affects derived words, and affects them regardless of frequency, is because they have no associated parochial constraints to block this markedness constraint. In the case of -t/d deletion, however, *COMPLEX should not only affect all regularly inflected forms regardless of frequency (Myers and Guy 1997 say true, Bybee 1996 says false), but more importantly, it should affect derived forms *more* than monomorphemic forms (everybody says false).

Why does unmarkedness emerge in the rhythm rule but not in -t/d deletion? The safe answer is that only in the former case are faithfulness constraints totally inoperative with words formed through productive morphology. That is, a word like *unknown* comes with no inherent stress pattern of its own to override *CLASH, but a word like *laughed* does come with a [t], and this can override *COMPLEX through a nonparochial IO-faithfulness constraint (something I assume does not exist). The problem is how to allow *CLASH into the grammar so that it can (a) affect derived words like *unknown* but (b) not affect low frequency simple words like *arcane*.

There are a number of ways of doing this, and choosing the correct one requires examining more cases, but one possibility worth exploring is as follows. Suppose that the phonetic motivations for the rhythm rule are gradient. In particular, suppose that rather than a categorical constraint *CLASH, there is a less stringent constraint that disfavors two adjacent strongly stressed syllables, but nods its grudging assent if there is even the slightest difference in the degree of stress. (I assume that phonetically-motivated constraints are all of this minimal sort.) Further suppose that among the set of lexical markedness constraints (i.e. those motivated by lexical processing) there is a family called CATEGORICAL(F), which requires that the

phonological property F be realized as discrete rather than gradient (see Kirchner 1997 for discussion of how such a proposal can be formalized in more detail, and Kirchner 1999 for speculations on where lexical categoricality comes from in the first place). Through constraint ranking, universal principles, or idiosyncratic specification, the constraint CATEGORICAL(F) may or may not apply for a given feature in a given language. My suggestion is that CATEGORICAL(stress) holds in English.

The way this would work is as follows. The gradient *CLASH-like constraint could be schematized as follows, where the tower of x's is just meant to represent very strong stress.

(93)    *STRONG-CLASH:    * x    x
                            x    x
                            x    x
                            σ    σ

As a universal phonetically-motivated markedness constraint that applies in every language the same way, the ranking of this constraint cannot vary across languages; let's say it's at the top, with all the other phonetically-motivated markedness constraints. Then we have our usual parochial IO-faithfulness constraints, but remember that they are operating on a lexicon in which higher frequency words like *antique* already have an innate preference to obey the rhythm rule (i.e. a phrasal clash environment will tend to trigger selection of the allomorph with initial stress), while lower frequency words like *arcane* have the opposite innate preference. Beneath all of the IO-faithfulness constraints (or at least those for words which are frequent enough for speakers to remember their lexical stress at all) is the constraint CATEGORICAL(stress). To make its operation precise, I assume that it bans towers of x's that do not have precisely three x's.

The tableaux for *antique book*, *arcane sort*, and *unknown chief* will thus be as follows, where the IO-faithfulness constraints rack up separate violations for each misplaced x.

(94)    Lexical assumption: *ántique* prefered to *antíque* in clash contexts

| antique book | *STRONG-CLASH | IO-*antique* | IO-*arcane* | CATEGORICAL |
|---|---|---|---|---|
| x        x<br>x        x<br>x        x<br>antique  book | * | *** | | |
| x<br>x        x<br>x  x        x<br>antique  book | | ** | | * |
| x<br>x        x<br>x  x        x<br>antique  book | | * | | * |
| ☞ x        x<br>x        x<br>x        x<br>antique  book | | | | |

(95)    Lexical assumption:  *arcáne* prefered to *árcane* in clash contexts

| arcane sort | *STRONG-CLASH | IO-*antique* | IO-*arcane* | CATEGORICAL |
|---|---|---|---|---|
| x    x<br>x    x<br>x    x<br>arcane  sort | * | | | |
| ☞        x<br>   x    x<br>x  x    x<br>arcane  sort | | | * | * |
|         x<br>x       x<br>x  x    x<br>arcane  sort | | | ** | * |
| x      x<br>x      x<br>x      x<br>arcane  sort | | | *** | |

(96)    Lexical assumption:  *unknown* is not listed in lexicon

| unknown chief | *STRONG-CLASH | IO-*antique* | IO-*arcane* | CATEGORICAL |
|---|---|---|---|---|
| x     x<br>x     x<br>x     x<br>unknown  chief | * | | | |
|         x<br>  x    x<br>x  x    x<br>unknown  chief | | | | * |
|         x<br>x      x<br>x  x    x<br>unknown  chief | | | | * |
| ☞ x      x<br>  x     x<br>  x     x<br>unknown  chief | | | | |

The correct results therefore emerge in every case without the need for a categorical markedness constraint like *CLASH to reside inside the lexical phonology itself.  All that is needed is for *STRONG-CLASH to give a slight nudge, and CATEGORICAL does the rest.  Note that the analysis predicts that the clash in phrases like *arcáne sórt* will not be phonetically complete.  This and other interesting issues (e.g. a thorough cross-linguistic typology of emergence of the marked/unmarked) are clearly worthy of follow-up.


**5.     Conclusions**

I have two central hopes about how this paper has affected the reader's thinking about lexical phonology. First, I hope I have provided convincing enough empirical evidence that analogy, as I have defined it, is indeed a real force in lexical phonology. Perhaps the reader even agrees with me that patterns in lexical phonology, to the extent that they are real parts of competence at all, can be described by nothing but analogy. Lexical patterns are certainly not due to universal markedness constraints of any motivated sort, and OT doesn't give us the option of simply inventing language-specific "rules" at will. If there is a coherent alternative in OT to analyzing lexical phonology with something like analogy, I would like to know what it is.

Second, I hope my formalism is a useful first step in taming analogy within generative phonology. As I noted earlier, I did this by translating the behavior of constraint-satisfaction connectionist networks into an OT framework, which may lead one to wonder whether it's not better to go all the way and just analyze lexical phonology with "real" connectionism. While I don't rule out this option, the disadvantages are many, including the fact that connectionism isn't exactly popular among linguists, and more importantly, the fact that connectionist models are overly complex: they typically have hidden fudge factors, they are overly sensitive to initial states and properties of training regimens, and even in simple forms their behavior is not yet well understood mathematically (see e.g. J. Anderson 1995 for a simple introduction to the math of connectionism). By building user-friendly "networks" with the tinkertoys of OT, we may be able to gain a more solid understanding of what exactly needs to be described in real lexical analogies.

In addition to these two main points, some other issues have come up in the course of this paper, and I hope that the reader is left with impressions of the following three theoretical points. First, I have argued that output-output correspondence should only hold of words actually stored in the lexicon, which means that it should not hold of words derived through highly productive morphology. This not only puts restrictions on the use of OO-faithfulness in analyses, but also has some interesting empirical benefits, as I have shown.

Second, my model predicts that phonological patterns can only interact in an opaque fashion if at least one of them is lexical, i.e. is actually analogy. Since the analysis of analogy only requires a ranking of parochial faithfulness constraints, there is thus no reason for OT researchers to resort to the cumbersome and inorganic formalism of sympathy theory (McCarthy 1998) or the like. Moreover, I predict that apparent "rule ordering" will tend to correlate with analogical "strength," which in turn correlates with the number of lexical items that conform to a given analogy (see also J. Myers 1993). Thus pattern A will tend to act as if it applies "before" pattern B if there are many more words conforming to B than to A in the lexicon. These predictions appear to be supported in English, and it would be important to know if they hold up in other languages.

Finally, generative phonologists, from the beginnings through OT, are simply wrong when they apply phonetically motivated constraints directly in lexical phonology. The reason lexical phonology conforms to phonetic factors to the extent that it does lies in performance (language acquisition), not in competence. However, there do seem to be some universal factors that directly affect lexical processing, such as the OCP perhaps, and there are other factors, such as constraints on syllable structure, of mysterious etiology. The hypothesis that lexicon-external

factors cannot directly affect lexical phonology thus leads to new and interesting predictions concerning the behavior of different kinds of markedness constraints.

In short, my proposed approach to lexical phonology may mean the end of generative phonology as we know it, but isn't it about time?

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