

## Chapter 6

# Discussion

In the preceding chapters, I have laid out a model of paradigm acquisition that starts by identifying a base form within the paradigm, and then develops a grammar of rules that take the base form as an input and derive the rest of the forms in the paradigm. I have pursued two specific hypotheses about how this is done: first, I have restricted the model to a single base form within the paradigm, and required further that the base form match a surface form from the same part of the paradigm for all lexical items (the “single surface base” hypothesis). Second, I have developed the idea that the learner selects the base form that is the most informative – that is, that permits the most efficient grammars for deriving the remainder of the paradigm. I have shown that this approach yields the right results in three cases. In chapter 2, I showed that for an older stage of Yiddish it selects the 1sg as the verbal base and correctly predicts the subsequent paradigm leveling. In chapter 4, I showed that for pre-classical Latin noun paradigms it selects an oblique form as the base, and correctly predicts the details of the *honor* analogy. Finally, in chapter 5, I showed that for Lakhota verb paradigms it selects a second person form as the base, and correctly predicts subsequent analogical changes.

This model is designed with two related tasks in mind. The first is to discover the inputs to morphology (the bases, or underlying forms) given a set of surface alternants. The second is to identify which form will serve as the base of analogical change in a particular language at a particular time. I have taken the strong position that the asymmetries we observe in analogical change are intimately related to the asymmetry between base forms and derived forms. In particular, I have assumed that analogy is a form of overregularization, in which exceptional forms are replaced with productively derived, grammatically expected forms.

It must be recognized that neither of these tasks is easy; both are fraught with problems, and both have been the subject of substantial discussion over the years. Furthermore, in both areas, the general consensus has been that it is impossible to provide an all-purpose discovery procedure. In the case of URs, a broad array of cases has been amassed showing that allowing URs to stray from a single surface alternant enables us to capture many generalizations that would have been missed otherwise (e.g., Kenstowicz and Kisseberth 1977; chap. 1; 1979, chap. 6). In the case of analogical bases, it has been shown that there are various generalizations that can be made about which forms are most likely to act as bases (isolation forms, morphosyntactically “unmarked” forms, forms with high token frequency, etc.), even if they are true only at the typological level (Hock 1991, pp. 234-237).

On the face of it, then, the current model appears to be inadequate on both fronts. It cannot

construct abstract URs, and it does not have any built-in biases for selecting unmarked forms, frequent forms, or any other particular form. Thus, there are two types of data that it is in danger of not being able to handle satisfactorily. The first is the set of cases in which it has been argued that underlying forms must go beyond single surface forms, or basic alternants. The second is the typological data regarding bases of analogical change, discussed by Kuryłowicz (1947), Mańczak (1958), Bybee (1985) and others.

In the sections that follow, I will consider how the proposed model might nonetheless be able to handle each of these types of data.

## 6.1 URs

It has been accepted since at least the 1970s that there may be no general, automatic discovery procedure to find the UR that unifies all of the surface alternants and allows them to be projected using a reasonably natural and efficient set of phonological rules or constraints (Hyman 1975, pp. 90-98; Kenstowicz and Kisseberth 1977, chap. 1; Kenstowicz and Kisseberth 1979, chap. 6). The reason that formalizing the UR discovery process is so hard is that several of the necessary steps require human insight or intuition.<sup>1</sup> We must be able to determine which forms should be derived from others, comparing each of the possibilities and taking into account how elegant, natural, or simple the rules that would be needed are. We must sometimes have the insight that abstraction is necessary, either by combining surface elements from different forms, or even sometimes by positing abstract structure that is never visible on the surface. In such cases, we must have some intuition about the right types of structure to posit, and whether the pay-off for positing the abstract structure is sufficient to motivate it. Finally, we must sometimes recognize that an alternation defies grammatical description, and certain alternants must simply be memorized as listed allomorphs or as lexical exceptions.

The current model does only a fraction of this: it can determine which alternants should be derived from one another by considering the efficiency of the rules in both directions, but it does not (at present) have any sense of naturalness, nor does it abstract away from the surface alternants to create a UR that does not appear as such somewhere in the paradigm. Thus, it could be said that the model is really discovering “basic alternants”, and not URs as they are known and used in generative phonology. For this reason, there are a great many analyses in the literature that it would never be able to discover. This restriction does have historical precedent, however. Kenstowicz and Kisseberth point out that the “basic alternant” restriction on URs, either with or without the additional constraint that we use the same part of the paradigm for all lexical items, is appealing because it is a strong and concrete hypothesis (Kenstowicz and Kisseberth 1977, pp. 28-29), and in fact Sapir seems to have operated under something like this

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<sup>1</sup>A similar sentiment is to be found in Hockett (1955): “We know of no set of procedures by which a Martian, or a machine, could analyze a phonologic system...” (p. 147)

restriction (McCawley 1967).<sup>2</sup>

The reason why this constraint is so strong is because it severely limits the possible analyses in cases of symmetrical neutralization, or in cases where there are multiple neutralizations working simultaneously. As discussed at length in the previous chapter, in the case of a symmetrical neutralization between *a*, *b*, and  $a \sim b$ , it means that the basic alternant cannot keep  $a \sim b$  distinct from both *a* and *b*, but it can at least keep it systematically distinct from one of the two. This is what allows the model to predict that invariant [a] should be merged with alternating [e]~[a] in Lakota, but kept distinct from invariant [e]. In this case, the device that the model is unable to use is the archiphoneme. However, it is not the case that the model cannot learn the difference between invariant [e] and alternating [e]~[a]—it simply learns it in a different way, relying on listed exceptions rather than on a distinct underlying representation to preserve the minority pattern.

The real challenge comes in the case of multiple neutralizations, such as the following: suppose *a* and *b* are always neutralized to *a* in form 1, while *c* and *d* are always neutralized to *d* in form 2. Under the usual assumptions of generative phonology, learners can abstract away from these forms and combine their information to yield an UR in which both distinctions are preserved: /ac/, /ad/, /bc/, /bd/. The single surface base restriction, on the other hand, means that the decision about which form to choose as the base for the *a/b* neutralization also affects the analysis of the *c/d* neutralization. If we choose form 1, *a* and *b* are distinct but *c* and *d* are merged, and the reverse for form 2.

Latin rhotacism is a case of this sort. Nominative forms in Latin had a number of neutralizations, including neutralizations in morphological classes (e.g., *-us* for both second and fourth declension nouns), phonological neutralizations caused by devoicing or deleting obstruents before the *-s* suffix (e.g., *ur[p]-s* ‘city’, *ar-s* ‘art’), and so on. One of the few neutralizations that went in the opposite direction was rhotacism, in which the [s]~[r] distinction was maintained in the nominative but merged in the remaining forms. If learners had been able to combine information from multiple forms—or even if they had just been able to use forms from different parts of the paradigm for different words—then they should have had no trouble setting up underlying /s/ to keep words like [hono:s] distinct from words like [soror]. Thus, the single surface base restriction plays a role in constraining the predictions of the model even when archiphonemes and underspecification are not involved.

Unfortunately, Latin rhotacism is very mild as neutralizations go, affecting only two segments in a restricted class of words. In some cases, each of the multiple neutralizations involves a large class of segments, potentially affecting a huge number of lexical items. A famous case of this, discussed by Kenstowicz and Kisseberth (1977, pp. 18-19 and 26-27), occurs in Russian. Russian, like German, Dutch, Turkish, and many other languages, has a process of final devoicing, by which voiced obstruents become devoiced in final position. Final devoicing can create alternations within noun paradigms, since the case endings are null for some noun classes

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<sup>2</sup>Zellig Harris, although usually associated with the approach of distributional learning of recurring partials, also proposes something like this towards the end of his *Methods in Structural Linguistics*. He suggests that in languages with multiple inflectional classes, such as Latin, it is necessary to list words with some sort of inflectional ending that will tell what class the word belongs to. In considering which form to choose as the default, or base, Harris proposes a model that is strikingly similar to the one I have implemented here: “The criteria for selecting a basic alternant are not meaning or tradition, but descriptive order, i.e. resultant simplicity of description in deriving the other forms from the base.” (Harris 1951, p. 308, fn. 14).

in some forms (the nominative singular and inanimate accusative singular of consonant-final nouns, and the genitive plural of a certain set of nouns). Examples of words with and without voicing alternations are given in (81).

(81) Russian final devoicing: *ryčag* vs. *rybak*

a. Underlying /g/ → [k] in nom./acc. sg. of *ryčag* ‘lever’<sup>3</sup>

	<i>sg.</i>	<i>pl.</i>
<i>nom.</i>	ryčák	ryčagí
<i>gen.</i>	ryčagá	ryčagóf
<i>dat.</i>	ryčagú	ryčagám
<i>acc.</i>	ryčák	ryčagí
<i>instr.</i>	ryčagóm	ryčagámi
<i>loc.</i>	ryčagé	ryčagáx

b. Underlying /k/ always [k] in *rybak* ‘fishmonger’

	<i>sg.</i>	<i>pl.</i>
<i>nom.</i>	rybák	rybakí
<i>gen.</i>	rybaká	rybakóf
<i>dat.</i>	rybakú	rybakám
<i>acc.</i>	rybaká <sup>4</sup>	rabakí
<i>instr.</i>	rybakóm	rybakámi
<i>loc.</i>	rybaké	rybakáx

From the point of view of final devoicing, then, we would need to take a suffixed form as the underlying form of a Russian noun. There is, however, a competing process that affects suffixed forms: vowel reduction of stressless /e/ and /o/ to [i] and [a], respectively. Since some nouns in Russian have stress alternations (sometimes on the root, sometimes on the suffix), this can create alternations in vowel quality. For example, there is a class of nouns in which stress falls on the first syllable of the suffix, if there is one; in forms with no suffix, the stress has no choice but to fall on the root. This means that the final syllable of the root surfaces intact in unsuffixed forms, but is reduced to [i] or [a] in suffixed forms (82a), causing a neutralization with underlying /i/ and /a/—cf. *rybaká* (81) vs. *sjedaká* (82a). The fixed-stress form in (82b) shows that vowel reduction really is conditioned by stress, since it does not occur when the stress remains on the root.

<sup>3</sup>I will use the transcription <y> for the Russian vowel <ы>.

<sup>4</sup>The difference between [račák] and [rabak-á] is due to a difference in animacy—animate nouns have overt accusative marking, and inanimate ones do not.

## (82) Russian vowel reduction in words with stress shift

a. Reduction in *sjedok* 'rider' (stress shift)

	<i>sg.</i>	<i>pl.</i>
<i>nom.</i>	sjedók	sjedakí
<i>gen.</i>	sjedaká	sjedakóf
<i>dat.</i>	sjedakú	sjedakám
<i>acc.</i>	sjedaká	sjedakí
<i>instr.</i>	sjedakóm	sjedakámi
<i>loc.</i>	sjedaké	sjedakáx

b. No reduction in *pr'itok* 'influx' (fixed stress)

	<i>sg.</i>	<i>pl.</i>
<i>nom.</i>	pr'itók	pr'itóki
<i>gen.</i>	pr'itóka	pr'itókof
<i>dat.</i>	pr'itóku	pr'itókam
<i>acc.</i>	pr'itók	pr'itóki
<i>instr.</i>	pr'itókom	pr'itókami
<i>loc.</i>	pr'itóke	pr'itókax

We see, then, that among nouns with a stress shift in suffixed forms, the processes of final devoicing and vowel reduction affect mutually exclusive sets of forms: final devoicing affects the suffixless forms, while vowel reduction affects the suffixed forms. This means that there can exist words in which no part of the paradigm reveals the full underlying form:

## (83) Devoicing in unsuffixed forms, vowel reduction in suffixed forms

a. *pirog* 'pie'

	<i>sg.</i>	<i>pl.</i>
<i>nom.</i>	pirók	piragí
<i>gen.</i>	piragá	piragóf
<i>dat.</i>	piragú	piragám
<i>acc.</i>	pirók	piragí
<i>instr.</i>	piragóm	piragámi
<i>loc.</i>	piragé	piragáx

b. *sapog* 'boot'

	<i>sg.</i>	<i>pl.</i>
<i>nom.</i>	sapók	sapagí
<i>gen.</i>	sapagá	sapagóf
<i>dat.</i>	sapagú	sapagám
<i>acc.</i>	sapók	sapagí
<i>instr.</i>	sapagóm	sapagámi
<i>loc.</i>	sapagé	sapagáx

This and similar cases compelled Kenstowicz and Kisseberth and others to conclude that URs must absolutely be able to combine information from multiple surface forms, since there is no single place in the paradigm where the vowel quality of the final vowel and the voicing status

Table 6.1: Distribution of terminations and accent patterns

Termination	Total Count	Suffix-accenting
/-ak/	196	100
/-ag/	46	4
/-ok/	948	9 <sup>6</sup>
/-og/	178	5

of final obstruents are both unambiguously revealed. As previously discussed, the assumption that drove this conclusion was that we would like to be able to derive all surface forms unambiguously from a single UR using a simple set of case endings and phonological rules, and in order to do this, we need to create underlying distinctions between all of the observed surface patterns.

What if we relaxed these assumptions, however? Suppose that we allowed the grammar to have fancier rules than simple affixation, including multiple, competing generalizations about specific phonological environments—such as changing *Xaga* → *Xok* to form the nominative, and so on. And suppose further that instead of unambiguously deriving all forms of all words, we merely required that the grammar get as many forms right as it could given the single surface base restriction, and then we allowed the remaining forms to be listed as exceptions. How much headway could the learner make on Russian using such a grammar, and how much would need to be memorized?

In principle, we might expect this to be a hopeless task, since there are so many underlying possibilities, and so many neutralizations. There could, for example, be nouns of all of the following four types (labeled, for convenience, with the UR they would receive under the traditional analysis):

## (84) Four possible types of Russian nouns

	/...ak/	/...ag/	/...ok/	/...og/
<i>nom. sg.</i>	[...ák]	[...ák]	[...ók]	[...ók]
<i>gen. sg.</i>	[...aká]	[...agá]	[...aká]	[...agá]
<i>dat. sg.</i>	[...akú]	[...agú]	[...akú]	[...agú]
<i>acc. sg.</i>	[...ák]	[...ák]	[...ók]	[...ók]
...				

In practice, however, not all of these possibilities are equally attested. Using Zaliznjak's reverse dictionary of Russian (Zaliznjak 1977), I did some rough counts of words ending in /-ak/, /-ag/, /-ok/, and /-og/, shown in Table 6.1. First, one may note that there are relatively few /ag/ stems, and there is a preponderance of /ok/ stems. What is more significant, though, is the relative distribution of stem vs. suffix-accenting nouns. Among stems ending in /-ag/, /-ok/, and /-og/, virtually all have fixed stress on the stem (like *pr'itók* in (82b) above), and vowel reduction is not an issue. Words like Kenstowicz and Kisseberth's *pírog* are vanishingly rare; they include only *pírog* 'pie', *sapog* 'boot', *batog* 'thick stick', *podog* (no gloss), and *tvoróg* 'curd'.<sup>5</sup> Almost all of the stress-shifting stems in this portion of the lexicon belong to the /-ak/ group.

<sup>5</sup>Zaliznjak also lists a variant of this word with fixed penultimate stress: [tvórok], [tvóroga], etc.

The result of all of this is that at least for this set of words, it is not at all difficult to learn to project the nominative from an oblique form (that is, a form other than the nominative or accusative). If the word has stress on the stem, one need only peel off the case suffix and devoice the final consonant. If the word ends in suffix-stressed [... aká], it is virtually certain to end in [ák] in the nominative, and the nine exceptions could be easily listed. If the word ends in suffix-stressed [... agá], it is a toss-up whether it will end in [... ák] or [... ók] in the nominative, but there are only four of the former and five of the latter, so listing these does not seem overly costly.

One might wonder, given the preponderance of /-ok/ stems in general, why there are not more stress-shifting /-ok/ stems that could be neutralized with the stress-shifting /-ak/ stems. In fact, a sizeable majority of /-ok/ stems (787 out of 948) participate in a different alternation, caused by deletion of stressless mid vowels in the final syllable of the root—the so-called *yers*. Without going into the details of *yer* alternations (see Lightner 1972, Yearley 1995, and Hermans 2001 for discussion), we can note simply that words with /-ok/ and /-ak/ remain distinct, even in stress-shifting forms, because the /-ok/ words generally delete the /o/ rather than reducing it:

(85) /-ak/ vs. /-ok/ remain distinct

a. *sudak* ‘pike perch’

	<i>sg.</i>	<i>pl.</i>
<i>nom.</i>	sudák	sudakí
<i>gen.</i>	sudaká	sudakóf
<i>dat.</i>	sudakú	sudakám
<i>acc.</i>	sudák	sudakí
<i>instr.</i>	sudakóm	sudakámi
<i>loc.</i>	sudaké	sudakáx

b. *sudok* ‘cruet-stand, dinner-pail’

	<i>sg.</i>	<i>pl.</i>
<i>nom.</i>	sudók	sudkí
<i>gen.</i>	sudká	sudkóf
<i>dat.</i>	sudkú	sudkám
<i>acc.</i>	sudók	sudkí
<i>instr.</i>	sudkóm	sudkámi
<i>loc.</i>	sudké	sudkáx

The forms in (85) show that if we take an oblique form as the base in Russian, as in Latin, then it is not difficult to predict what vowel a reduced [a] should correspond to in the nominative—it is almost always [a]. The forms with *yer* deletion in (85b) show that there may be other problems, such as predicting whether a word should have a vowel inserted between the consonants in the nominative, and which vowel should be inserted ([o] or [e]). However, even this may be somewhat predictable. Various scholars working on Polish have suggested that *yer* deletion

<sup>6</sup>I am excluding here the large number of nouns that have suffix accentuation, but also have final *yer* alternations (see (85b) below). The reason is that in these words, there is no issue of trying to recover the underlying quality of a neutralized vowel, since the relevant vowel is deleted altogether.

might actually be analyzable as an epenthesis process to break up word-final clusters (\*[sudk] > [sudok]) (Gorecka 1988; Czaykowska-Higgins 1988, and others). Yearley has argued that this is harder in Russian than in Polish (p. 538), but it does not seem impossible if we allow exceptions. For example, the *-ok* nominatives listed in Zaliznjak far outnumber the *ek* nominatives and *-Ck* nominatives, so a genitive form with *-Cka* most likely corresponds to a nominative in *-ok*.

It appears, then, that for a particular subset of the consonant-final nouns in Russian, the final vowel of the root is not nearly as hard to predict as Kenstowicz and Kisseberth's discussion would imply.

It is important to consider whether this might just be an “easy” region of the vocabulary, or whether it is representative of the language as a whole. As it happens, there are reasons to believe this is not an especially easy sample, and that the *-ak/ -ok/ -ag/ -og* nouns are actually some of the hardest nouns to predict. Recall that vowel reduction affects only mid vowels, so the only relevant types of stems are [...oC] and [...eC] stems—which happen to also be the environments for yer deletion. Furthermore, a quick glance through Zaliznjak's dictionary reveals that many final consonants appear to have few, if any stress-shifting nouns. (For example, there are virtually no [...ad] or [...od] nouns with shifting stress.) For nouns that do not shift stress, the vowels are constant throughout the paradigm, and there is no predictability problem. Thus, the region of the vocabulary that I have considered here may actually have *more* neutralizing vowel reduction than average. The single surface base analysis of Russian, in which it would probably be an oblique form that serves as the base, appears to be worth pursuing.

My conclusion from this section is that it is not obvious that a model operating under the single surface base restriction would be completely unable to learn languages like Russian, or by extension, perhaps also Tonkawa, Pengo, or any of the other cases in Kenstowicz and Kisseberth (1977) in which it was argued that it is necessary to combine information from different forms. It seems that languages tend to have a surprising amount of surface predictability between forms; even if the possible underlying forms could theoretically have been distributed evenly and randomly throughout the vocabulary, this appears not to happen.<sup>7</sup>

If an analysis using a single part of the paradigm as the base could be made to work for Russian—that is, if a suitably detailed grammar could manage to productively derive the correct outputs for a reasonably large proportion of the vocabulary—it would have the advantage of making strong predictions about possible errors and analogical changes, as in Lakhota. In particular, we would expect that stem vowels of stress-shifting nouns might sometimes be overregularized—e.g., \*[pirák] instead of [pirók]—but we should not get any changes to oblique forms, such as changing the voicing of obstruent, or leveling the placement of stress (\*[piróga] instead of [pirogá]). It would be interesting to see if changes of the former type have already been going on in Russian, helping to create and solidify the regularities noted here.

## 6.2 Bases of analogical change

Most of the evidence that I have used here about bases has come from historical analogical changes, either in the form of paradigm leveling (Yiddish, Latin) or in the form of extending alternations (Lakhota). I have shown that the proposed model makes the right predictions in each case. These three changes are all “hard cases” — they involve typologically unusual

<sup>7</sup>This phenomenon has also been noted for Yidiş by Hayes 1999, pp. 11-14.



changes, in that forms that are typically “more basic” (nominative singulars, third person singulars) have been changed on the basis of more marked forms. The model accomplishes this in part by ignoring the factors that are often said to play a typological role in determining the base of an analogical change: the degree of suffixation (unsuffixed, single consonant suffix, full syllable suffix, etc.), the relative markedness of forms, type frequency, and so on. However, even if these factors are not the right explanation for why analogical effects take place, the typological approach has uncovered a number of tendencies that must be accounted for. If learners are really able to search the entire paradigm to find whatever form is the optimal base form, why should the same forms be chosen in language after language? Furthermore, if a lexically predominant pattern of alternation can be extended just as easily as non-alternation leveling, then why is there such a strong tendency for paradigm leveling?

I will consider each of these issues in turn.

### 6.2.1 Why a nominative/3sg/etc. preference?

It is often noted that certain forms tend to act as bases over and over again. In noun paradigms, it is the nominative singular that usually drives paradigmatic changes (e.g., Lahiri and Drescher, 1984), while in verb paradigms, it is most often the 3sg present form, although the 1sg is also common (Bybee and Brewer 1980; Bybee 1985, chap. 3). Intuitively, there are a number of factors that could conspire to favor these forms: they are often the forms with the highest token frequency, they often have null affixes making them “isolation forms”, and morphosyntactically, they may in some sense be the “least marked” members of the paradigm. As Bybee points out, these factors are highly correlated; the morphosyntactically least marked forms also tend to be the most frequent, and also tend to have the least overt morphological marking. Thus, in many cases, it is impossible to say which of these is responsible in making one form the most basic, and in fact, we might rather say that it is the collective influence of all of them together.

To the extent that we can sometimes differentiate these factors in languages where they are *not* correlated, this does not seem to help in identifying a universal principle of what privileges forms as bases. For example, sometimes the 3sg present form has an overt affix, but it acts as a base anyway; one example is the operation of “Watkins’ law” in Provençal discussed by Bybee (1985, pp. 55-56), in which the 3sg form replaced all other forms, including the relatively less suffixed 1sg form. In other cases, such as the Yiddish leveling discussed in chapter 2, the suffixless 1sg form acted as the base, taking precedence over the 3sg form. Thus, it is generally concluded that each of these factors is merely a typological tendency, and it is impossible to predict which will win out in a given case. They are, however, tendencies with a great deal of empirical support, and an adequate model of analogical change has to be able to explain why these factors seem to increase the chances that a particular form will serve as base.

I believe that the answer does not lie in how learners seek to find contrasts and organize their grammar, but is rather an epiphenomenal effect of how they receive the input data for morphological learning. In the discussion up until this point, I have been operating under the idealized (and unrealistic) assumption that learners have all parts of the paradigm available in equal proportions when considering their effectiveness as bases. This is clearly not true in real life; some parts of the paradigm are much more frequent than others. This means that learners do not have equal amounts of data about the possible subgrammars. Suppose, for example, that you are learning verb paradigms and comparing the relative effectiveness of three forms: the

1sg, the 2sg, and the 3sg. Suppose further that 3sg forms are by far the most frequent, followed by 1sg forms, and then 2sg forms. This means that on average, you will have heard 3sg forms for most words, and 1sg and 2sg forms for relatively fewer words. Moreover, the number of words that you have heard in both the 3sg and 1sg will be greater than the number heard in both the 3sg and 2sg, and greater yet than the number heard in both the 1sg and 2sg. Figure 6.1 shows a hypothetical input of this sort, listing the attested forms that a learner has encountered for 100 verbs. The most frequent form (the 3sg) is on the left, while the least frequent form (the 2sg) is on the right. Arrows indicate that a verb has been heard in at least two forms, and can thus contribute an input pair for morphological learning on the mapping between those two forms.

How could these frequency differences be used to derive a typological prediction? In section 3.2 (p. 40), I mentioned that the minimal generalization model uses lower confidence limit statistics to favor rules that are based on larger numbers of forms. The original intent of including this adjustment was to encourage the model to use large-scale generalizations, and to be wary of generalizations based on just a few forms that may turn out to be exceptional. This adjustment may have another practical consequence, however, which is to favor subgrammars that are based on larger numbers of input pairs. In particular, if the learner has encountered 60 pairs leading from the 3sg (43 to the 1sg, 17 to the 2sg), but only 29 pairs leading from the 2sg (17 to the 3sg, 12 to the 1sg), this will make the rules in the 3sg-based subgrammars on average more reliable than those in the 2sg-based subgrammars. This captures the intuition that you want to choose a base that you are sure will be reliable in the language as a whole, and you also want to choose a base that will generally be available in order to produce the remaining forms.

The effect would be that sometimes a less common form may preserve contrasts slightly better than a more common form, but the lower confidence limit adjustment would make its subgrammars less reliable, forcing us to select a somewhat less informative (but more frequent) form as base. This bias to choose more readily available forms should be seen not as part of the grammatical learning mechanism, but rather as an incidental external factor that interferes with the ability of the learner to select the truly optimal base for the language. This leads us to wonder, naturally, how suboptimal a base could we be forced to choose, given a large difference in the availability of forms?

A possibly telling example is the change currently underway in Korean, discussed at the end of chapter 4. Korean nouns occur in a variety of forms. Frequently, they occur with no overt case marking (i.e., as just a bare stem), and are affected by word-final neutralization processes caused by severe phonotactic restrictions on what sequences and segments are allowed to occur in word-final codas. For example, Korean has eight coronal obstruents ( $[t, t^h, t^8, t_j, t_j^h, t_j', s, s']$ ), but only one coronal obstruent is allowed in word-final position:  $[t']$  (Kim 1987). Non-coronal obstruents involve similar, but less dramatic neutralizations—in particular, all three laryngeal types are reduced to unreleased unaspirated stops, but there are no fricatives or affricates at the other places of articulation. Thus, unsuffixed forms involve a potentially massive neutralization, as illustrated in Figure 6.2.

When nouns are overtly marked, they may appear in one of several cases: topic (*-in* after consonants/ *-nin* after vowels), nominative (*-i/ -ka*), accusative (*-i/ -li*), dative (*-ey/ -ekey*), genitive (*-i*), and so on. These suffixes protect the noun from the word-final obstruent neutralizations, but they may cause phonological modifications of their own. The nominative suffix *-i*,

<sup>8</sup>I will follow the practice of using  $[C']$  to indicate a tensed obstruent in Korean.

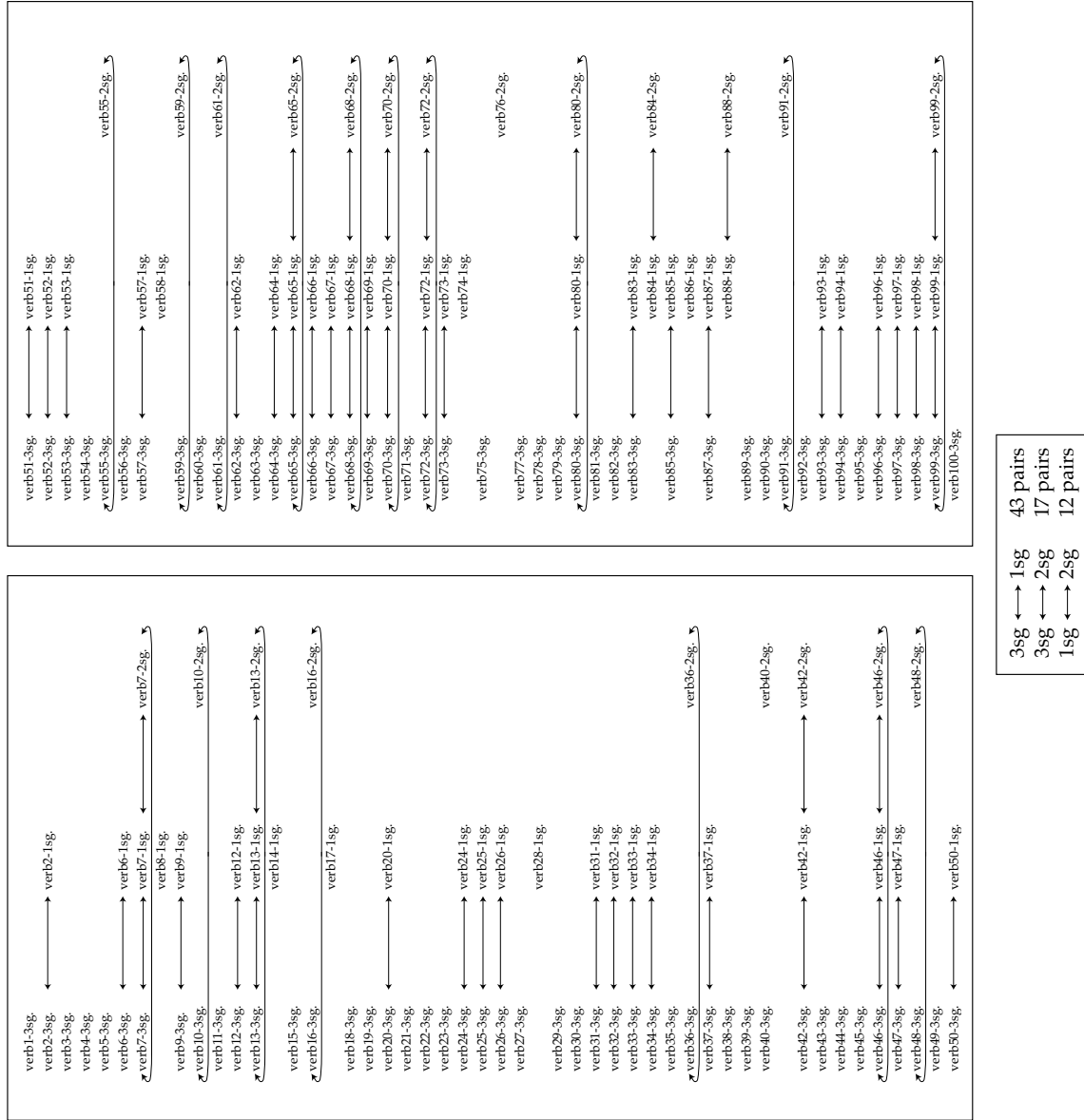


Figure 6.1: Sample input assuming unequal frequencies

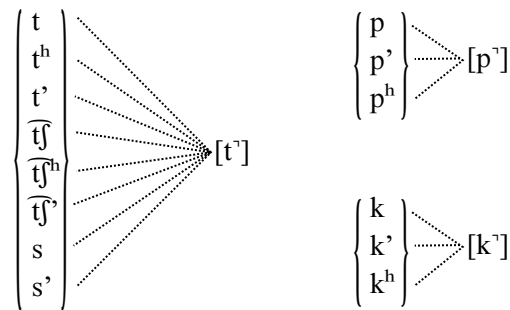


Figure 6.2: Theoretical neutralization of obstruents in word-final position in Korean

for example, causes palatalization of preceding coronal segments ( $/t/ \rightarrow [\widehat{tj}]$ ,  $/s/ \rightarrow [j]$ ,  $/n/ \rightarrow [ɲ]$ ), neutralizing the contrast between  $/t/$  and  $/\widehat{tj}/$  in this form. The topic and accusative case markers begin with a non-front vowel ( $[i]$ ), so in this context, all underlying contrasts would be preserved (though there is one non-neutralizing modification whereby lax stops become voiced intervocally). As with the other languages discussed in the previous chapters, we see that different forms in the paradigm involve different degrees of neutralization.

The reason why Korean is a problem is that in this case, the base of the analogical change appears to be the unmarked isolation form, even though that form is the most neutralizing form of the entire paradigm. Recall from section 4.4.4 (p. 72) that a change currently underway in Korean is replacing the final coronal obstruents of noun stems with a  $s$  in inflected forms, apparently because speakers are attempting to project inflected forms from the massively neutralizing isolation form. This would not be a problem for a more typological approach, since we could simply say that in Korean, the preference for leveling to isolation forms has won out over whatever other preferences might have favored other forms. How could this change be reconciled with the informativeness approach?

I speculate that there are two factors that might allow the current model to select the isolation (unmarked) form as the base in Korean, in spite of its neutralizations. The first is that the picture in Figure 6.2 is not quite accurate. In fact, not all obstruents occur in stem-final position in Korean; in particular, there are no stems ending in a tensed obstruent ( $[p', t', \widehat{tj}', s']$ ), and there are no nouns ending in unaspirated  $[t]$ . Furthermore, among the remaining four coronal obstruents,  $[c]$  seems to be relatively rare,  $[t^h]$  and  $[\widehat{tj}^h]$  are moderately well attested, and  $[s]$  outnumbers all of the others (Figure 6.3). We see, then, that the actual neutralization is not quite as bad as it could have been; given an unsuffixed noun ending in  $[t']$ , the set of possible suffixed forms is far more constrained than the full set of  $[t, t^h, t', \widehat{tj}, \widehat{tj}^h, \widehat{tj}', s, s']$  at equal probabilities of  $1/8$  each.

These asymmetries lessen the severity of the word-final neutralizations, but they do not eliminate them altogether. Even if the unsuffixed form is not as terrible as we might have imagined, it is still the least informative member of the paradigm—what could possibly have

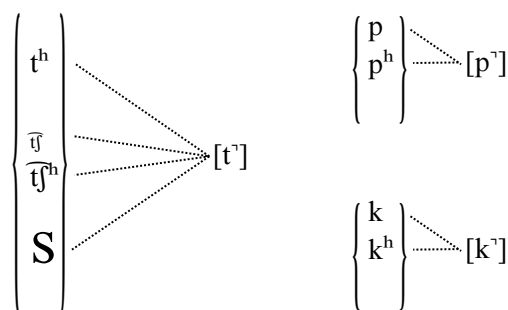


Figure 6.3: Actual neutralization of obstruents in word-final position in Korean

led learners to choose it as the base?

What we have not taken into account is the relative availability of different forms in Korean. Unlike many Indo-European languages, case marking in Korean is optional in many contexts; in fact, marking appears to be omitted in a majority of tokens. In corpus counts of various case forms in child speech and in the child-directed speech of mothers, I. Lee (1999) found that case marking was omitted by mothers 70% of the time when talking to their children. Furthermore, among forms with overt case marking, nominatives far outnumbered accusatives: nominative *-i/-ka* occurred on 25% of child-directed tokens, while accusative *-i/-li* occurred on only 5% of tokens. Thus, the hypothetical proportions shown in Figure 6.1 probably *overestimate* the availability of accusative forms in the learning data. The accusative may be a perfect base in Korean from a predictability point of view, but it is rare enough that learners would have a hard time finding enough input pairs to learn that this generalization can be trusted.

In sum, the two factors that could conspire to select a less informative form as the base in Korean are: (1) the predictability asymmetry between the suffixed and unsuffixed forms is smaller than we might have expected, owing to gaps and asymmetries in the Korean lexicon, and (2) small differences in predictability may be overwhelmed by substantial differences in the availability of forms to construct input pairs for the morphological learner. Forced by these considerations to choose the unsuffixed form as the base, speakers must then learn to predict which obstruent an unreleased [t̃] corresponds to on a noun by noun basis, leaving the oblique forms open to overregularization. Since the predominant pattern in the lexicon is for [t̃] to correspond with [s] (or [ʃ] in palatalizing environments), this is the pattern that is winning out.

An interesting (but unverified) prediction of the current approach is that new, previously impossible noun paradigms could arise, parallel to the change we saw in Lakhota and to the cases discussed by Hayes (1998). Suppose that a particular noun is relatively frequent in a particular case—for example, a word for a duration may occur often in accusative adverbial constructions, or a word for a place may occur often in locative constructions. This would allow it to be memorized as an exception to the [t̃] → [s] pattern for the accusative or locative. When adding the nominative marker *-i*, however, the speaker is unable to use the accusative form to

project the correct consonant, because the accusative is not the base form. Thus, we predict that “inconsistent” paradigms of the following sort might arise:

(86) A potential inconsistent paradigm in Korean

<i>unaffixed</i>	[...tʰ]
<i>nom.</i>	[...ʃ-i]
<i>acc.</i>	[...tʰ-i]

Stated differently, the prediction is that there may be nouns for which a speaker prefers the more conservative, etymologically correct form for some cases, but the newer, analogically created form for other cases.

An example of this sort may occur with the word for ‘field’. This word is [patʰ] in its isolation form, and its etymologically expected final segment is an aspirated *t* (/patʰ/). In an experimental study on variability in the treatment of final consonants, H.-J. Lee (Lee, in progress) asked ten native Korean speakers to produce nouns before a variety of different case markers: some beginning with [i], such as *-in* (topic), some with [i], such as *-i* (nominative), and some with [e], such as *-e* (dative). In the topic form, none of Lee’s participants used the etymologically expected [tʰ]; all of the responses were analogical, half with [tʰ̃] and half with [s]. In the dative, however, which is also sometimes used with a locative meaning, eight out of ten of the participants used the more conservative [tʰ] form. This suggests the following, inconsistent paradigm:

(87) An inconsistent paradigm in Korean

<i>unaffixed</i>	[patʰ]
<i>top.</i>	[patʰ̃-in]/[pas-in]
<i>dat.</i>	[patʰ-e]

Inspection of Lee’s data shows that such discrepancies seem to exist for many words, to varying degrees. These inconsistent paradigms are problematic for a theory that attempts to derive all surface forms from the same UR whenever possible, in the same way that the Lakhota examples in the previous chapter are. What UR can we set up for the new [tʰ] ~ [s] ~ [tʰ̃] paradigm? And more importantly, what UR were the first speakers who uttered [paʃ-i] using that allowed them to change the nominative and not the locative?<sup>9</sup> Such a change is to be expected, though, under a model in which speakers choose a single form in the paradigm as the base, and then derive the remaining forms using separate morphological rules, each with a potentially different set of stored exceptions.

It is worth mentioning that another famous analogical change that may be similar to the Korean change is one that has occurred in Maori passive (Hohepa 1967; Hale 1973; Hyman 1975, pp. 184-185; Hock 1991, pp. 200-202; Barr 1994, pp. 468-477; Kibre 1998). In this case, a historical word-final deletion of consonants caused a massive neutralization in unaffixed forms (all consonants → ∅). As in Korean, a suffixed form like the passive would be much more informative, since it preserves the identity of the stem-final consonant.

<sup>9</sup>I cannot preclude the possibility that the first speakers to introduce analogical forms used analogical forms in all cases (*patʰ̃-in/pas-in* ‘field-TOP’, and also *patʰ̃-e/pas-e* ‘field-DAT’), but that the analogical forms failed to catch on in the dative. This scenario does not seem likely, however, since it is unclear why a subsequent generation of speakers would adopt the innovative nominative form but reject the innovative dative, choosing an inconsistent paradigm when a more consistent one was available in their input data.

(88) Older stage of Maori

base	passive	gloss
<i>afi</i>	<i>afitia</i>	'embrace'
<i>hopu</i>	<i>hopukia</i>	'catch'
<i>tau</i>	<i>tauria</i>	'come to rest'
<i>waha</i>	<i>wahajia</i>	'carry on back'

What has happened, however, is that the passives of many words have changed, so that they now use *-tia*, or have at least gained *-tia* variants (e.g., *wahajia* > *wahatia*). It appears, therefore, that rather than using the more informative passive form as the base, Maori speakers instead used the unsuffixed form as the base, and were forced to memorize or guess what consonant should appear in the passive (*-tia*, *-kia*, *-ria*, etc.). As shown by Sanders (1990), *-tia* is the statistically predominant pattern, and would be the most reliable rule for creating passives. Like the Korean change, the Maori change could be handled in the informativeness approach if there is a substantial difference in the frequency or availability of verbs in these two forms. Whether or not this is actually the case in Maori remains an open question; Kibre (1998) notes that Maori passives are used in a considerably wider array of contexts than English passives (see also Chung 1978), but I do not know whether this means that they actually occur as frequently as actives, or whether it is just a smaller asymmetry than in English.

Crucially, the skewing based on token frequency proposed in this section would most likely not change the results in the previous chapters for Yiddish, Latin, or Lakhota. In the case of Yiddish, the base was the 1sg form, which, although not as frequent as the 3sg form, is probably the next most frequent form. There are no available corpus counts for Yiddish,<sup>10</sup> but Bybee (1985, p. 71) lists some equivalent counts from Spanish, which we may take as a rough estimate. She finds that from a written corpus of adult Spanish (Juilland and Chang-Rodríguez 1964), the 3sg is the most frequent form (44% of the tokens), while the 1sg is second (23%); approximately the same proportion also holds for a corpus of Spanish spoken by children (Rodríguez-Bou 1952) (3sg: 41%, 1sg: 24%). Thus, although the 3sg is significantly more frequent, the 1sg also occurs frequently enough to allow generalizations based on more than just a few input pairs, and could still be chosen as a base form.

In the case of Latin, the sample counts in section 4.4.2 (p. 70) showed that on the whole, oblique forms are at least as frequent in Latin as the nominative form is. If frequency counts on larger corpora and larger numbers of words continued to show the same pattern as in Table 4.1, then the confidence limit adjustment would not affect the results for Latin, either.

Finally, in the case of Lakhota, I argued that it was a second person form that acted as the base in verbal paradigms. This result seems most in danger of being affected by the proposed skewing for confidence. Bybee's counts for Spanish show that the 2sg form is quite a bit lower in token frequency, with 16% of the tokens in the written adult corpus, and 11% in the spoken child corpus. However, I believe that morphological differences between Lakhota and Spanish would make the 2sg form more frequent in Lakhota than in Spanish. In particular, the 2sg form is used not only for statements and questions, but also for imperatives, making 2sg sentences

<sup>10</sup>The equivalent counts from the German portion of CELEX are also uninformative, because they fail to differentiate homophonous forms. For example, an ambiguous form like *geben* is counted in the token frequency of the infinitive, the 1pl present indicative, the 3pl present indicative, and so on, meaning that all homophonous forms are listed with the same token frequency.

three-ways ambiguous, as in 89:

- (89) *Shunka he walaka-he*  
 dog DET. see-2sg. (female speaker)  
 You see the dog. / Did you see the dog? / Look at the dog!

Bybee's Spanish counts do not include imperative forms, but they are probably quite frequent, especially in child-directed speech.<sup>11</sup> Berman (1985, p. 268) notes that in Hebrew, children acquire imperative forms very early, with boys using the masculine imperative and girls using the feminine imperative, in spite of the fact that they often involve difficult consonant clusters. Thus, as with Yiddish, even if 3sg forms are more frequent, it appears that second person forms are not so uncommon that they would be penalized too severely by the confidence limit adjustment.

### 6.2.2 Local Markedness

An argument that is often put forth in favor of the role of markedness in analogical change is the fact that when the meaning of individual lexical items gives them unusual markedness relations, analogy can go in the opposite direction. This has been argued most explicitly by Tiersma (1982), who called this phenomenon *local markedness*. Tiersma showed that in particular dialects of Frisian, Dutch, and German, nouns denoting objects that occur most frequently in the plural (such as eyes, arms, geese, etc.) have sometimes undergone paradigm leveling to the plural form. This is taken as evidence that any explanation of analogical change must take into account the markedness or frequency of forms not only in general, but also on a word-by-word basis. The single surface base hypothesis, on the other hand, precludes such word-by-word effects, because the organization of the grammar requires that the same base be used for all lexical items. How can these two viewpoints be reconciled?

We might start by noting that there is very little evidence of local markedness effects in any of the cases discussed here. In Yiddish, paradigm leveling affected virtually every verb in the language, with only one possible case of local markedness: the verb *gefeln* 'to be pleasing' is apparently derived from a 3sg instead of a 1sg form. This could have been caused by local markedness in Tiersma's sense, but it may also be caused by a far more drastic version of local markedness in which lexical items do not occur at all in certain forms (defectiveness). If a mere imbalance is enough to reverse the direction of leveling, then why is this the only such form? Similarly, the Latin *honor* analogy was remarkably complete; all of the words of the right phonological shape underwent the *s* > *r* change in the nominative, with no evidence that more "agentive" nouns retained *s* merely because the nominative was (even) less marked for these forms. Furthermore, words that did not meet the criteria consistently failed to undergo leveling; the only possible exceptions were neuter nouns with masculine doublets, but never words with unusual markedness relations. Finally, in the case of Lakhota, there is no evidence of words in which an unmarked 3sg caused an analogy based on the 3sg, such as extending the *-e* (*chepe*, \**chepe-pi*) or extending ablaut to an invariant *-e* (*washte*, \**washta-pi*). In these, and

<sup>11</sup>One may speculate further that children probably pay more attention to utterances that are addressed directly to them, both because of the attention, and because they need to understand what is required of them. Anecdotally, it is interesting to note that the vast majority of the Lakhota sentences that Ms. Iron Teeth could recall verbatim from the more archaic speech of older people (such as her grandparents) were commands for her to do various things.



many other cases, paradigm leveling is across the board, affecting all words of the appropriate morphological and phonological classes in the same way. If paradigm leveling could choose different bases for different lexical items, then such clean changes should never happen.

Even in the case of Frisian, it is not clear how good local markedness is as an explanation for the behavior of the particular words that Tiersma discusses. It is true that arms and geese do occur most naturally in the plural, but so do hundreds of other objects in the world. In fact, the nouns discussed by Tiersma seem to be frequent enough that we would expect learners to have had ample exposure to them in both the singular *and* the plural; surely the singular of ‘arm’ is attested often enough that in Bybee’s terms, it should be strong enough to protect itself from leveling. The words that one should really expect to see on Tiersma’s list of levelings are words with both reversed markedness and lower frequency, like ‘kidney’, ‘termite’ or ‘barnacle’.<sup>12</sup>

The model I have proposed here makes the strong prediction that local markedness should not play a role in analogical change. This leaves us without an explanation for Tiersma’s Frisian, Dutch, and German cases. However, it appears that such cases may be quite rare (and inadequately explained by local markedness in any event), so one might hope to find alternative explanations for them.

### 6.2.3 Why a leveling preference?

Another typological preference that is sometimes mentioned is a preference for paradigm leveling (eliminating alternations) over analogical extension (introducing alternations). Mańczak (1958), for example, claims that it is a fundamental tendency of analogical change that root alternations are generally leveled, not extended.<sup>13</sup> The model that I have proposed here, however, does not have any built-in preference to level; it derives leveling simply as the extension of a dominant pattern of non-alternation. Why might this tend to lead to leveling more often than analogical extension?

A comparison of Latin and Korean may help to shed light on this problem. In Latin, rhotacism affects just one segment (/s/), so the only words that exhibit the alternation are those with stem-final [s]. In Korean, on the other hand, coda neutralizations affect all obstruents, so many lexical items exhibit alternations. I conjecture that morphophonemic alternations typically affect only a small subset of the phonemic inventory; rhotacism affects just /s/, umlaut affects only back vowels, palatalization tends to affect just coronals (and often just coronal stridents), and so on.

When the minimal generalization learner is confronted with a language that has such an alternation, it will therefore end up with two competing processes: the rules that produce the morphophonemic alternation (such as [tʰ] → [tʰ-i]), and the general rules that cover the rest of the segments (such as ∅ → [-i]). When the alternation can be learned as automatic phonology, this is no problem—using the general rule to suffix [-i] would produce incorrect \*[...t-i], but this would be fixed by the phonology to yield [tʰ-i]. If, on the other hand, the process cannot be learned as automatic phonology (for example, if sound change or borrowings have reintroduced

<sup>12</sup>The frequency and number-markedness of this set of words is a guess on my part. Also, it is important to bear in mind that the leveling discussed by Tiersma could only occur in words that meet the right environment for the phonological “breaking” process that caused singular~plural vowel alternations in Frisian.

<sup>13</sup>Hock (1991, pp. 235) appears to be rather non-committal about what we should conclude from this fact, pointing out that there are nevertheless a large number of cases in which alternations have been extended. He concludes his discussion by saying that both occur frequently, and must be explained.

surface [ti] into the language), then the [tʰ] → [tʃ-i] rule and the ∅ → [-i] rules will yield different outputs.

The preference for leveling, then, comes from the fact that when an alternation can be produced only using the smaller, more local rule ([tʰ] → [tʃ-i]), the “regular” output produced by the more general process (∅ → [-i]) will be a strong competitor, and will provide the impetus for leveling. Whenever alternations affect only a subset of segments and a minority of the words in the language, they will be threatened the more general non-alternating pattern. The end result is that this model captures common traditional insight about paradigm leveling: once phonological alternations are no longer surface true and must be learned as morphophonology, they will be open to leveling because the general, dominant pattern of simple affixation cannot produce the alternation. Furthermore, since phonological alternations, even when surface true, often involve just a subset of the segment inventory, non-alternation is often the dominant pattern in languages. I conjecture that this suffices to predict whatever preference there may be for leveling over analogical extension.

#### 6.2.4 Frequency, forgetting, and overregularization

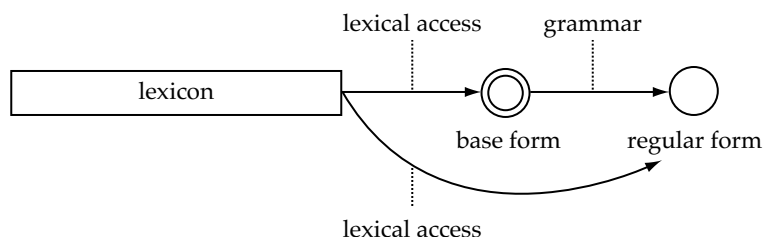
Throughout this thesis, I have been assuming that speakers have two means of producing non-basic forms: they may either use a lexically listed form (encoded here as a word-specific rule), or they may use their grammar to derive a form. For regular forms, these two routes yield the same answer, but for irregular (exceptional) forms that the grammar cannot derive, they yield different results. This was illustrated in Figure 1.1 in chapter 1, and is repeated in Figure 6.4.

In the usual case, it is assumed that a lexically listed form takes precedence over, or *blocks* its grammatically derived competitor (Aronoff 1976). The mechanism for change under this model is *overregularization*—exceptional forms that could be produced only by retrieving them from memory are replaced with their regular, grammatically derived rivals (Marcus, Pinker, Ullman, Hollander, Rosen, and Xu 1992).

There are numerous reasons why blocking might fail, leading to overregularization. For example, the speaker might not know the exceptional form, or she might know it but fail to retrieve it in time to block the grammatically derived form. It is widely agreed that the more frequent an exceptional form is, the easier it is to retrieve, and the better it is able to block the synthesized output (Bybee 1985; Baayen, Dijkstra and Schreuder 1997; Pinker 1999, pp. 129-131). In principle, it would not be difficult to incorporate these “accessibility” effects directly into the current model to produce a more explicit production model of overregularization and language change. One way to do this would be to redefine the probability of using a word-specific rule (currently assumed to be 1) to be related to the token frequency of the output of the rule. This would mean that very frequent outputs would be produced with near 100% reliability, while infrequent outputs would be more likely to resort to the output derived by the grammar of generalized rules.

Such a model would be useful in capturing the overregularizations errors seen in children (and even adults), but it would not be sufficient to capture the historical changes that I have discussed here. In both Yiddish and Latin, paradigm leveling affected all of the relevant lexical items, not just the less frequent ones. What mechanism allowed speakers to replace even very high frequency forms like ‘he takes’ (\**nimt* > *nemt*) or ‘they know’ (\**visən* > *veysən*)? The usual assumption, dating back at least to Paul (1920), is that such changes should be attributed not

- a. Two (identical) ways to produce non-basic, regular forms:



- b. Two (non-identical) ways to produce non-basic, exceptional forms

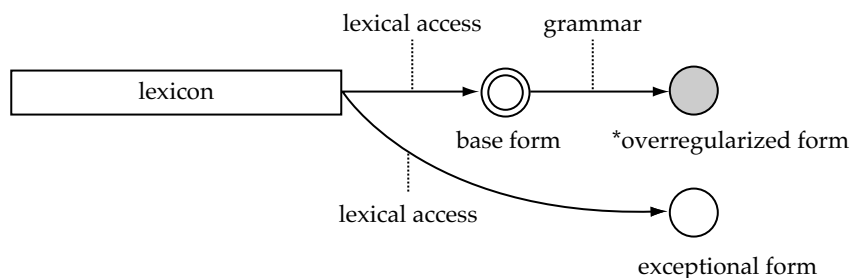


Figure 6.4: Routes for deriving different types of words

to the adult production system, but rather to the errors that children make. For children, no words have extremely high token frequency yet, and it is far more likely for a child to make an error on ‘they know’ than for an adult to do so. However, as Marcus et al. (1992) show, the number of child errors on high frequency verbs is never all that high, even at the height of the overregularization stage (though cf. Maratsos 2000 for a challenge to these counts). Furthermore, one may question to what extent children’s errors are really able to propagate themselves; adults seem impervious to them, and even if younger children are impressed by them, they still constitute only a fraction of their linguistic input.<sup>14</sup> If imperfect acquisition by children was really the cause of such wholesale paradigm levelings, we would expect to sometimes encounter older children (or even adults) who had never emerged from the overregularizations. However, this seems not to happen, at least in English; every child examined by Marcus et al. eventually stopped producing more than the occasional overregularization. I believe that there must be some factor other than low accessibility and imperfect memorization contributing to the widespread levelings seen in Yiddish or Latin.

In the case of Yiddish, it is quite likely that sociolinguistic factors encouraged speakers to favor overregularized forms. If my conclusions about the dating of paradigm leveling in section 2.1.2 are correct, then leveling must have occurred at a time when Yiddish was beginning to develop as a written language with its own literature, distinct from German. This may have

<sup>14</sup>One possible response to this would be to posit that wholesale paradigm levelings can only occur at moments of social upheaval or violent discontinuities, in which access to adult forms is limited and child errors assume abnormal importance. This seems not to have been the case in either Yiddish or Latin, however.

provided exactly the environment necessary for speakers to be willing to produce and accept overregularized forms at a far greater rate than in, say, present day English. As distinguishing features go, such overregularizations would be easy to produce and remember, since, under this analysis, they were the grammatically preferred forms.

The suggestion, then, is that at least for adults, morphological blocking and the failure to block involve more than just lexical access; they involve sociolinguistic factors like normative pressure, the desire to maintain conservative forms, the desire to mark sociological affiliation through linguistic innovations, and so on. A more complete model that takes all of these factors into account is far beyond the scope of this thesis.

### 6.3 Scaling up: discovering local bases

As I have reiterated throughout this thesis, the part of the proposed model that is responsible for predicting many of the observed asymmetries is the single surface base hypothesis. According to this hypothesis, learners are restricted to selecting just one form as the base within the paradigm, and all other forms must be derived from the same base. As discussed in chapter 1, this assumption is appealing from a learnability point of view, because it greatly reduces the number of possible grammars that must be explored. I did admit at that time, however, that the paradigms under discussion here have all been relatively small and local—a single tense or a small set of case and number forms. In this section, I will present data that appears to require a more sophisticated paradigm structure, composed of subparadigms each with its own local base. I will then discuss several possible ways in which the procedure proposed in chapter 3 could be extended to accommodate such cases.

#### 6.3.1 The need for local bases

When we look at larger paradigms, involving more tenses, moods, and so on, it often appears that we need local bases for each sub-paradigm (something like the traditional idea of *principal parts*, or multiple stems). Furthermore, Bybee (1985) and others have noted that analogical changes tend to operate within a particular subpart of a paradigm (a particular tense, aspect, or mood, or in the Polish example from chapter 1, just within diminutives), providing further evidence that there must be local bases. The set of forms that will require a local base cannot be assumed to fall within any particular tense, mood, or aspect, however. Aronoff (1994) points out that in Latin verbs, a certain set of forms seem to be derived from a particular local base (the so-called “third stem”), but these forms do not fall within any natural class.

A nice example of the need for local bases comes from the Spanish present subjunctive (Harris 1969; Butt 1997). In an earlier stage of Spanish, a phonological rule that deleted velar stops before front vowels created alternations within verb paradigms.

(90) *cono[s]er* ‘to know’

<i>imper.</i>	cono[s]e	
<i>pres. ind.</i>	<b>cono[sk]o</b>	cono[s]emos
	cono[s]es	cono[s]éis
	cono[s]e	cono[s]en
<i>pret.</i>	cono[s]í	cono[s]imos
	cono[s]iste	cono[s]isteis
	cono[s]ió	cono[s]ieron
<i>pres. subj.</i>	<b>cono[sk]a</b>	<b>cono[sk]amos</b>
	<b>cono[sk]as</b>	<b>cono[sk]áis</b>
	<b>cono[sk]a</b>	<b>cono[sk]an</b>

Within the present indicative paradigm, this led to a neutralization of [k] with  $\emptyset$  in most of the paradigm, and created what looks like unpredictable insertion of a [k] in the 1sg form. In spite of this neutralization, the optimal base within the present indicative paradigm turns out to be the 1pl form,<sup>15</sup> meaning that the mapping to derive the 1sg form often involves needing to decide whether or not to insert a velar. Some support for this analysis comes from the fact that historically incorrect velars have been added to new verbs, such as in the verb *venir* ‘to come’:

(91) *venir* ‘to come’

<i>imper.</i>	ven	
<i>pres. ind.</i>	<b>ven[g]o</b>	venimos
	vienes	venís
	viene	vienen
<i>pret.</i>	vine	vinimos
	viniste	vinisteis
	vino	vinieron
<i>pres. subj.</i>	<b>ven[g]a</b>	<b>ven[g]amos</b>
	<b>ven[g]as</b>	<b>ven[g]áis</b>
	<b>ven[g]a</b>	<b>ven[g]an</b>

One might think, given this data, that the analysis that Spanish speakers had come up with was to use a form like the infinitive as the base, and then the mappings to the forms with back vowels each included a set of rules adding a velar (1sg indicative: *imos* → *go* / \_\_\_#; 1sg subjunctive: *imos* → *ga* / \_\_\_#; etc.). There is another way to describe the same facts using a local base, however. In particular, we could say that the 1sg indicative is derived from the infinitive, and then that the subjunctive is derived from the 1sg indicative (1sg indicative: *imos* → *go*; 1sg subjunctive: *o* → *a*). There is historical evidence that this is in fact the analysis that speakers

<sup>15</sup>I have used the model proposed here to verify this computationally, using a database of Spanish verbs taken from the 5.5 million word LEXESP corpus (Sebastián, Cuetos, and Carreiras, in press). It turns out that the problem of predicting velar insertion and other changes that take place in the singular, such as diphthongization and raising, are all much easier than the problem of predicting which conjugation class a verb belongs to (-*ar*, -*er*, or -*ir*). Therefore, the model chooses a base form that unambiguously reveals the conjugation class (namely, the infinitive, 1pl or 2pl), and has to learn how to predict velar insertion. Bybee (1985, p. 60) claims that the 3sg is the base of Spanish paradigms and that the remainder of the forms can be derived from it, but this is only true for the first conjugation example (*cantar* ‘to sing’) that she uses; in the other two conjugations, the theme vowel is neutralized to *e* in the 3sg. Much of Bybee’s evidence in favor of the 3sg in Spanish can be reinterpreted as showing that the 3sg is a local base for the other singular forms, not the global base of Spanish verb paradigms.

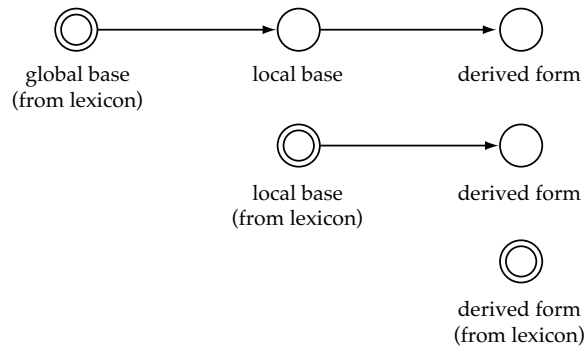


Figure 6.5: Three possible ways to produce a non-basic form using a local base

were using. There is an extremely irregular Spanish verb *caber* ‘to fit’, which has the 1sg form *quepo* [kepo], with a completely idiosyncratic vowel change and stop devoicing. This irregularity has been extended from the 1sg to the entire present subjunctive:

(92) *caber* ‘to fit’

<i>imper.</i>	cabe	
<i>pres. ind.</i>	<b>quepo</b>	cabemos
	cabes	cabéis
	cabe	caben
<i>pret.</i>	cupe	cupimos
	cupiste	cupisteis
	cupo	cupieron
<i>pres. subj.</i>	<b>quepa</b>	<b>quepamos</b>
	<b>quepas</b>	<b>quepáis</b>
	<b>quepa</b>	<b>quepan</b>

This example, like Aronoff’s Latin third stem example, shows that speakers know that some local paradigms may be productively derived from forms other than the global base. In other words, there is no question that we need local bases.

How would a system with local bases be used to produce forms? In the simpler case of a single paradigm, we saw that there are two ways to produce a form: it may either be memorized (provided that it is frequent enough), or it may be synthesized, sometimes yielding different results (Figure 6.4). The same is also true with local bases, but when there are local bases, there may be more steps in the derivation, as shown in Figure 6.5. The prediction, then, is that two types of regularization are possible: leveling of a subparadigm to its local base, and leveling of a subparadigm to the global base. Leveling to the global base requires that a lexically listed local base be inaccessible, and have been regenerated starting from the global base. What is excluded under this model, therefore, is a leveling in which some forms in the subparadigm are overregularized on the basis of the global base, while others continue to be derived from an irregular, listed local base. The reason is that if a local base is inaccessible for some of its derivatives, it should be (on average) inaccessible for all of them.

### 6.3.2 Extending the model to find local bases

In order to discover local bases, we need two things. First, we need a mechanism that finds not only the single optimal base form for the entire paradigm, but also other local pockets of strong predictability that might diagnose local bases. In addition, we need the model to be able to discover which forms should be derived from which bases, given that this may be somewhat arbitrary (as in the case of the Latin third stem). This is shown schematically in Figure 6.6, which indicates that a remote part of the paradigm that is strongly predictable from the 1pl remains attached to it, while another remote part of the paradigm with common unpredictable features “break off” for be derived by a more local base.

To a certain extent, the procedure for discovering local bases is a natural extension of the procedure described in chapter 3 for finding global bases; we compare numerous possible mappings and find the forms that provide the most reliable mappings to the remaining forms. There are several possible approaches.

The procedure outlined in chapter 3 is designed to find a single global base. It does this by determining which form has the best average score in deriving the remainder of the paradigm. Recall how this worked in the case of Latin; the scores for each form according to the “percent correct” metric are given in Table 6.2. The rows in the table represent all of the subgrammars based on a certain input form (all of the subgrammars based on the nominative, on the accusative, etc.), while the columns represent all the ways to derive a form as an output. As can be seen in the rightmost column, the dative performs, on average, best in deriving the other forms, and would be selected as the base (indicated by shading). This is not to say, however, that the dative is the best base for every single form in the paradigm. For example, in the nominative column, we see that the nominative can be predicted somewhat more accurately from the accusative form (94.5% accuracy) than from the dative form (91.7% accuracy), because the nominative and accusative are identical for all neuter nouns; this is shown in bold type.

What we want to do in establishing local bases, then, is to allow forms to delink from the global base and reassociate themselves to forms that could derive them more efficiently. In this case, the nominative and accusative forms could both find better bases elsewhere in the paradigm. So, one possible algorithm would be to allow a second “clean-up” stage of base identification: after the global base has been established (using the procedure in chapter 3), we then check each form in the paradigm to see if we could do better by deriving it more locally from some other form. If so, we “demote” the form so that it is derived by a local base. In the

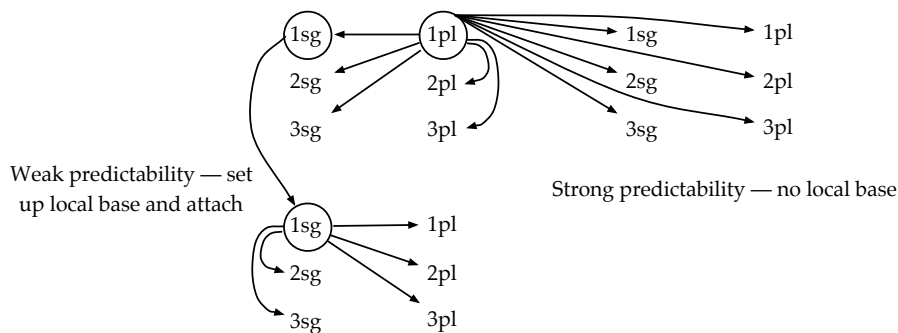


Figure 6.6: Setting up local bases by finding pockets of high reliability

Table 6.2: Percent correct in deriving Latin noun paradigms

↓In/Out→	nom.sg.	gen.sg.	dat.sg.	acc.sg.	abl.sg.	nom.pl.	average
nom.sg.		0.818	0.771	0.870	0.854	0.797	0.822
gen.sg.	0.929		<b>0.972</b>	0.964	0.957	0.945	0.953
dat.sg.	0.917	<b>0.982</b>		0.963	<b>0.966</b>	<b>0.957</b>	<b>0.957</b>
acc.sg.	<b>0.945</b>	0.937	0.921		0.931	0.924	0.932
abl.sg.	0.917	0.974	0.966	<b>0.968</b>		0.949	0.955
nom.pl.	0.893	0.941	0.943	0.945	0.943		0.933

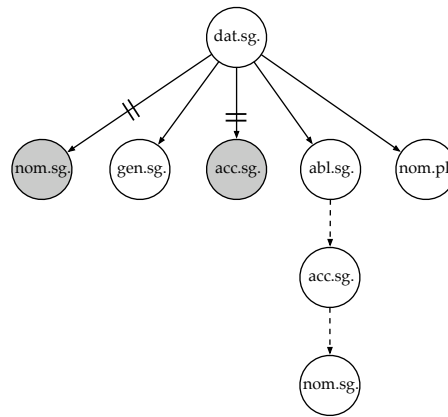


Figure 6.7: Result of “local demotion” for Latin noun paradigms

case of Latin, the nominative is best derived from the accusative, so it would get reassociated to the accusative as a local base, while the accusative in turn is best derived from the ablative. The resulting, somewhat more articulated paradigm is shown in Figure 6.7. (In this and subsequent figures, I borrow some notations from suprasegmental phonology: a double broken line indicates a form that has been “demoted”, a dashed line indicates a derivation that has been added in the “demotion” stage. Suboptimally derived forms in search of a better base are shaded gray.)

When we apply this procedure to Spanish verbal paradigms, however, we run into problems. The equivalent accuracy scores for the Spanish present tense indicative paradigm are given in Table 6.3. Here we see that the infinitive, 1pl, and 2pl all do equally well in predicting the remainder of the paradigm, though the confidence limit adjustment discussed in section 6.2.1 would probably favor the infinitive over these other two forms. These forms are the best on average, but there are still a number of particular forms that we could do better for; the 1sg, 2sg, 3sg, and 3pl are all highly interpredictable because they share properties like mid-vowel diphthongization and raising. These “unhappy” forms are the ones shaded gray in Figure 6.8.

Now consider trying to demote or reassociate individual forms, as we did for Latin in Figure 6.7. Here we run into a problem of ties: the 1sg form could be equally well derived from the 2sg, the 3sg, or the 3pl; the 2sg form could be equally well derived from the 3sg and the 3pl, and so on. the 3sg and 3pl are 100% mutually predictable, leaving us unable to decide which to demote, while the 2sg is equally well predictable from both the 3sg and the 3pl, leaving us uncertain which to derive it from. What should we do in this case? If we were to allow multiple



Table 6.3: Percent correct in deriving Spanish present tense paradigms

↓In/Out→	1sg	2sg	3sg	1pl	2pl	3pl	infin.	average
1sg		0.895	0.896	0.867	0.867	0.896	0.867	0.881
2sg	<b>0.986</b>		0.995	0.740	0.740	0.995	0.740	0.866
3sg	<b>0.986</b>	<b>1.000</b>		0.750	0.750	<b>1.000</b>	0.750	0.873
1pl	0.902	0.908	0.909		<b>1.000</b>	0.909	<b>1.000</b>	<b>0.938</b>
2pl	0.902	0.908	0.909	<b>1.000</b>		0.909	<b>1.000</b>	<b>0.938</b>
3pl	<b>0.986</b>	<b>1.000</b>	<b>1.000</b>	0.750	0.750		0.750	0.873
infin.	0.902	0.908	0.909	<b>1.000</b>	<b>1.000</b>	0.909		<b>0.938</b>

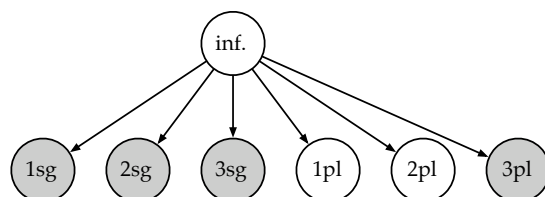


Figure 6.8: Results of global base selection for Spanish present tenses

reassociations, we would end up with the rather complicated paradigm in Figure 6.9.

Paradigm structures of this type seem not only needlessly complicated, but also insufficiently predictive. The most interesting results in the preceding chapters came from the restriction that grammars be asymmetrical and have a single base; allowing paradigms like those Figure 6.9 removes many of these asymmetries. If we want to preserve the restrictiveness of the single base hypothesis as much as possible, we need some additional conditions on how local bases are assigned.

A sensible restriction would be to require that each form be derived from at most one other form, ruling out the multiple and circular derivations in Figure 6.9. Under this restriction, forms are reassociated to just one of their optimal local bases. (Note that this restriction also means

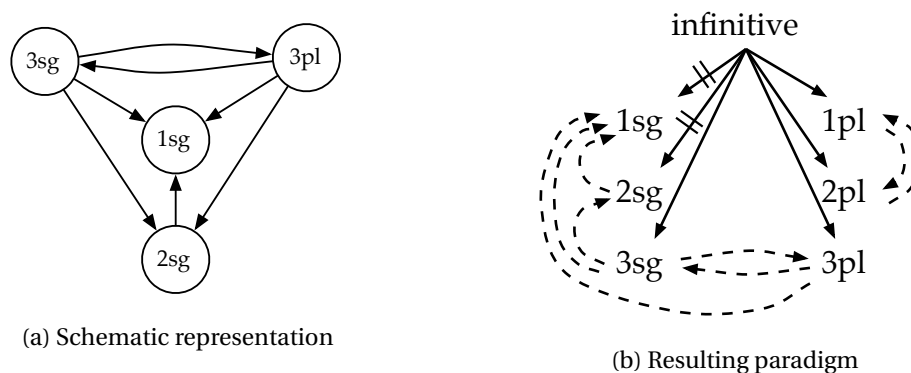


Figure 6.9: Circularity and ties in the Spanish present tense paradigm

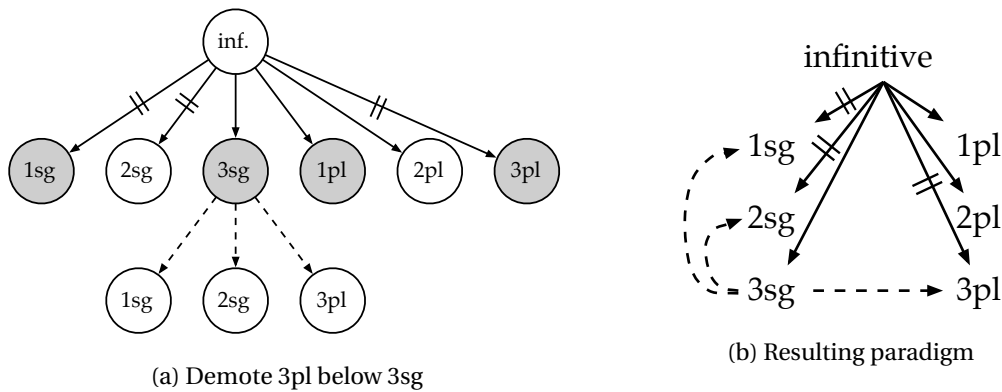


Figure 6.10: 3sg as local base of Spanish paradigm

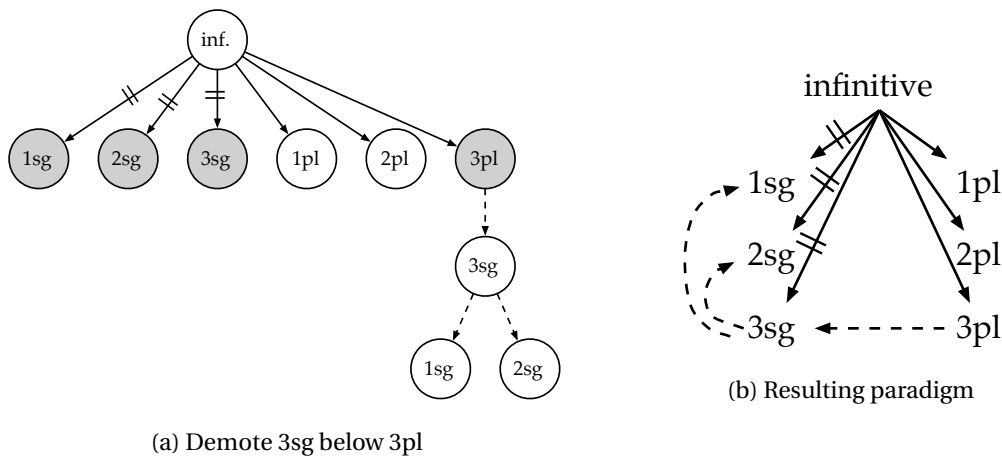


Figure 6.11: 3pl, 3sg as local bases of Spanish paradigm

that one of the unhappy forms must remain attached to the global base, in order to “break into the circle” of Figure 6.9.) This alone is not enough, however. The paradigms in Figures 6.10 and 6.11 both satisfy this restriction, but the one in Figure 6.10 seems simpler and more restrictive than the one in Figure 6.11. For ease of interpretation, each paradigm is shown in two notations: a tree-like notation showing demotions, and a more traditional paradigm-like notation. The problem is that the 3sg and 3pl forms are mutually predictable, so there is no way to decide which one to derive from the other.<sup>16</sup>

Intuitively, we want to find the grammar that allows each form to be derived as reliably as possible, but also makes use of the smallest number of local bases. The most promising way to do this is to let the base selection algorithm proceed recursively, rather than letting each suboptimally derived form float off individually in search of a better local base.

<sup>16</sup>Crucially, the confidence limit adjustment favoring better-instantiated forms could not help to decide here, because the number of (3sg,3pl) pairs is the same no matter which is the input and which is the output.

Table 6.4: Competition for basehood of 1sg,2sg,3sg,3pl subparadigm

↓In/Out→	1sg	2sg	3sg	3pl	average
1sg		0.895	0.896	0.896	0.896
2sg	<b>0.986</b>		0.995	0.995	0.992
3sg	<b>0.986</b>	<b>1.000</b>		<b>1.000</b>	<b>0.995</b>
3pl	<b>0.986</b>	<b>1.000</b>	<b>1.000</b>		<b>0.995</b>

The way this would work is as follows: first, the algorithm compares the entire paradigm, and selects a global base, as before. Then, each form is examined to see whether it can be derived more reliably by a form other than the global base. If it can, it is submitted to the second round, in which a secondary base is chosen. In the case of Spanish, this would mean that the 1sg, 2sg, 3sg, and 3pl must compete to become the local base of a subparadigm consisting of just these forms. The numbers for this competition are shown in Table 6.4. We see from the table that the 3sg and 3pl are equally good at deriving the other forms; in this case the higher token frequency of the 3sg would probably be the deciding factor favoring it over the 3pl, since there are more (3sg,2sg) and (3sg,1sg) pairs than there are (3pl,2sg) and (3pl,1pl) pairs available in the input set.

Once a local base is identified for the subparadigm, it stays where it is and the remaining forms are reassociated to it. At this point, all of the forms are being derived by one of their optimal bases, so there is no need to iterate further. This procedure yields the paradigm in Figure 6.10, and in general it will favor “flatter” paradigms because at each step in the process, it tries to find a single base form that can derive all of the remaining forms.

The procedure outlined here is a first pass at identifying local bases. Finding the optimal local bases given a set of predictability values is a hard problem, and there is clearly much more work to be done in understanding how local bases are identified. I anticipate that two additional restrictions may be helpful or even necessary in tackling the problem of larger paradigms, with multiple tenses, aspects, etc. The first is some sort of restriction on the batches of forms that are grouped together for the purpose of finding local bases. It is commonly observed (e.g., Bybee 1985, chap. 3) that forms within a particular tense/aspect/mood are more cohesive than forms from more distant parts of the paradigm. I have avoided making use of morphosyntactic features in choosing base form within relatively small paradigms, because it seems that no particular combination of morphosyntactic features guarantees that a form will serve as the base in a particular language. It would not be inconsistent with this approach, however, to limit the sets of forms that can be grouped together as subparadigms, by restricting the types of morphological rules that the system is allowed to consider. For example, it would be quite reasonable to prohibit the system from considering (2sg present indicative active, 3pl perfect subjunctive passive) pairs, since this would require a rule that simultaneously changes person, number, tense and mood features. The question of which mappings are possible is orthogonal to the question of how bases are selected from among a set of mappings. Nevertheless, in extended paradigms, these two factors would interact crucially in determining the final structure of the paradigm.

The second restriction on local bases that may be useful is some sort of predictability threshold for positing a local base. In the Latin noun paradigms above, the predictability advantage for deriving the accusative from the ablative (a local base) instead of from the dative (the global

base) is tiny— 0.968 vs. 0.963. It seems unlikely that speakers complicate their paradigm structure enormously for such minuscule gains in predictability. It is an open question how large a threshold would achieve the desired balance between simplicity and effectiveness. An interesting sidenote, however, is that allowing more deeply embedded paradigm structure could help to alleviate the problem of “inferring nothing from non-base forms” mentioned at the end of the previous chapter. If Spanish 2sg forms are derived from a local base (the 3sg) which in turn is derived from a global base (the infinitive), then when the global base is not known, a speaker needing to produce a 2sg form would be less impaired, since it is likely that at least the local base (the 3sg) would be known. Thus, there may be a trade-off between choosing a paradigm with less structure, and a paradigm that is more likely to provide the input necessary to derive an unknown form.

The local bases algorithm proposed here is not likely to change the results in any crucial way for the cases discussed in the previous chapters. In the case of Latin, it would result in the nominative being derived locally from the accusative (mainly because they are identical for neuter nouns), which is still compatible with the result in chapter 4 that the nominative was rebuilt on the basis of oblique forms. In the case of Yiddish, the local base algorithm would likely create sub-paradigms for the plural forms (which would have always had identical vowels in pre-leveling Yiddish), and for the 2sg and 3sg (which were also always identical. In this case, too, the crucial aspect of the analysis (all forms ultimately derived from the 1sg) is preserved. Finally, in Lakota, the unsuffixed forms (1sg, 3sg) would form their own local subparadigm, since they always share the same final vowel. However, the suffixed forms would still be derived from a global 2nd person base form, so in this case, too, the crucial asymmetries observed in chapter 5 would be preserved.

## 6.4 Relation to acquisition evidence

Another important source of evidence about bases that I have not made use of here is acquisition evidence from children’s productions and errors. Bybee (1985) provides numerous pieces of evidence that 3sg forms, which she argues are the universally best base forms, are acquired first by children, and are also systematically substituted for other forms in the paradigm. A careful review of the acquisition evidence would be a logical next step in testing whether language learners really do use a strategy like the one proposed here for learning the organization of paradigms. It should be noted, however, that there are some types of child acquisition data that are probably *not* relevant to this question, including descriptions about which forms are uttered first by children. For example, Bybee (1985, p. 50) notes that Spanish-speaking children typically master 3sg forms first, while Berman (1985, p. 268) claims that for Hebrew-learning children, it is the imperative. The fact that a child can utter a form means only that she has heard it enough to have memorized it, not that she has established it as a base form.

The errors made by children are a much more useful source of data about bases. Bybee cites data from Simões and Stoel-Gammon (1979) that shows that Portuguese-learning children incorrectly extend the vowel of the 3sg indicative to the 1sg. Similarly, Clahsen, Avelado and Roca (to appear) show that Spanish-learning children make systematic errors in diphthongization, substituting the non-diphthongized form of the verb (found in the infinitive, 1pl, and 2pl) for the diphthongized version that should appear in the 1sg, 2sg, 3sg, and 3pl. It is this type

of asymmetry that can help to shed light on the organization that children are imposing on paradigms in the course of acquisition.

## **6.5 Summary**

In this chapter, I have tried to show how the proposed model, or a suitable extension of it, could handle not only the Yiddish, Latin and Lakhota cases discussed in the previous chapters, but also a wider variety of data, including problematic cases and typological tendencies. In some cases, such as Russian, I argued that the model's inability to arrive at the traditional analysis may not as much of a problem one might have thought. In other cases, such as Korean, I argued that more realistic simulations, taking into account not only the informativeness of forms, but also the amount of data available to learners, might help to explain why it is sometimes not the most informative form that acts as the base. Finally, I discussed the need to relax the single surface base hypothesis to accommodate local bases in extended paradigms, suggesting some possible (but tentative) approaches to the problem. In all of these cases, I have discussed ways in which the current approach makes strong, testable, and unique predictions about asymmetries in possible errors and historical changes.

