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Theory and Method

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Introduction

While Geoffrey Pullum has pointed out that it is bad form to introduce a talk – and, one assumes, by extension, a book – with an apology, I have to introduce this book with two.[1] First, much of this introduction would really much more appropriately be called a "Preface." I have nevertheless included this material in this Introduction because in my experience students rarely, if ever, read prefaces, and I think they should read this material (which is why, after all, I've written it). Of course, many of them don't read introductions either, and some of them appear not to read textbooks at all, but I hope all of the preface-reading scholars out there will forgive the slight stylistic infelicity of the Preface sneaking into the Introduction with the understanding that I may have increased student reader ship a few percent by this maneuver.

I had originally subtitled the book "Five Lectures," even though none of what follows was ever delivered, in anything like the form they have here, as a lecture. Indeed, it seems to me that if I attempted to deliver any of the five parts of this book as actual lectures, it would take me many, many more hours to deliver the material than any audience could reasonably be expected to endure – even given the rather loose standards used in academia in this domain.

Why then did I nearly choose this obviously misleading label? This is, I fear, a rather long story. The material which follows began to take shape in the mid-1990s. It is almost certainly a decade later than that now, as you hold the book in your hands. The book has not been dormant during most of this decade, but was rather subject to regular, but generally unsatisfying, rewrites, reorganizations, and reconceptualizations. It has long plagued me just why, precisely, this book, which concerns material near and dear to my heart, material to which I have dedicated much of my adult life, has proven so difficult to bring to fruition. In the end, I have concluded that a major problem with much of what I had written was that it was not in my own voice – i.e., when I myself read it, it did not sound like I would have said those things in that way. Doubtless at least

[1] By the time it's over, the reader will probably feel I should have apologized for a lot more stuff, I'm afraid. But I'll limit myself to two at this point.

part of this disconnect arises because my own native dialect is somewhat distinct from that of modern North American academic prose.

By using the label "lecture" I intended to prepare the reader for the often informal nature of the prose which will follow, as well as for the essentially concept-driven (rather than data-driven) nature of much of the exposition. The positive side-effect of this is that the book should be a relatively easy read. In any event, it appears that this is the tone I feel comfortable using in discussing these matters, and if at least *I* am to enjoy this book, it is the tone which it must have. If you are a stickler for the niceties of academic prose style, you will be dismayed, time and time again, by what follows.

The greatest risk that arises in writing a book of this type in relatively colloquial style, and doubtless the impediment to my applying this solution years ago, is that the book will appear more superficial in content than is, I hope, the case. My goal here is to deal with issues of some complexity in relatively straightforward language. This may not in fact be possible – the invitation by use of colloquialism to interpret technical terminology as everyday usage may constantly impede coherent interpretation of what follows, though of course I sincerely hope that it does not.

The optimal reader of this book would probably have been exposed to the basic methods and practices of historical linguistics – sound change and reconstruction problems, issues in subgrouping, and the like – as well as to the basics of modern theoretical linguistics.[2] In general, I think of this book as a maximally useful follow-up to the type of historical linguistics to which one might be exposed in a good "Introduction to Linguistics" course, or perhaps in a one-semester undergraduate "Introduction to Historical Linguistics" course, or for the student who has worked his/her way through one of the standard, brief, introductory textbooks to the field. While I realize that most historical linguists would be loath to sacrifice the already limited time allotted in a standard historical intro course to material which goes beyond that of the standard texts, as this one clearly does, it is my hope that the kinds of issues raised in this volume would spark the kind of serious "big-picture" consideration of the technicalia introduced in the course of the semester which would allow students to come to a much deeper and more firmly-grounded understanding of the historical linguistics enterprise.

The book should not, however, be used without serious consideration of the following fact: virtually no one agrees with most of what follows, whether they come primarily from the "theoretical" or "traditional" sides of the historical linguistics continuum. If you are an instructor seeking a textbook for purposes of the type outlined in the preceding paragraph, do not hastily grab this book based on its size, price, writing style, or overall simple appearance thinking you've found the perfect solution to your textbook needs UNLESS you're prepared to engage with the controversial ideas which are presented here. This

[2] A sketch of the latter can be found in Hale and Kissock's introductory textbook, available at http://modlang-hale.concordia.ca/LING200.html.

book does not depict the current "state of the art" of advanced or intermediate-level thinking in historical linguistics, nor does it attempt to.

To say that the ideas presented are controversial, or not widely accepted, is not of course to admit that they are not valid or useful. They represent my best thinking on the matters over a period of many years or I wouldn't have presented them here. My hope, of course, is that every one will be convinced by the arguments in the pages that follow, but somehow I'm thinking that that's not all that likely. Failing that, I would hope that anyone teaching or taking a historical linguistics course would find it enjoyable and stimulating to work through the arguments presented here, discovering or exploring the sources of their disagreements with me and with one another. The field of historical linguistics is vital and exciting. While this book certainly under-explores many of its interesting areas,[3] it is my hope that it shines some light on aspects of historical linguistics which, while less quaint, may reveal a deep and engaging connection between the work of the historical linguist and that of his/her theoretical colleagues.

If you've glanced at the table of contents, it will already be apparent to you that this book does not treat in detail all aspects of historical linguistics. Indeed, it is highly selective in the issues it considers. I am hoping that the logic behind the selection of the areas discussed will become clear as the reader makes his or her way through this text, but in case I'm wrong, I'll give a sketch of what's included, and why, here in the Introduction.

As hard as it may be to believe, I am not myself omnicompetent. In writing a textbook one has only a few choices about domains which seem appropriate for coverage but which also happen to fall outside one's own core competence. One can enlist the aid of a competent colleague to read and reread one's many incompetent draft treatments of the matter, slowly shaping them into something at least superficially competent; one can insist that the areas don't really matter, and skip them; or, one can acknowledge the significance of the areas in question, confess to one's own limited competence in these areas, and let the responsible instructor (or reader) seek out competent materials as a supplement. I have generally taken the third road in this situation, in some cases with what I think is a pretty good excuse for my own incompetence (e.g., in the areas of diachronic morphology and diachronic semantics).[4]

The focus of this book arises in part from my observation, made over many years of teaching and studying the field of historical linguistics, that in many departments, particularly those (the vast majority, I might add) which focus primarily

[3] For example, people are usually fascinated by etymologies, especially unusual or cute ones, and surprising genetic affiliations, and the like. I myself love a good etymology, or a surprising genetic wrinkle. It is not these pleasures, however, that this book treats. Ample cases of such phenomena come readily to hand to most historical linguists and, in any event, tend to heavily populate most introductory texts on the subject.

[4] These excuses require some relatively extensive foundation and thus will not be offered at this time.

upon training in contemporary theoretical linguistics, the usually required course on historical linguistics runs the risk of appearing seriously disconnected from the bulk of the training being provided to the students. Such courses seem to share little with theoretical phonology and syntax aside from the fact that they concern "language" in some way (and even in that domain the similarity is at times more apparent than real, as discussed in chapter 1 of this text).

In spite of this apparent disconnect between the study of core theoretical domains such as phonology and syntax in their synchronic and their diachronic aspects, I believe that the issues confronted in the two approaches to the study of linguistics are in fact tightly intertwined, though often not in the way proponents of one or the other approach seem to believe. It is in large part to the clarification of the relationship between these two interlocking enterprises that I turn in this book. I hope that the following pages will make it clear that diachronic linguistics has a central role to play in the discussion of fundamental issues in contemporary linguistics.

As one trained in the methods of traditional historical linguistics, it has been possible for me, in the course of the past two decades, to test the efficacy of those methods empirically in several very distinct and well-defined contexts, making use of everything from archaic Indo-European texts to newly published Oceanic language materials. It is unfortunately not possible to convey to those who have not themselves engaged in primary historical work in relatively uncharted waters just how remarkably productive and precise these traditional methods are. Their usefulness became particularly clear to me as new dictionaries of various Oceanic languages have appeared in print. After examining 20 or 30 carefully selected lexical entries in such dictionaries, I was able to deduce with a high degree of accuracy the phonological shape of dozens and dozens of additional lexemes. This ability is to be traced to my familiarity with the phonological shape of lexemes in the ancestor of these languages, Proto-Oceanic. The hypothetical phonological shape of these lexemes is itself a product of the application of the Comparative Method. Given a dictionary of a language of South America or a (non-Indo-European) language of Central Asia – language areas for which I possess no knowledge of the shape of lexemes in the relevant protolanguages – I would fare no better than chance at such predictions. It is difficult to escape the conclusion that the methods of the traditional historical linguist must have embedded within them some fundamentally accurate assumptions about the nature of language change and, therefore, perhaps, about the nature of language itself.

Like many historical linguists, however, I recognize that my ability to generate accurate empirical predictions regarding the morphology of a previously unstudied Oceanic language will, while still being better than my abilities in the South American or Central Asian case, lag considerably behind my abilities in the phonological domain. Moreover, even if I were lucky enough to have obtained a dictionary which was richly illustrated with example sentences (a rarity, in Oceanic languages as elsewhere), allowing me to deduce basic aspects of the syntactic structure of the language, my ability to generate predictions about the syntax of that language from a few well-selected examples would probably be no better than my abilities in the case of a language from South America or Central Asia. That is, any predictions I could generate would be based on my ability to do a syntactic analysis of the language, not on my knowledge of the basic syntactic structure of Proto-Oceanic and how that structure might have developed into that of the language under study. If we recognize that my skills in the phonological domain are based on an accuracy embedded within traditional methods, we must recognize as well that my failures in the morphological and syntactic domain may reflect some fundamental weakness of our methods in those domains.

In seeking to understand where this weakness might lie – or whether it might best be sought in something essential in the nature of the phenomena (morphological, syntactic vs. phonological) themselves – I felt that I needed to develop a conception of why the method worked so well in the phonological case (and why it did not *always* work even in this domain). This proved to be rather more difficult than I had first anticipated when I set myself the task, principally because those historical linguists who had done the clearest and most insightful work on the historical phonologies of specific languages or language families have been relatively silent about why and how they thought the method worked – the method appeared to have been shaped more by pragmatic than by conceptual considerations. Indeed, it appeared that in those cases where such historical linguists did explicitly discuss the assumptions which they took to underlie their methods, the stated assumptions were frequently either not relevant to or, in some cases, explicitly in conflict with the methods being used. Moreover, since those assumptions were developed before the advent of modern linguistic theory, they frequently involved principles or assumed models of language which seemed glaringly inconsistent with those advocated by many contemporary linguists.

Modern linguistic theory has proven, in my view, to be a highly productive and insightful approach to the synchronic study of language. I found this rather disconcerting, since I was actively engaged in historical research which used methods which were unrelated, perhaps even (as many historical linguists readily assert) directly antithetical, to those of contemporary theory. There were, it seemed to me, two possible explanations for the productivity of two opposing approaches to linguistic study: either the two approaches were not as unrelated as was frequently asserted, or fundamentally distinct approaches to the study of the same object could be equally productive. I am enough of a reductionist that I believe that the second of these possibilities is unlikely – that, minimally, the two approaches must embed similar assumptions about basic low-level constructs (and thus should at some reductive level be intertranslatable) if they are both to be useful in the long run, as both have proven to be.

This assumption, coupled with the fact that the available literature by historical linguists about their methods and the assumptions which lie behind them were so unsatisfying to me, led to the program of research which is presented here. Since the theoretical literature tended to be much more explicit about the assumptions upon which their models were constructed, the exercise I set for myself was to see to what extent I could ground my practices as a historical linguist in theoretical assumptions of a relatively standard type.[5] The benefits of such a grounding seem clear to me. First, the reasons for productivity in the diachronic study of phonology and the relative lack of productivity in the diachronic study of morphology and syntax might be more clearly diagnosed if we understood in as explicit terms as possible why the method works for phonology. Second, as is well-known, the method is sometimes less productive than we would hope even in the area of phonology ("sporadic" sound change, e.g., is a broadly accepted loophole for cases in which the method appears to break down). It would be of some value to know whether our failures in phonology were ultimately related to our failures in the diachronic study of morphology and syntax.[6] Finally, inherent weaknesses of the method may become manifest when we attempt to explicitly ground it.

My general sense is that working historical linguists shy away from discussions such as these, or consider them of secondary importance *vis-à-vis* data-oriented problem solving, which is clearly our forte. As will be clear from what follows, I believe we have suffered from our lack of willingness to confront these issues head on – there is much for linguistics as an enterprise to learn from historical linguists, but current practices in this field, as I argue in detail below, hinder the successful incorporation of its insights into general linguistic methodology.

Acknowledgments

Many, many people have read and commented upon earlier drafts of this book. It has been so long since the earliest drafts were circulated that it is probably the case that I have forgotten some of them. For this, I apologize. The book has been inflicted, as a textbook, on students at Harvard, Michigan, McGill, Concordia, and LSA Summer Institutes at Cornell and MIT/Harvard. Much of what follows arose as a direct response to the questions and challenges raised by these students, and the book has been greatly improved through their input, for which I am very grateful.

[5] I realize that I give the impression at times in this discussion that "contemporary linguistic theory" and "traditional historical linguistics" are well-defined research paradigms built around a large number of common and agreed-upon assumptions. I also realize that this is not the case – that both disciplines show internal diversity, even about basic issues in some instances. I have made my own, no doubt highly idiosyncratic, selection of basic principles and methods from the available work in both fields – those wishing to adopt a different set of principles will have to attempt their own "linking" between the approaches.

[6] There is a corollary to this point: it may be possible to discover aspects of diachronic syntax and morphology that are more like aspects of diachronic phonology in being generally amenable to standard historical methods.

My teachers, especially Calvert Watkins and the late Joki Schindler, gave me a foundation in historical linguistics and its methods without which none of the following would exist. While neither of them would agree with everything I've said here, their example provided the foundations for what follows.

In May of 1995, Michael Flier arranged a day-long workshop at Harvard University to permit discussion of some of the issues raised by the first draft of this book. Höskuldur Thráinsson and Andrea Calabrese, together with several others mentioned below, provided valuable feed back as well as vigorous and engaged debate on many issues central to this document. Many Harvard students, but most especially Jeff Bournes, took part in an electronic discussion of the issues raised in that early draft and raised issues which greatly improved matters treated here.

A large number of colleagues have had the manuscript foisted upon them and have kindly offered comments in response. Stephanie Jamison, in particular, provided detailed and characteristically insightful comments on an early draft.

Sam Epstein had the dubious fortune of having an office too close to my own and somehow thereby managed to influence virtually every aspect of how I think about the linguistic enterprise. Those of you who have known us both over the years will, I am sure, be able to detect that influence in what follows. It is too pervasive and fundamental for there to be appropriate contexts for citation in most cases.

Andrew Garrett was originally supposed to be the co-author of this book. It would doubtless be a much better book if he were the co-author, but distances of time and space have precluded that happy event. Nevertheless, he has provided many pages of detailed and valuable comments on this project at numerous stages. While this should not be taken as implying agreement on his part with any particular aspect of what follows, it is very likely that in some cases the ideas being presented are more his than mine.

Since he was a student, and now even more intensely given his status as my colleague, Charles Reiss has continually challenged my conception of the field of historical linguistics and how it works, in an exceedingly annoying but ultimately very helpful manner. I would express effusive thanks to him here, but for the risk that such an action might encourage him to yet unimagined levels of irritating behavior.

Finally, Madelyn Kissock has read this book more often than anyone else, and lived through much of its long and painful generation in a way others have not had to deal with. For this, and many, many other things, she deserves my unending thanks.

To all these people, and many others unnamed, I owe an extreme debt of gratitude. Needless to say, they bear no responsibility for what use I have made (or not made) of their kind efforts to assist me.

Mark Hale
Montréal

Part I “Language” and “Language Change”: Preliminaries

1 What is "Language"?

> there are situations in dealing with natural data – polemical situations, in particular – in which a statement of principles saves waste and leads to the realization that certain factual problems were ill-formulated; or that some factual problems are related to other factual problems as special, or as more general, cases . . . One soon learns that certain formal principles, appealed to indirectly perhaps, but with an implication of evident validity, are less cogent than they are claimed to be. And yet the practices and concrete interpretations which they are designed to buttress do stand up as reasonable and, on occasion, as verifiable.
>
> *H. Hoenigswald (1973: ix–x)*

It is commonplace to observe that "languages are always changing." Indeed, it seems clear that any conception of the nature of language as an object of scientific study which does not account for this universal property of human languages is, regardless of the apparent richness of its empirical support, in a very real sense fundamentally flawed. On these grounds alone some historical linguists regard with deep suspicion any theoretical approach to the study of language which appears to neglect this diachronic aspect of the nature of language. For their part, theoretical linguists quite correctly observe that the fact of change is not necessarily relevant to the synchronic functioning of language. Past change events do not form part of the "competence" of adult native speakers, for example; our knowledge of such events results not from the acquisition process, but from academic study.[1] This apparent "universal property" of human languages therefore seems to differ fundamentally from other claimed universals (e.g., those of Universal Grammar) in that it is not, at least an any obvious sense, the result of contraints on human cognitive systems. To the extent I believe, with most theoretical linguists, that the object of study of linguistics is precisely those aspects of human cognitive systems which constitute our linguistic competence, it would appear that there may be no place for historical linguistics in such an enterprise.

[1] There is an explicit recognition of the lack of knowledge of language history on the part of native speakers in well-established concepts from historical linguistics such as that of "folk etymology." There appears to be no serious dissent from this view.

It is a fundamental belief of the author of this book that the apparent conflict just sketched between the views of historical linguists regarding the inevitability and universality of language change as a fundamental property of linguistic systems and the concentration of modern theoretical linguists on cognitive systems is in fact illusory. The fact of language change is not only wholly consistent with modern theoretical conceptions of the object of study of linguistics, it is, in my view, highly dependent upon them. I envision a new and critical place for the study of historical linguistics in the modern linguistic enterprise as a result of the resolution of this conflict, which I will attempt to sketch in this book.

Historical linguistics, as a discipline, has existed in a form readily recognizable to the modern practitioner for at least 150 years. The wisdom accumulated during this extended period provides the modern-day historical linguist with a tremendous body of information upon which to draw in pursuing his or her research. Perhaps the greatest benefit of this collective knowledge resides in the fact that most practicing historical linguists have a well-developed and reliable sense of what types of change are attested in human languages and what types are not. On the other hand, this sense is largely a pragmatic one. While it was built up from data gleaned by the detailed investigation of many languages (though, of course, in the end only a small subset of human languages), it was not articulated (nor even usually conceptualized) as *derivable*, in principle, from some broader theoretical construct, but rather as a developed and informed intuition arising from the scholar's individual experience and the collective experience of the field as a whole.

It is, I think, hardly surprising that historical linguists favored this "intuition informed by experience" approach to doing linguistics. Theories of the nature of language were, then as now, usually linked to whatever particular conception of human psychology was in vogue at the time, and these theories displayed wide variation during the long history of the discipline of diachronic linguistics. It was fairly clear to historical linguists, at least since the Neogrammarian era of the late nineteenth century, that linking their investigations too closely to psychological theory impeded, rather than enhanced, the investigation of issues which were uniquely their own concern (such as "sound change"). Indeed, the Neogrammarian doctrine represented the first major "theoretical" breakthrough in linguistics. It was not expicitly tied to any particular psychological model[2] – it was a theory about language as such, the insights of which would need to be incorporated into a coherent social or psychological theory (rather than being dependent upon one). It brought a conceptual independence to linguistics as a domain of inquiry which has been steadily increasing since the Neogrammarian era, giving rise directly to the *synchronic* theorizing of Neogrammarians like Saussure and, indirectly, to much of subsequent, including contemporary, linguistic theory.

For all its advantages over previous work, however, Neogrammarian doctrine represented an attempt to directly implement diachronic generalizations about language without a coherent synchronic theory of the nature of language as an object of study. As Saussure asserted early in the twentieth century, such an

[2] Though of course it presupposed one which has never been made fully explicit.

attempt is doomed from the outset: diachronic generalizations must hold over what are, in fact, a series of synchronic stages. It is not possible in any meaningful sense to know what "changed" between Stage I and Stage II of some "language" without knowing what Stage I and Stage II were, as synchronic systems. It is therefore not possible to construct a restrictive diachronic theory of possible relationships between any two arbitrary sequential stages without a coherent, equally general, synchronic theory. I will attempt to show below that historical linguists have never fully incorporated this fundamental insight of Saussure's into their methodological discussions, much to the detriment of the discipline.

Thus, although historical linguistics provided the discipline of linguistics with its first explicit "theory," this theory was highly stipulative. It did not follow in any direct way from a combination of (1) a conception of the nature of the object of study and (2) a conception of the principles governing the "change" of that object over time. The first is, of course, an explicit theory of synchronic linguistic structure (the definition of "language" as an object of study is a necessary prerequisite to such a theory). The second is the goal of the discipline that historical linguistics is striving to be in the modern era. The necessity of the development of such diachronic principles has long been noted by historical linguists, but progress, in spite of the rich compilation of empirical data which should bear directly on their construction, has been frustratingly slow, particularly in areas other than phonology.

The shortcomings in conceptualization and method which have given rise to this slow development lie in two areas: the failure to incorporate a coherent conception of the nature of language into the working methodology of diachronic linguists and the lack of clarity (in part arising from this failure) surrounding the notion of "change." In what follows I will attempt to reveal where I believe the shortcomings in each of these areas lie and suggest ways in which they might be reduced. I will begin with two fundamental questions: what is "language," as the object of study of linguistics? and what is "language change"?

1.1 Synchronic "Language" vs. Diachronic "Language"

There are, in fact, two distinct issues which arise in attempting to answer the first question in the context of historical linguistics – what is "language" as a synchronic object in the world (independent of its history), and what is "language" in its diachronic aspect (i.e., what does it mean to say that a language "changes" over time)?[3] The first I will simply call "the synchronic question," and it arises quite

[3] I will not at this juncture address arguments, such as those in McMahon (2000), that no principled distinction can be drawn between synchrony and diachrony. The reasons for this will probably become apparent in what follows, but the matter will be explicitly discussed in the concluding chapter.

$$
\begin{tikzcd}
\boxed{G_1} \arrow[r] \arrow[rr, bend right] & \boxed{G_2} \arrow[r] & \boxed{G_3}
\end{tikzcd}
$$

Figure 1.1: Synchrony and diachrony

clearly in the work of Ferdinand de Saussure, perhaps most unambiguously in his discussion of what has come to be known as the "Primacy of Synchronic Linguistics":[4]

> For example – and to begin with the most obvious fact – they [i.e., the "synchronic" and "diachronic" approaches to linguistic analysis] are not of equal importance. Here it is evident that the synchronic viewpoint predominates, for it is the true and only reality to the community of speakers. The same is true of the linguist: if he takes the diachronic perspective, he no longer observes language but rather a series of events that modify it. (Saussure 1959: 90)

We can represent Saussure's model of the relationship between synchronic linguistic states and language change as in figure 1.1. In this figure, each box represents a synchronic state of the language. Diachronic linguistics concerns itself with the differences between these successive states (e.g., between G_1 and G_2, or G_2 and G_3, or even, by extension – as discussed in some detail below –, between G_1 and G_3).

What is in the boxes in figure 1.1? That is, what is the nature of the "synchronic states" G_1, G_2, and G_3? This is the *synchronic* version of the "What is 'language'?" question. As is not uncommon in scientific disciplines which concern themselves with domains of human activity, the answer to this question has proven difficult. In particular, much of the linguistic literature shows clear mixing, in one and the same discussion, between an empirically useful conception of "language" (as the object of study of linguistics) and more everyday uses of this term. Not surprisingly, such ambiguities do little to further the cause of scientific clarity. In this book, I will attempt to keep clearly distinct the widespread, pretheoretical concept of "language," with its predominantly sociopolitical foundations, and the theoretical concept of the "grammar" as the object of study of linguistics as a discipline.[5]

[4] "Par exemple – et pour commencer par le fait le plus apparent – ils n'ont pas égale importance. Sur ce point, il est évident que l'aspect synchronique prime l'autre, puisque pour la masse parlante il est la vraie et la seule réalité. Il en est de même pour le linguiste: s'il se place dans la perspective diachronique, ce n'est plus la langue qu'il aperçoit, mais une série d'événements qui la modifient" (Saussure 1984, 128).

[5] Given the lack of success of my fellow linguists in trying to maintain terminological clarity in this domain, I can only imagine that I, too, at times will fail. I beg the reader's pardon in advance for any such lapses.

"Language," like many terms which have widespread informal, pretheoretical use, can be conceived of in a variety of ways.[6] The discipline of linguistics could, in principle, adopt any of a number of these. "Language" as a sociopolitical notion (reflected in terminology such as "the English language") represents a set of distinct ways of speaking – the speakers of this "language" typically feel bound to one another through their self-designated identity as, for example, "speakers of English." It has been clear for some time that this particular conception of "language" is difficult to systematically implement in linguistic research: there is no empirical test which allows us to determine that, e.g., the English of a Yorkshire village and of Ypsilanti, Michigan, are manifestations of the same entity ("English"), while a given dialect spoken in Denmark and a similar dialect spoken in Norway represent distinct entities ("Danish" and "Norwegian"). The standard attempts to develop tests to establish "languagehood" (such as mutual intelligibility, shared core vocabulary, etc.) have not proven fruitful.[7]

This is not to say that one couldn't establish some criterion or set of criteria for identifying a grammar as "English" – one could pick some particular piece of morphology, a syntactic structure of some type, a lexeme, or some combination of these. There is no limit to the human ability to create arbitrary categories. The question is whether there are any motivated, i.e., nonarbitrary, criteria available – criteria which solve the *scientific* problem of defining "sociopolitical" languages.[8]

Likewise, this is not to deny that linguistic self-identity on the part of speakers may affect their behavior (including their linguistic behavior, e.g., in willingness to adopt innovations), but only that at base the notion is not a "linguistic" one. After all, many factors influence the spread of linguistic innovations – socioeconomic status, dress, ethnicity, age, gender, etc. – without it being necessary to see these various aspects of speaker status and behavior as the proper object of an explicit linguistic theory. We must distinguish between the behavior of individuals and the nature of their linguistic competence. The interaction of issues such as language choice – including here selection between dialects – and general theories of human behavior provides valuable material for the study of human behavior, but it is only when we can precisely characterize the linguistic competence of an individual – in detailed, explicit, and empirical terms – that we can even begin to coherently investigate the much more complex question of the implementation of that competence to perform specific (e.g., social) tasks.

6 The discussion of Chomsky in *Knowledge of Language* (1986) is still foundational on this matter.

7 For a clear demonstration of the shortcomings of these techniques in actual practice, see Schütz (1972).

8 Note that arbitrarily selecting some property *x* as criterial for "English," some unrelated property *y* for "French," and yet a third criterion, *z*, for Marshallese, would never provide us with a scientific answer to the general question of what is "sociopolitical" language. For each new language we come across, or which comes into being afresh, a new criterial property would need to be posited.

We next to turn to the question of how the concept of "language" as an object of scientific inquiry has come to differ from its common pretheoretical predecessor.

1.2 "Language" as a Synchronic Object

What is "language" as a *synchronic* object of scientific study within the science of linguistics, then? As noted above, linguists over the centuries have answered this question in a variety of ways. For example, in the nineteenth century, when the focus of linguistics as a discipline was on a relatively small set of dead languages, it was clearly commonplace to assume that a "language" was a *corpus*, a set of linguistic forms encoded in the surviving records for a given period of a given linguistic tradition. This conception of the object of study of linguistics turns out to be seriously inadequate when we, as linguists, turn our attention to living, spoken languages. First, corpora have a number of properties which living languages lack. Through their fixed nature, for example, they have limited vocabularies, indeed, even a highly restricted set of utterances (those which happen to be attested in the extant record of the language). By contrast, living languages possess numerous processes which allow the productive creation of new words (e.g., via morphological derivation, including compounding, and inflection) and cannot in any meaningful sense be said to consist of a finite and listable set of sentences. It seems unlikely, for example, that, although you are a "speaker" of English, you have ever before been confronted by the specific sentences you have read thus far in this book. Second, living languages generally provide evidence – e.g., detailed phonetic data – which we could never extract from the historical record of "dead" languages.

It is universally acknowledged that the features found in "dead" languages which make them unlike living languages are not due to the fact that these languages were in fact different in kind from their modern sisters, but rather must be attributed to the accidental nature of their attestation (which artificially restricts our access to them). No one believes that the sentences preserved on our Hittite tablets were the only sentences Hittites could say![9] It seems clear that we should not define the object of study of linguistics on the basis of such

[9] While this is, I assume, universally recognized, some of the relatively straightforward implications of this conception of things appear to be missed in some "corpus" linguistics. For example, one often sees fairly dramatic conclusions being drawn on the basis of the statistical distribution of some particular forms (in some particular sentences) in a dead language. Nevertheless, there is no reason to believe that the Hittites said (the Hittite equivalent of) "I will destroy his land" more often than they said "Meet me here tonight," but the former sentence, and thus the morphological objects in that sentence, could easily occur in the corpus *far* more frequently than the (unattested) latter. This is the normal state of affairs when dealing with a dead language.

fundamentally irrelevant properties as accidentally preserved records. That is, we must not confuse the nature of the attestation of a language with what that language was when alive. While we will need special tools – philological tools, in particular – to extract linguistically relevant and significant data from the corpora of dead languages, the so-called Uniformitarianism Principle demands of us that we assume that the languages of antiquity and other earlier historical periods were in fact no different in their fundamental properties than the living languages around us. That is, the extant record of a dead language provides us with a glimpse at the "tip of the iceberg" of what was in fact a vital, living language, no different in its basic design properties than the living languages which surround us today. The limited set of sentences and related morphological limitations are simply accidents of attestation. This matter will be discussed again when we turn to the question of the diachronic nature of "language" below.

The question of the nature of synchronic language states as the object of scientific investigation has been addressed perhaps most prominently by Noam Chomsky in numerous works, nowhere as clearly as in his well-known *Knowledge of Language* (1986). Chomsky distinguishes between "externalized" or E-Language – language as something observable, out there in the world – and "internalized" or I-Language – language as a *knowledge state.*

> Let us refer to such technical concepts as instances of "externalized language" (E-language), in the sense that the construct is understood independently of the properties of the mind/brain . . . Let us refer to this "notion of structure" [in the mind of the speaker] as an "internalized language" (I-language). The I-language, then, is some element of the mind of the person who knows the language, acquired by the learner, and used by the speaker-hearer. (Chomsky 1986: 20–2)

As Chomsky has argued at length, the proper object of empirical linguistic investigation must be I-Language.[10] The sociopolitical (or "normative," as Chomsky calls it) concept of E-Language is, unfortunately, fairly close (to the extent it can be made sensible) to our everyday use of the term "language" in phrases such as "the English language" or "the Marshallese language."[11] In my opinion it is quite commonplace in the literature for major debates to arise as a result of linguists' deft ability to switch unconsciously in the course of their discussion between

[10] As Chomsky notes regarding E-Language: "That any coherent account can be given of 'language' in this sense is doubtful, none has been offered or even seriously attempted. Rather, all scientific approaches have simply abandoned these elements of what is called 'language' in common usage" (Chomsky 1986: 15).

[11] As Charles Reiss regularly points out to me, equating the "sociopolitical" or "normative" conception of language fully with Chomsky's concept of "E-language" represents a simplification of Chomsky's characteristically complex thinking on these matters. I do not consider the simplification to do any particular damage in the present discussion, and will therefore persist in this usage. The reader should of course refer directly to the discussion in *Knowledge of Language* for a more sophisticated version.

these two quite distinct senses of "language." In this book, I will have occasion to point out several instances of this unfortunate phenomenon.

It will therefore be useful if we can be as precise as possible about what the notion "I-Language" encompasses and precisely how this concept differs from the more common notion of "E-Language." However, it is of some value to point out that, in general, discussion of linguistic issues is phrased, even in relatively nontheoretical work, in such a manner that it implicitly recognizes the importance of I-Language for linguistics. For example, "language acquision" is typically treated as the coming into being of a particular knowledge state (I-Language) in a particular human being. The notion of E-Language – whether defined as a "set of utterances" in use in a particular "speech community" or in some related manner – plays no central role in the study of this process.[12] The transmission of grammatical competence does not consist of the transfer of sets of utterances from one generation to another, but rather in the coming into being in the mind of the acquirer of a system for generating linguistic representations (which may then be passed off either to a production system, in the case of speech, or to a processing system, in the case of parsing). It is clear that parts of this generative system (the I-Language, which we will call the "grammar") owe their existence to the input data received by the acquirer, but it is equally clear that the relevant aspects of that input data are not the utterances themselves, i.e., the specific set of sentences, but rather the set of linguistic structures which the acquirer has posited in developing an account of the primary linguistic data which formed the basis for his/her grammar construction.

Speech communities, to the extent this notion could be turned into something empirically well-defined, would seem to need to acquire their properties through the concatenation of the properties of the individual I-Languages which are present within the community. The set of "potential utterances" of such a speech community,[13] i.e., the E-Language of that community, is therefore, in any event, at best a derivative notion, deducible from a sufficiently rich understanding of the relevant set of I-Languages, which we will therefore be required to construct anyway. Advocates of E-Language approaches not only lack a clear definition of what an E-Language might be, but also fail to provide any compelling statement of what it is we gain by seemingly arbitrarily constructing these E-language objects from our posited I-languages. What is it we can learn from studying these new constructs that can't be discovered from an examination of their component I-language entities?

To answer the question of what a particular I-Language is with some greater degree of precision, it is important to acknowledge in the first instance that

[12] That is, acquisitionists do not typically collect the set of sentences in use in a particular speech community and then contrast these with the set of sentences used by acquirers. What are compared are linguistic *structures* rather than surface (output) strings.

[13] This conception of the object of study of linguistics was widely held by American structuralists. Bloomfield (1933) states that "The totality of utterances that can be made in a speech-community is the *language* of that speech-community."

the notion does not necessarily refer to the generative systems which give rise to *all* of the linguistic representations of a single individual. Multilingualism and polydialectalism are both well attested (if not nearly universal) properties of individuals. It seems clear that attempting to develop a linguistic analysis of a so-called "Spanish–English" bilingual by treating, e.g., what is usually referred to as "Spanish"-type output and what is usually referred to as "English"-type output as the product of a single linguistic system (with a single morphological, phonological, and syntax module) is not likely to be very productive. In the case of a so-called "Spanish–English" bilingual speaker it is useful to posit *two* (at least) distinct grammars in the brain of a single individual.[14] It also does not seem possible, or even desirable, to impose the sociopolitical distinction between "language" and "dialect" on our I-Language models. Multiple production systems, whether responsible for generating representations which are treated in pretheoretical terms as "different languages" or as "different dialects of the same language," are, by definition, different grammars (i.e., distinct I-Languages). Grammars must be distinct if they generate nonidentical sets of representations.[15]

Each I-Language ("grammar") is thus responsible for generating a particular set of output representations. It is of some significance, since I will return to the point in the discussion of specific change mechanisms later in this book, that "output representation" is distinct from "output." The actual acoustic[16] output of an individual is, of course, the result of "playing" the grammar's output representation through a particular system (the "performance" or "production" system) under some specific set of (cognitive and physical) conditions. Acoustic output, due to the nature of the conditions governing its form, may be highly variable. The output representation generated by the grammar, which consists, if we consider only production for a moment, of a set of features, is assumed to be constant for some particular sentence (to the extent it was generated by a single grammar, almost certainly the norm). Clearly, if a particular set of features is played through a system which is producing output at a rapid rate, for example, the acoustic output will differ considerably from the same set of features played through the same body in a context in which it is producing its output at a slower rate. The variability of output in these contexts is conditioned extralinguistically. As noted above, the contrast between those aspects of the output which are

[14] Note that this assertion is neutral on the question of the precise details regarding, e.g., the storage of "overlapping" information. It is entirely compatible with a model in which duplicated "knowledge" is stored only once, but accessed by both grammars, much as the interface modules – responsible for, e.g., prelinguistic acoustic parsing and articulation – are.

[15] The amount of "overlapping" information, possibly stored once but accessed by both generative systems, may differ considerably in the cases of bidialectal vs. bilingual speakers, of course. The evidence is not clear on this point, nor does it seem particularly important to worry about the question at this time.

[16] I will take the license of using terminology appropriate to spoken language in this discussion, though the same generalizations hold of other natural human linguistic systems, such as signed language.

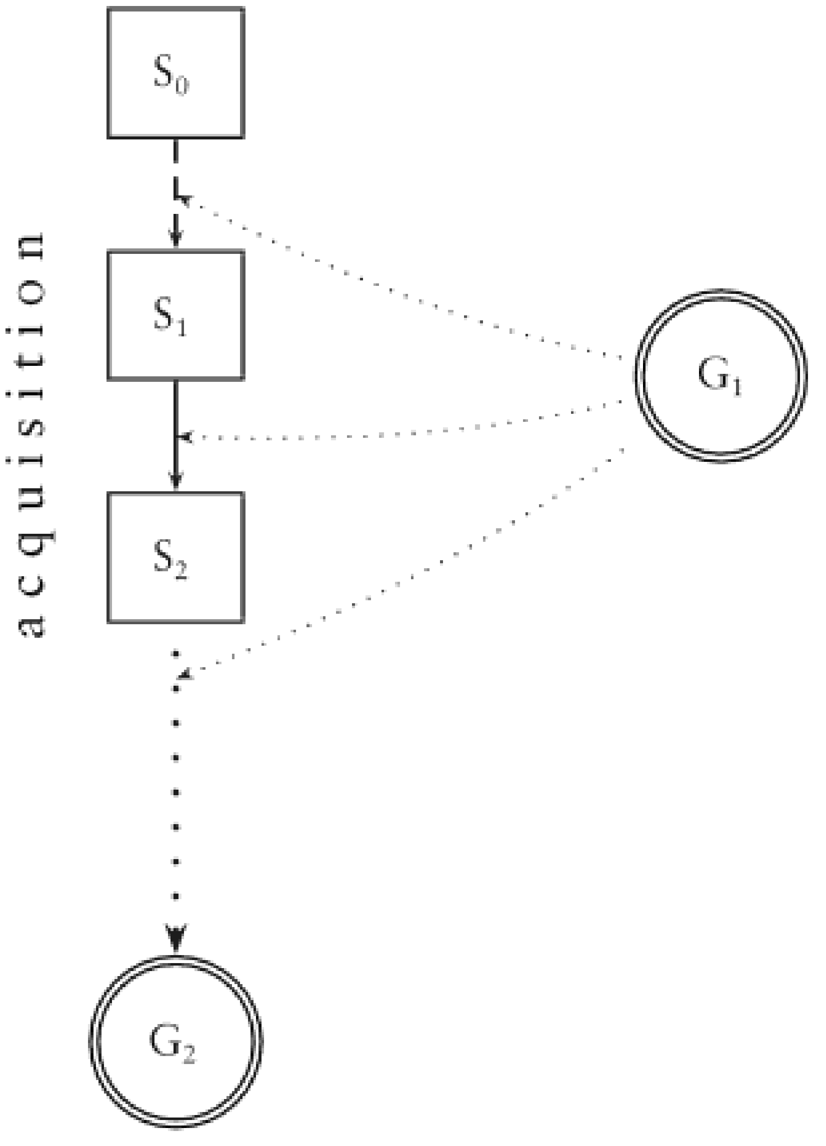


Figure 1.2: The nature of acquisition

conditioned by features of the grammar and those aspects which are conditioned by extragrammatical factors will play a key role in the discussion of certain types of change events to be considered below.

"Language," therefore, for linguistic purposes is equivalent to I-Language, i.e., the "grammar." The grammar itself is a knowledge state. This knowledge state arises in the acquirer through the interaction of the primary linguistic data (PLD) to which the acquirer is exposed during the acquisition period with the initial state of knowledge of language. This innate initial state is usually referred to as "Universal Grammar" (UG). We might represent the acquisition process as in figure 1.2. In this figure, S_0 represents the state of knowledge in an acquirer at birth, before exposure to the primary linguistic data of his/her environment. This knowledge state changes upon exposure to the PLD, such changes being the "stepwise" developing chain of knowledge states ("grammars") represented as S_1, S_2 . . . in figure 1.2.[17]

The most important feature of the acquisition process for our purposes in this book is the "terminal" state, G_2. The existence of this state in figure 1.2 reflects

[17] There are several niceties involved in this figure whose full explication must await the discussion of language acquisition and language change in the concluding chapter. One of these is the difference between the lines connecting S_0 and S_1 and those connecting other stages in the acquisition process. I'll defer discussion of these points at this time.

my belief that the acquirer, in the course of a given acquisition path, actually attains a point at which acquisition of that particular grammar can be said to be complete. Subsequent data in the PLD which appears to diverge from the current knowledge state of the acquirer is taken as evidence for *other* grammars after this point, whereas before this point, it may be taken as evidence that the grammar currently hypothesized (e.g., S_2 in figure 1.2) is in need of revision to some other knowledge state (e.g., S_3 in figure 1.2).

Note that nothing in this model precludes an acquirer from simultaneously engaging in several distinct learning paths towards distinct targets (e.g., towards a variety of "English" and a variety of "Spanish," to put the matter informally). Nor does it prevent the acquirer from beginning a new learning enterprise well after their first grammar has been fixed (as in, e.g., late childhood or even adult second-language acquisition). The key distinction between L2-acquisition and the "stages" in acquisition represented in figure 1.2 can be described as follows. State S_2 in figure 1.2 represents a revision to knowledge state S_1 based on the evidence provided to the acquirer by the PLD. Basically, it reflects the fact that the acquirer has reached the point where they *reject* state S_1 in favor of S_2 as a plausible grammar for generating the data in the PLD. Since the target of acquisition is a knowledge state which will generate such data, the rejection of S_1 in favor of S_2 is a direct result of the acquirer's informed hypothesis that S_2 offers a "better match" to the system generating the data the acquirer is receiving than does S_1. The "stages" in acquisition thus represent *replacements* of hypothesized grammars which were well grounded at an earlier point but which ultimately were deemed less consistent with the evidence.

We can contrast this process with that of the acquisition of a "second language" by an acquirer who has already attained the terminal state, G_2, for their first acquisition target. It is manifestly obvious that a 7-year-old child who is a competent speaker of some variety of "English," suddenly exposed to steady and sufficient input evidence for some variety of "Spanish" such that s/he begins to acquire this variety of "Spanish," does not engage in a *replacement* of their current knowledge state (of some variety of "English") by this new, "Spanish-like," knowledge state. That is, whereas in the intermediate stages of a given acquisition path additions to knowledge represent replacements of now outdated and contraindicated hypotheses, in the case of L2-acquisition no already acquired, steady-state L1 systems in the acquirer get "replaced" by new, L2-consistent knowledge states. Instead, a new learning path, with a new acquisition target, is begun, the initial L1 knowledge remaining intact. Put simply, the recognition of the existence of, e.g., a variety of "Spanish" in one's linguistic environment does not trigger a complete rejection of an acquired grammar of a variety of "English," in spite of the fact that the existence of this variety of "Spanish" in one's environment is inconsistent with the knowledge state a monolingual speaker of a variety of "English" is in.

It is clear that in this model – a model which will play a critical role in our discussion of the nature of language change – there is an important distinction between easily-rejected, tentative intermediate hypotheses (the S_1, S_2, etc. of figure 1.2) and the fixed, established knowledge state (G_2) which represents an acquired grammar. There is implicit recognition of this contrast even in pretheoretical work in linguistics. For example, historical linguists do not accept, as the basis for the study of language *change*, a comparison between my grammar and that of my 4-year-old child. Put another way, the "synchronic states" being compared in Saussure's sketch (figure 1.1 above) of the object of study of diachronic linguistics must be mature G_2 states – the differences between my grammar and that of my child do not represent language change.

Imagine a contrary hypothesis – one which held that *grammatical* (as opposed to lexical) learning continued throughout one's lifetime.[18] That is, imagine a model within which G_2 is never acquired such that throughout one's life one continued to modify one's knowledge state just as one did during the early stages of acquisition, constantly "correcting" one's knowledge on the basis of additional evidence provided by the PLD. We could depict this hypothesis as in figure 1.3.[19]

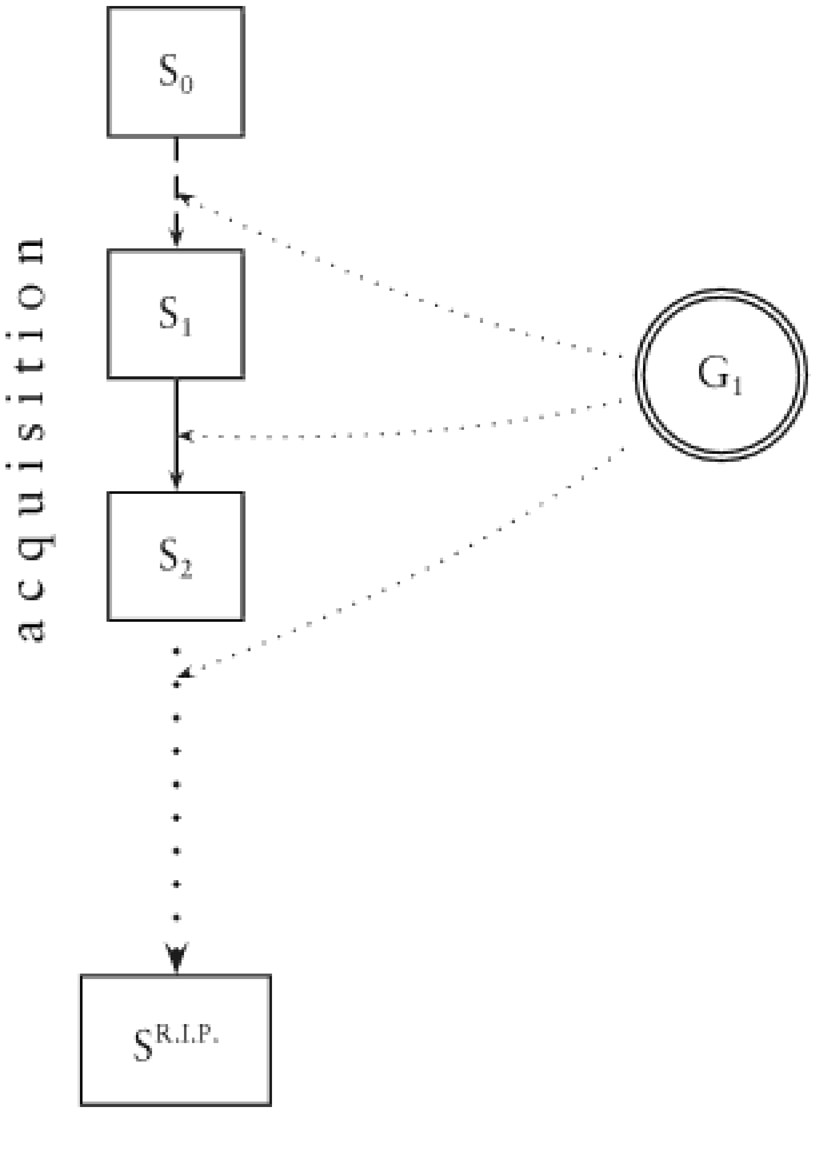


Figure 1.3: Acquisition without end

[18] There is little question that one's language behavior can change during the course of one's mature adulthood. Once one acknowledges the necessity of a contrast between "knowledge" and "behavior" – in particular, once one accepts that the set of factors which influence one's behavior, including one's "language behavior," is much broader than just the "grammar," the relevance of this fact becomes considerably more problematic.

[19] $S^{R.I.P.}$ in this figure represents the speaker's knowledge state at his/her death.

Note that under such a hypothesis there would be no justifiable reason for setting some arbitrary point, S_x, during the acquisition process, after which we could use the evidence provided by this grammar as reflecting a well-defined synchronic state. The grammar of a 30-year-old and the grammar of a 30-month-old would both represent simply a point somewhere along this never-ending chain of development. Under such an assumption, the differences between my grammar and that of my 4-year-old child *would* represent "language change," at least with equal justification as that gained by adopting any other point, clearly an undesirable result.[20]

To summarize, then, it would appear that the most coherent and productive conception of "language" as a synchronic object of study is that of the I-language grammar. Indeed, it is not clear that there is any even vaguely coherent alternative. In my experience, traditional historical linguists find this conclusion unsettling, feeling that it may undermine many of the hard-earned scientific gains of their field. This book argues in some detail that this discomfort is unnecessary.

1.3 "Language" as a Diachronic Object

It is possible that it would turn out that the optimal scientific conception of the nature of language for synchronic linguistics could be distinct from the optimal conception of that object for diachronic purposes. This seems *a priori* unlikely,[21] but in any event an even cursory examination reveals that sociopolitical, E-Language conceptions of language fail to provide a usable foundation for diachronic work, just as they do for synchronic.

Imagine that we attempt to define language, for diachronic purposes, as something akin to, though hopefully more precise than, the pretheoretical notion of "the English language" and the like. We could conceive of language in this sense as, e.g., a set of grammars (i.e., I-languages) which share some criterial property or properties.[22] Note that it would no longer be the case that such "languages"

[20] Or at least not a result which would be accepted by any historical linguist, theoretical or traditional. If you believe that this represents a desirable result, it would be incumbent upon you to give some coherent explanation for how one might do historical linguistics under such an assumption and for why historical linguistics appears relatively successful using what would turn out to be, if you were right, strongly counterfactual assumptions.

[21] Note that it would be rather surprising that under such a conception "diachronic" languages, like the distinct objects known as "synchronic" languages, have a phonology, with features and segments and rules, or processes, or constraints or whatever, and a morphology, and a syntax, etc.

[22] Since this conception will be summarily rejected in what follows, I will not belabor the problems (mentioned earlier) involved with establishing in a valid, motivated, and compelling manner what criteria might be invoked in such a case.

have things like "a phonological system" in any meaningful sense (they "have" only their members, i.e., individual I-languages). If one wanted to create some superordinate entity which did "have" things like a phonological system, we would instead need to build this object by extracting information from the individual I-languages and compiling it in some manner into a "language." One can imagine ways to do this, but none seem likely to be of much use in diachronic linguistics. For example, one could adopt the position[23] that the phonological inventory of the superordinate "language" was the union of the phonological inventories of the relevant I-languages. The inventories of "languages" being used for diachronic purposes would then suddenly be massively unlike those found in any natural human language, containing far more segments and making *phonological* distinctions nowhere attested. Alternatively, one could take the position that the phonological inventory of the superordinate "language" was the intersection of the phonological inventories of the relevant I-languages. Unfortunately, "languages" in this sense would have dramatically smaller inventories (in some cases possibly null) than any attested human language.

It is of course not possible for me to discuss the likely uselessness of every possible operation one could perform on a set of I-languages to try to generate some conception of "language" for diachronic purposes which differs from that of I-language itself. However, it seems clear that any such notion would be at best a derivative one, since it builds on the concept of I-language, already available for scientific exploitation. It is hard to see what "language change" would be under any "superordinate" set definition of "language" in any event: Does the "language" contain tokens of I-languages or types?[24] Does the "language" change if my linguistic descendants develop front round vowels even if some speakers in Northern England already have them? What if the distribution of the front rounded vowels differs in the two grammars? Does this affect the phonological system of the superordinate "language"? Fortunately, no such conception has ever been seriously *employeds* in diachronic linguistics, in spite of the fact that many historical linguists have asserted that some such "superordinate system" conception of "language" is what they are adopting. And this grants me an opportunity to emphasize a point to which we will return time and time again in this book. The rhetoric of scholarly disagreement and dissent – both of which are healthy and necessary elements of the scientific enterprise – is shaped largely by the sociocultural landscape of the field, including the complexities of interpersonal (and interinstitutional, and international) relationships, and a myriad of other out and out mysteries of the ways of the world. It is very often the case that the

[23] It is worth pointing out that since these matters are purely definitional, one is free to adopt whatever definition one likes. However, purely stipulative and unmotivated definitions, designed merely to avoid the technical problems arising from an inaccurately structured initial definition, are generally not very compelling.

[24] Note that if the answer is "tokens," the causes of death in human speakers become mechanisms of language change, a seemingly undesirable consequence.

rhetoric employed by historical linguists – both more "traditional" in orientation and more "theoretical" in orientation – in discussing the foundational assumptions behind their work bears little or no relationship to the actual assumptions which must be posited if we are to ground their *methods* logically. If we look beyond the rhetoric we will often find that scholars rabidly opposed to one another's research, nevertheless use methods which make identical assumptions on matters of seeming controversy.

So let us return to the I-language conception of language as a possible foundation for diachronic as well as synchronic investigation. While the synchronic version of the "What is 'language'?" question concerns what is in the boxes in the Saussurean figure 1.1, the diachronic question asks what licenses the connection between the boxes by the arrowed lines. That is, under what conditions would we be justified in treating a given G_1 as an appropriate object to be linked to G_2 by an "arrow of diachrony"? Put another way, the question about the contents of the box G_1 is about synchronic identity (under what conditions can we treat output as evidence for "the same" grammar), and the question about the arrow between G_1 and G_2 is about diachronic identity (under what conditions can we treat the relationship between G_1 and G_2 as one of "descent")? We can work towards the development of a useful answer to this question by a consideration of the nature of historical evidence and the scientific investigation of historical records, i.e., by introducing the field of philology.

1.4 Discussion Questions and Issues

A. Consider the following alternative definition of "language": "Language" is a set[25] of distinct grammars (i.e., grammars with different – although perhaps only "minimally" so – properties), such as, e.g., those which we might, sociopolitically speaking, consider to be present in the minds of "English" speakers, or "French" speakers, or "Marshallese" speakers, or whatever. What would "language change" be, under such an assumption, and what practical or methodological issues about the object of linguistic study might arise in pursuing research under such an assumption?

[25] I'll phrase these definitions in terms of set theory like this, because I think it's a useful way to think about the possibilities I have in mind. It is worth noting that a set doesn't have the properties of its elements – e.g., the set of all squares does not itself have four right angles, or four sides of equal lengths, etc. So a set of grammars will not have the properties of those grammars. This should become clearer as you read the subsequent proposed alternative definitions of "language," which are designed to tease these issues apart.

B. Consider the following alternative definition of "language": "Language" is the *intersection* of all the elements of a set of distinct grammars such as, e.g., those which we might, sociopolitically speaking, consider to be present in the minds of "English" speakers, or "French" speakers, or "Marshallese" speakers, or whatever. Thus, for example, if every one of the grammars of "Marshallese" (sociopolitically defined) had a /k^w/, or postposed definite articles, or whatever, then the "Marshallese language" would have said property, but if a single "Marshallese" grammar lacked that property, the "Marshallese language" would also lack it. What would "language change" be, under such an assumption, and what practical or methodological issues about the object of linguistic study might arise in pursuing research under such an assumption?

C. Consider the following alternative definition of "language": "Language" is the *union* of all the elements of a set of distinct grammars such as, e.g., those which we might, sociopolitically speaking, consider to be present in the minds of "English" speakers, or "French" speakers, or "Marshallese" speakers, or whatever. Thus, for example, only if any one of the grammars of "Marshallese" (sociopolitically defined) had some property such as /k^w/, or postposed definite articles, or whatever, then the "Marshallese language" would also have that property. What would "language change" be, under such an assumption, and what practical or methodological issues about the object of linguistic study might arise in pursuing research under such an assumption?

D. Consider the following slightly varied version of the [B] scenario above. Instead of selecting the grammars sociopolitically, imagine that we take a set of properties P_1, P_2, P_3, . . . P_n and use these linguistic properties to define a set of grammars. Any grammar defined by possession of all of these criterial features will be considered to be part of "language X." Take the defining properties to be the (relevant) properties of the "language," and ask yourself what would "language change" be under such a scenario, and what practical and methodological issues about the object of linguistic study might arise in pursuing research under such an assumption.

E. I assert above (in the discussion of figures 1.2 and 1.3), controversially it seems, that there is a termination point to the acquisition process – i.e., that at some point, relatively early in life, one is "done" with acquisition proper. A contrasting view, briefly dismissed above, holds that one continues to change one's grammar (i.e., "learn") throughout one's life (at least potentially). Discuss the implications of removing the "termination point" in the acquisition process and using the alternative assumption of continuous, life-long grammatical learning for the development of a useful definition of "change." That is, if acquisition simply continues unabated throughout one's life, at what point does it make sense to think about a "change" having taken place, and what justifies selecting the particular point you posit?

2 Linguistic Artifacts: Philology

2.1 Objects vs. "Texts"

Historical linguists in several traditions work with data preserved in ancient texts of various types (manuscripts, epigraphic records, etc.). Leaving to one side issues arising from historical work on languages which lack such records at any reasonable time depth, is the answer to our current question (what is the object of study of the historical linguist?) not obvious? The object of study of these historical linguists is the historical record of the languages in question, isn't it? In fact, I will argue that in practice it is not (and it should not be).

The historical artifacts which provide us with evidence for Latin, or any other attested earlier stage of some linguistic system, are usually found in disparate places, having come into being in a variety of situations (often at some remove from the date of the original composition of the "text" itself). Frequently, we are faced with a situation in which such artifacts span a considerable block of time, often with gaps or interruptions of the tradition. One, ultimately inadequate, way to conceive of the object of study of historical linguistics is as in figure 2.1.

Under this conception, which is not one which anyone has ever maintained (to my knowledge), historical linguistics is about the historical relationship between these artifacts. Artifact I came into being before Artifact II and Artifact II before Artifact III (as indicated by the arrow of time, *t*, in figure 2.1). To make the example concrete, and reveal why this particular approach has never been adopted, consider the following "Latin" artifacts:

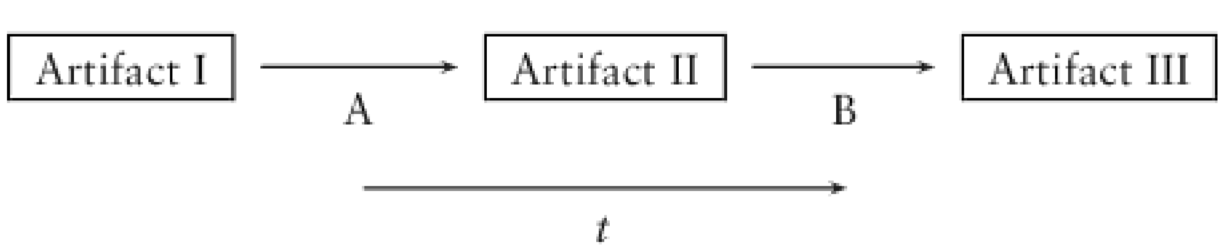


Figure 2.1: The relationship between historical artifacts

Figure 2.2: CIL 9.39416, dedication of a shrine to *Fons*

- **Artifact I:** CIL 6.39416, dedication of a shrine to *Fons*, pictured in figure 2.2 (May 24, 70 AD)[1]
- **Artifact II:** The Strassbourg Oaths (earliest "Old French," ninth century AD)
- **Artifact III:** Cicero's letter to Brutus, *Ad Brut. II 1* (first attested in a printed edition of Cicero's letters (by Cratander), Basel, 1528).

The dating of these artifacts is quite well-established. Eight centuries separate the *Fons* inscription (Artifact I) from the Strassbourg Oaths (Artifact II); seven centuries separate the Strassbourg Oaths from the only record we have of this particular letter from Cicero to Brutus. It would simply not be sensible to write the history of Latin as if it once had the properties of the *Fons* dedication, developed the features found in the "Old French" Strassbourg Oaths, and subsequently "reverted" some centuries later to a form much more archaic than that found in the *Fons* inscription. Since Cicero was assassinated in December 43 (BC), and since this particular letter to Brutus appears to have been written

[1] The text of the inscription is as follows: *Imp(eratore) Vespasiano Caesare Aug(usto) II, / Caesare Aug(usti) f(ilio) Vespasiano, co(n)s(ulibus), / dedicatum VIII K(alendas) Iunias: / P(ublius) Pontius Eros, G(aius) Veratius Fortunatus, / mag(istri) $\overline{II}$ quinquennales lustri primi, / cum Tutilia Helice et Popillia Pnoe, coniugib(us) suis, / aedem a fundamentis, sua pecun(ia), Fonti d(ono) d(ederunt)*. It can be translated as "In the consulship of the emperor Vespasian, for the second time, (and) his son Titus, dedicated May 24: Publius Pontius Eros (and) Gaius Veratius Fortunatus, fifth-year Masters, for the second time, with Tutilia Helice and Popillia Pnoë, their wives, have given a shrine, from its foundations, at their own expense, to Fons."

around April 1st of that year, it is absurd to take it as evidence for the properties of "Latin" in the sixteenth century (AD).

The actual artifacts differ in numerous ways such that the differences themselves would confound any effort to do historical linguistics at all of we accepted that the field was in fact about the A and B arrows of figure 2.1 – i.e., if historical linguistics was about the artifacts themselves. They differ in material, in location, in script, etc. None of these differences represents the *subject matter* of historical linguistics, however significant they might be for evaluating the relevance of some particular artifact for use by historical linguists.

Put simply, artifacts are not *language*. They may reflect in some imperfect way the workings of a linguistic system, but in order to extract linguistically relevant information from them, they must be subjected to some analysis. The scientific enterprise which is responsible for this analysis is not linguistics in the narrow sense, but philology. It is impossible for a linguist to do anything with a historical artifact before it has been subjected to detailed and careful philological analysis (this is why the best historical linguists in fields in which historical artifacts play a major role, as in the Indo-European language family, are also philologists). Philology is responsible for establishing the attributes of a text, many of which may be relevant for subsequent linguistic analysis. For example, in the case of our "Latin" text above, philologists will evaluate the sixteenth-century printed text of Cicero's letter to Brutus, establish the date of its original composition, and formulate a hypothesis about the form the text had at the time of composition (via text criticism).[2] The product of philological analysis is a dated, localized text.[3] After philological analysis we thus could convert figure 2.1 to figure 2.3.

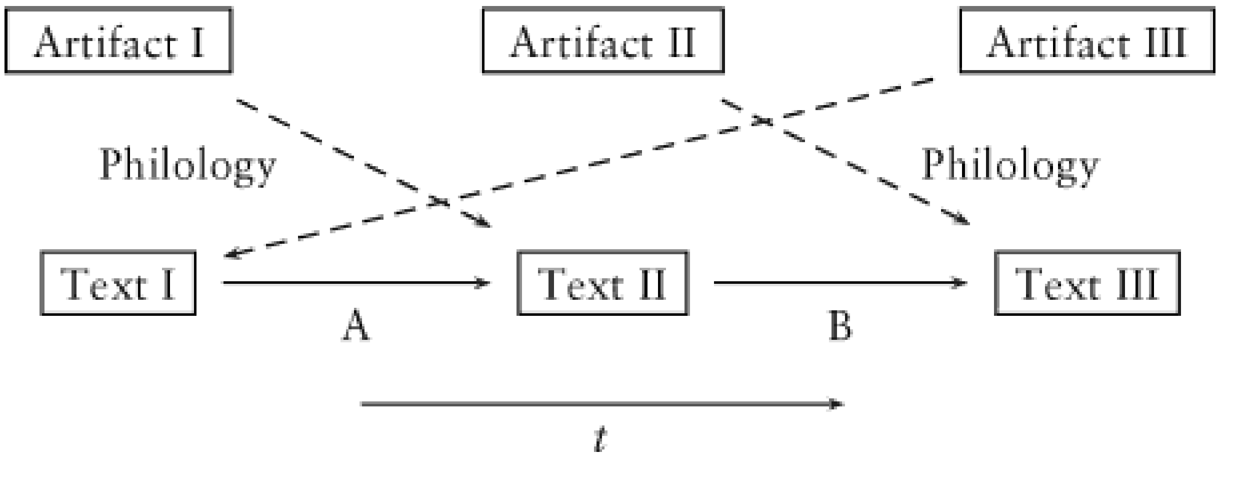


Figure 2.3: The relationship between historical texts

[2] The philologist, for his/her part, is also frequently dependent upon the work of historical linguists. They regularly use linguistic tools to establish *nonlinguistic* features of the text under study (e.g., what the best reading for a given orthographic character might be, etc.).
[3] Obviously, I intend here a *hypothetical* dating, localization, and constitution of a given text. Each of these attributes may have varying degrees of confidence associated with them – so, e.g., we may be confident about dating, give a range for localization ("somewhere in Southern Germany"), and have several outstanding questions about specific readings within the text.

In figure 2.3, philology has established the text of the three artifacts. We now have a new entity – the hypothesized text which lies behind the attested artifacts, which are probably imperfect realizations of this original text. Philological analysis has also revealed, in our Latin case, that the chronology of the artifacts is not the same as the chronology of the texts, correctly noting that Cicero's letter to Brutus (Artifact III) is in fact the chronologically earliest composition (thus Text I). Having established these hypotheses, we can now ask the following question: is historical linguistics about the relationships between Text I, Text II, and Text III (i.e., is it about the A and B arrows in figure 2.3)?

If we are to keep this discussion coherent at all, we must carefully distinguish between the features of the text (established by philological methods) and features of the language of the text (which can be established by a *linguistic* analysis of the contents of the text). This is not an easy task, since, as pointed out above, philological and linguistic analyses show a mutual interdependence and are frequently carried out by one and the same person. However, accepting that it is necessary to distinguish between the text itself and the linguistic structures hypothesized to be evidenced in that text, the linguistics/philology contrast seems to correlate with this distinction quite well. What is clear is that many aspects of the relevant historical linguistic developments crucially involve factors which are not *in* the text: e.g., syllable structure plays a key role in the development of the "Old French" of the Strassbourg Oaths from Latin, yet it is not represented in the text (or, for that matter, in any other Latin text).[4] Similarly, understanding the morphological structure of "Old French" is necessary to understand the form of language we find in the Strassbourg Oaths – but there is no morphological information encoded directly in the texts. The details of Latin syllable structure and morphology can be discovered, but only through the use of the tools of linguistic analysis.[5]

So the relationships indicated by the A and B arrows in figure 2.3 would fail to provide key evidence required to actually understand the historical linguistic developments from "Latin" to "Old French." It is also worth considering just what type of relationship one would want to claim holds of the texts themselves. The texts, as philological objects and historical artifacts, do not stand in any descent relationship with respect to one another. The Strassbourg Oaths do not owe any of their properties *as texts* to the *text* of the *Fons* dedication.

[4] Of course, there are Latin texts (e.g., some of the metrical ones) for which a reasonable hypothesis about what the syllable structure is can be *deduced*. But we are trying to distinguish carefully between the readings present in the text itself and what linguistic inferences can be drawn by the linguistic analysis of the text reflected in those readings.

[5] This is in addition to the fact that understanding the relationship between Latin and Old French requires much more than understanding the relationship between any *individual* Latin text and a given Old French one – the relationship of interest to historical linguists is the relationship between the linguistic structures, not between texts as such, as we shall see more clearly as this discussion develops.

2.2 "Texts" and "Languages"

The necessary next step is thus the linguistic analysis of the texts which philological tools have established.[6] There are two goals, related to one another, of this enterprise: to understand the linguistic structures present in the text itself (let's call this the "local" goal) and to understand the structures, entities, and processes which made up the grammar of the "composer" of the text (let's call this the "ultimate" goal). Some progress on the local goal is obviously a necessary prerequisite to attainment of the ultimate goal – one cannot simply posit a Latin grammar on the basis of *no evidence* and then use that to determine the features of a given philological text.[7] While any linguistic analysis of a Latin text will of course be guided by the established principles of linguistic analysis – such that we do not need to consider, e.g., possible syllabifications of Latin which are ruled out for human linguistic systems on general principles – it seems clear that such analysis involves attributing properties to the data, then making generalizations over those properties.

So a rigorous linguistic analysis of the text which was philologically established from a given historical artifact would lead, in principle, to a hypothesized *linguistic* structure for the relevant aspects of that text. Just as the philologist's text is a different type of entity than the artifact itself, the linguist's text is a different one from that of the philologist. The linguist's text requires the establishment of what linguistic representations the text reflects. Just as the philologist must deal with extraneous interference (copying errors, weathering, physical damage to the artifact) in the textual transmission, the linguist must deal with the fact that extraneous factors (e.g., the performance system of the text composer, the writing system, and of course the limited range of the data which happens to be attested) may distort or hide linguistic aspects of the text. We can envision the results of the linguistic analysis as providing us with a set of linguistic representations (a phonetic representation, a morphological parse, syntactic structure, etc.) for a given philological text. See figure 2.4.

[6] I will continue to talk as if these are "steps" in developing an analysis. It should be clear, however, that matters are much more complicated than that. A comprehensive philological assessment of a text requires taking into consideration the linguistic structure of the language the text is assumed to be written in. Thus one normally moves back and forth between the two domains, with additional linguistic evidence informing subsequent philological analyses, and new philological insights informing the relevant linguistic analyses. The same is true with respect to the two possible "targets" of linguistic analysis to be discussed next.

[7] I will try to remember to say "philological text" when not refering to the artifact itself, but rather to the philologist's hypothesized original form of the textual material that the artifact contains.

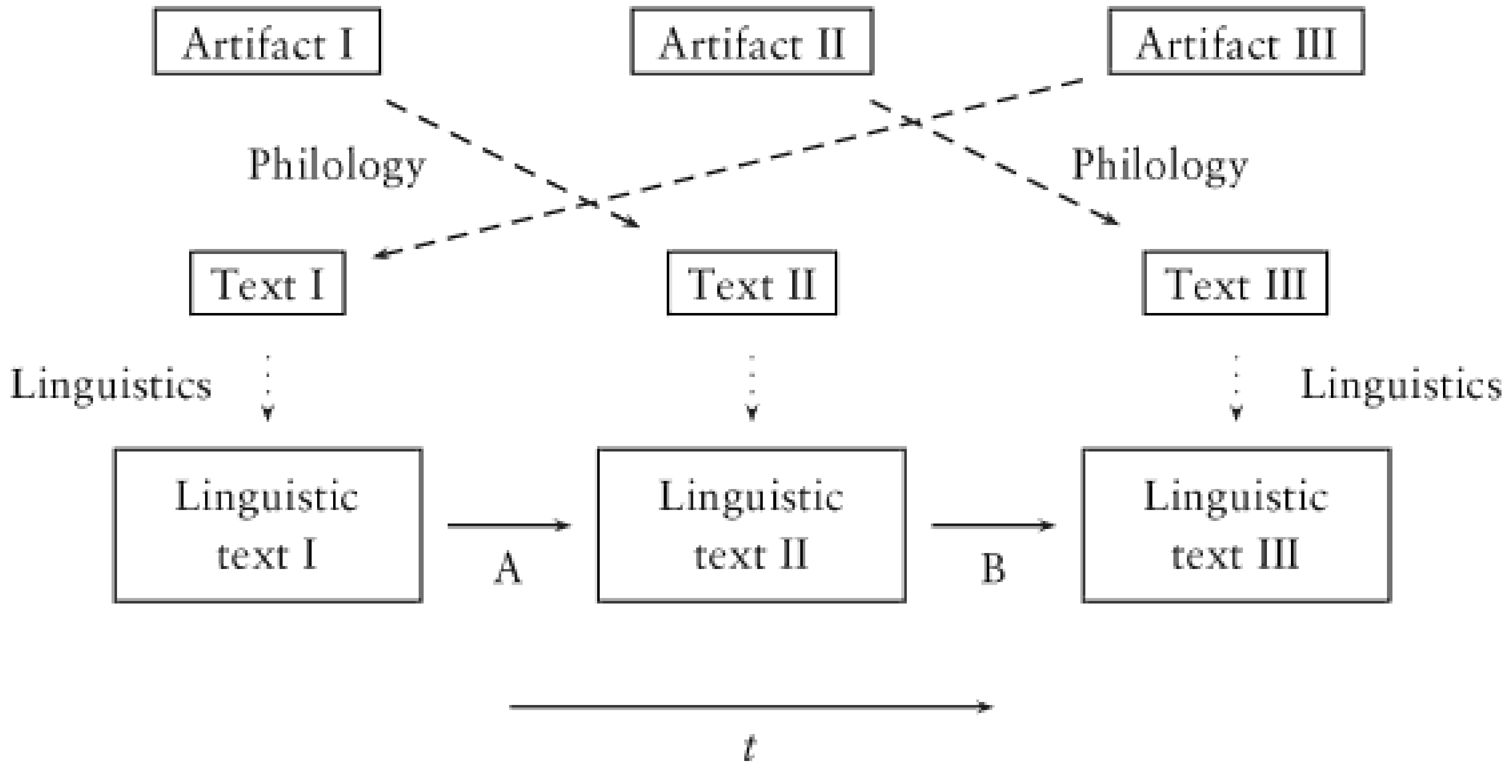


Figure 2.4: The relationship between "linguistic" texts

If one were to claim that the A and B arrows in figure 2.4 represented the type of relationships which historical linguistics should concern itself with – i.e., the relationship between attested "surface" (more on this later) linguistic structures at different historical period – one would *not* be open to all of the criticisms raised above. Matters such as syllable structure and morphological composition of the text will be contained within the object created by the linguistic analysis of the philological text. However, at this point it seems clear that the relevance of the individual text – philologically and linguistically "cleansed" of misleading data – to the enterprise is relatively marginal. One is not interested in the relationships between the linguistic texts of the *Fons* inscription, this particular letter from Cicero to Brutus, and the Strassbourg Oaths,[8] but rather in the relationships holding between the grammars that produced these documents (which are assumed to stand in some kind of descent relationship with one another).

We can see how important the grammar is, as opposed to the linguist's text, by an examination of the make-up of the linguistic text. While I referred to the linguist's text as containing "surface" linguistic structures (as opposed to deeper "underlying" linguistic structures), this is not technically correct. The surface output of linguistic systems can be thought of in at least two quite distinct ways: the actual output (filtered through the speaker's production system) or the output representations of the grammar (which will get passed on to the production system for physical realization). The relevant entities and relationships can be seen in figure 2.5.

It is not the case that either of the "outputs" represented in figure 2.5 corresponds to the kind of representations which result from linguistic analysis. The "actual" output, i.e., the output of the body (defined either acoustically or

[8] Not in strictly linguistic analysis, in any event. There may of course be *historical* connections between individual texts that are not without general interest.

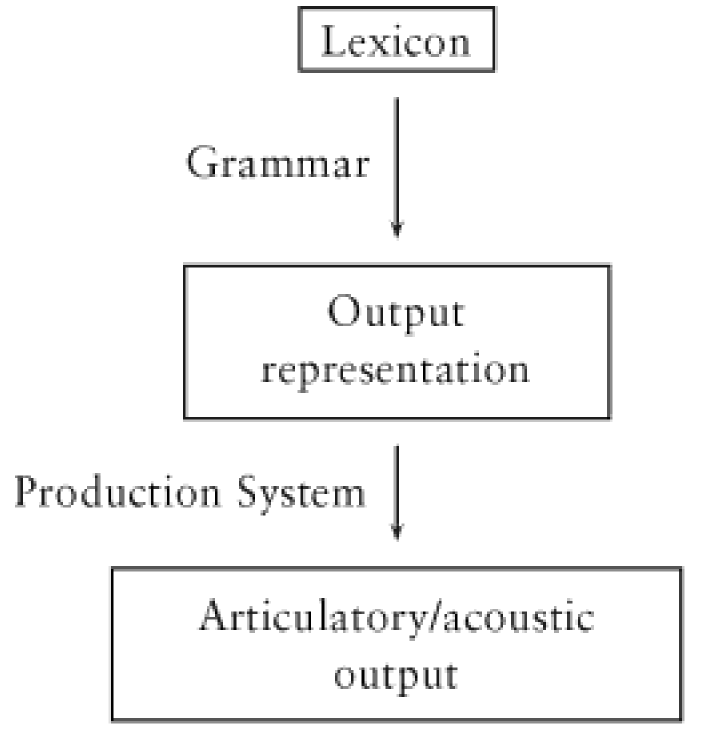


Figure 2.5: Two kinds of "output"

articulatorily) does not contain the type of information that the historical linguist needs. Syllable structure and morphological information, for example, are present neither in the acoustic stream nor in the articulatory acts which give rise to that stream. While syllable structure may be represented at the more abstract level of "output" in figure 2.5 – the output of the grammar before the performance system acts on it – there seems little reason to believe that morphological structure is present at that level, and it is generally assumed that it is not. At any rate, linguists do not as a rule believe – and there is considerable evidence from the study of the phonology–syntax interface that they are correct – that syntactic structure is present in the output representation at all. Yet such structure must form part of what linguists study, for how else could we make claims about diachronic syntax? Linguists' practice, in fact, reveals that the usual object of inquiry is not either of the output representations in figure 2.5, but rather features of the grammars involved in the generation of the texts in question. We can represent this as in figure 2.6.

The idea behind this now almost ridiculously convoluted figure is the following. We have a set of artifacts, attested in some chronological sequence. It is assumed that the source of the linguistic forms on these artifacts is a set of "texts": philological constructs which abstract away from irrelevant features such as weathering, worm-holes, and scribal errors. These "texts" may have come into being in a different chronological sequence than the artifacts themselves did. They were produced by the conversion of some linguistic output into orthographic representations (broadly construed) of some interpretable type. Interpreting these orthographic output sequences, the linguist formulates a hypothesis about the linguistic structure attested in the philological text, thereby creating a "linguistic text." From this text (or, more usually, from a set of such texts) the linguist develops a conception of the grammar which could have given rise to such an output. The bidirectional arrows between the "linguistic text" and the "grammar"

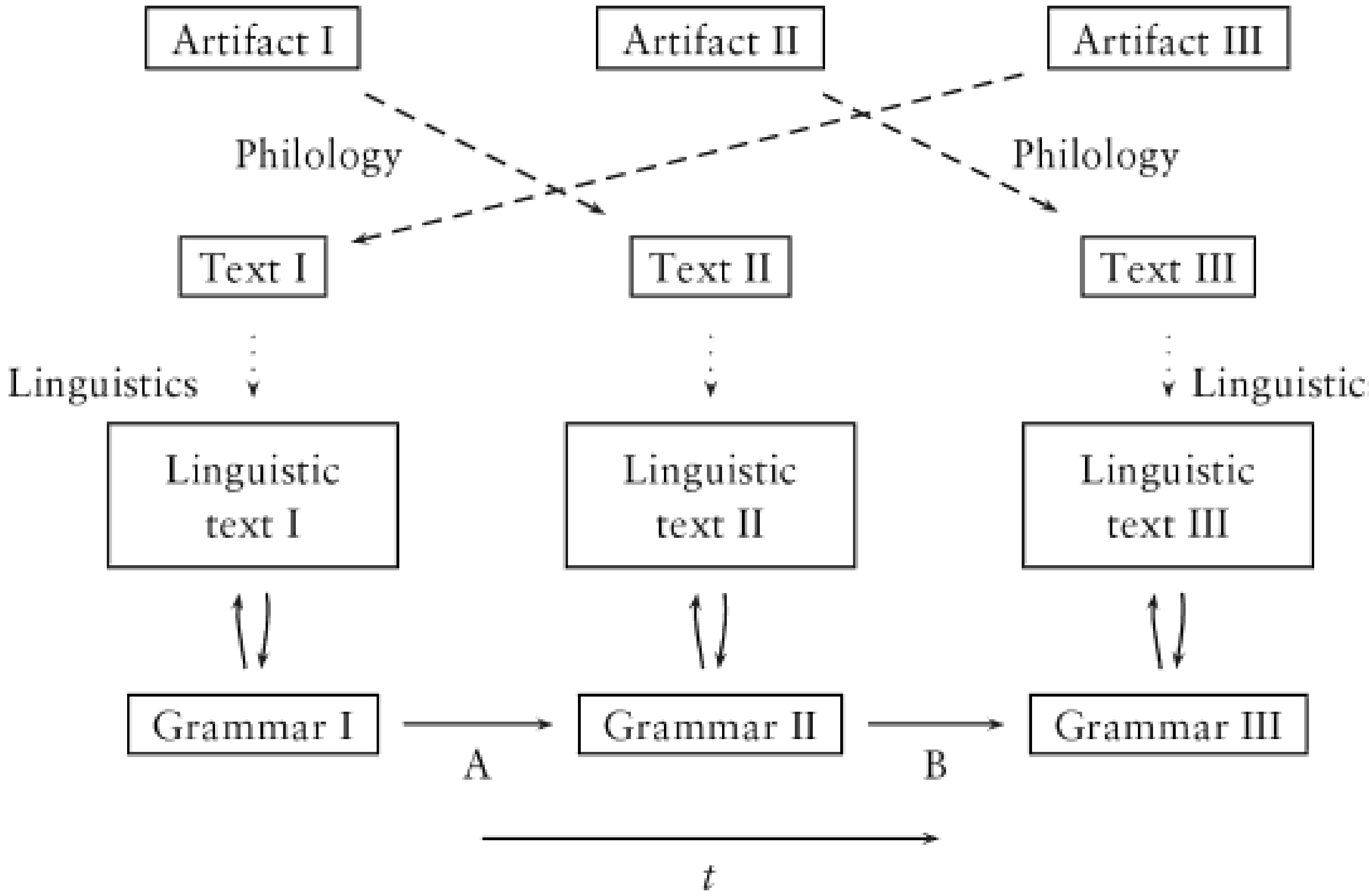


Figure 2.6: The relationship between grammars

are intended to indicate that the relevant features of the grammars are posited on the basis of the "linguistic texts," and of course those same grammars play a key role in establishing the contents of the "linguistic texts" themselves. This compels the individual constructing the "linguistic text" to use not only philological methods (to get from the artifact to the "philological text") but also linguistic ones (to get from the "philological text" to the "linguistic" one). Principles of grammar constrain what a possible "linguistic text" is, since they constrain the set of the possible grammars (which in turn are assumed to give rise to "linguistic texts"). The ultimate focus of diachronic linguistic research concerns the relationships between the *grammars* in figure 2.6.

It is important to understand that this is not *my* claim about what diachronic linguistics should be about – it is an observation about what such work has always, to my knowledge, been about *in practice*. The similarity of the (inexplicit) grounding assumptions of diachronic work are strikingly similar to those of contemporary synchronic linguistic theory.

2.3 Discussion Questions and Issues

A. Discuss, in your own words, what the meaning and importance of *all* of the elements of the rather intimidating figure 2.6 are.

B. Try to summarize, again for yourself and in your own words, the relationship between philology as a field, and historical linguistics as a field.

3 What is a "Descent" Relationship?

3.1 The Nature of Linguistic "Descent"

It was asserted in the first chapter above on general scientific grounds that the object of diachronic linguistic investigation should be "the grammar." In chapter 2 we have seen that by a careful, but relatively straightforward, chain of reasoning the same principle can be deduced if we start with the raw data (the "artifacts") themselves: i.e., that historical linguistics is about relationships between grammars.

A major complication with such research was alluded to in passing above: if we are to use evidence from two temporally separated historical text-artifacts to infer aspects of linguistic history, the grammars which gave rise to the linguistic representations (which in turn gave rise to the orthographic representations – perhaps very indirectly – found on the artifacts in question) must be assumed to stand in a "descent" relationship. In principle it will rarely if ever be possible to establish that this is the case. Does this mean that the enterprise of historical linguistics is doomed from the outset?

To answer this question, we must first consider just what it means for two grammars to stand in a "descent" relationship with respect to one another. Building on our earlier "acquisition" figure (1.2), we might sketch a roughly parallel "change" figure (3.1).

It would seem that the "descent" relationship is fairly obviously expressed in figure 3.1 – G_2 is a descendant of G_1 because G_1 provided the "primary linguistic data" (PLD) which formed the basis for the acquirer's construction of G_2. This represents a case of what we might call "immediate descent." Clearly no such relationship can hold between Cicero and the composer of the *Fons* dedication – chronology alone allows us to exclude that possibility. However, descent of a less direct sort may satisfy our technical needs.[1] If G_1 formed the primary basis

[1] It may not, of course – see below for a discussion of the problems less immediate lines of descent have created for historical linguists.

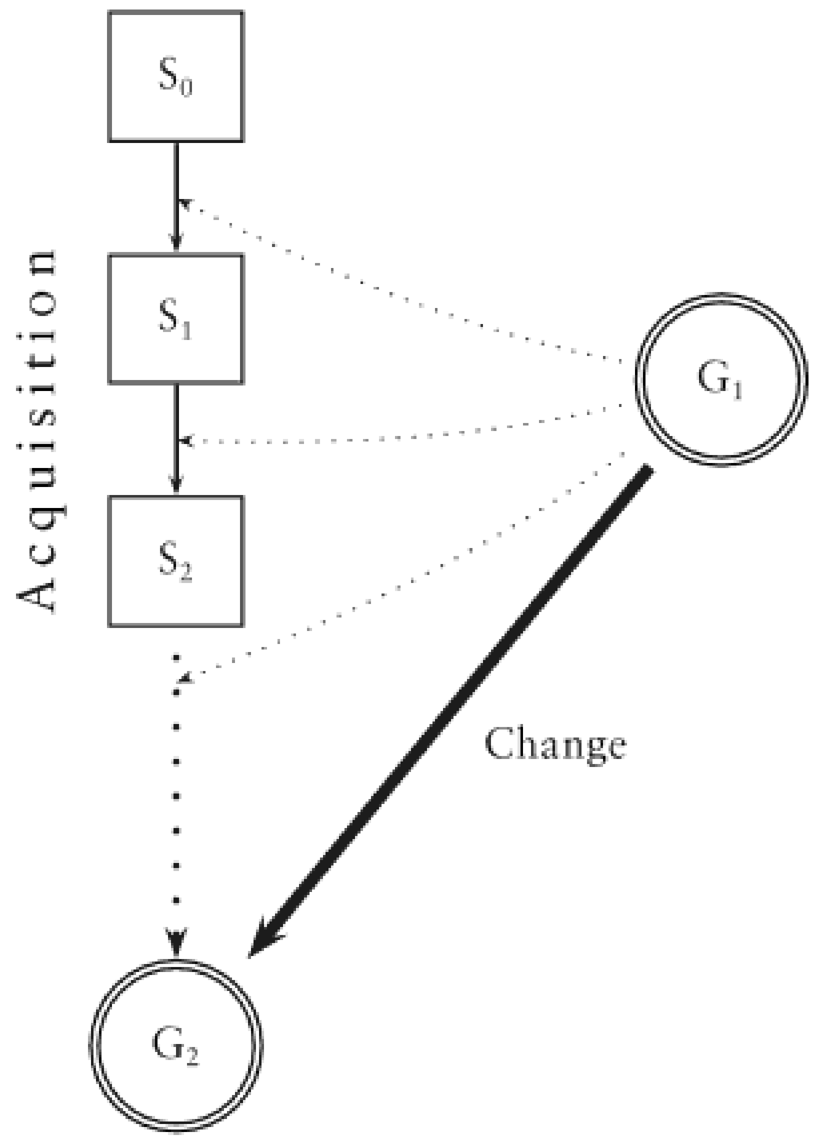


Figure 3.1: The nature of change

for the construction of G_2 and G_2 did so for G_3 and G_3 did so for G_4 and so on, to G_n, we may treat G_n as a *lineal descendant* of G_1.[2]

In figure 2.6 there were arrows going between the temporally quite divergent grammars (Grammar I, Grammar II, and Grammar III), and in figure 3.1 there is an arrow (labeled "change") linking the source grammar (G_1) and the grammar constructed by the acquirer (G_2). These arrows are frequently treated, in the historical linguistics literature, as having the same epistemological status. It is quite clear, however, that they do not. The set of possible "changes" that can occur between a source grammar (like G_1 in figure 3.1) and a grammar constructed on the basis of the PLD generated by the source grammar (like G_2 in that figure) is highly constrained by the learning algorithm and the limits that the grammar G_1 imposes on possible relevant PLD's. By constrast, Grammar I and Grammar II of figure 2.6, if we assume they stood in a lineal descent relationship, are nevertheless separated by many distinct acquisition events of the type represented in figure 3.1. The features of Grammar II in figure 2.6 are *not*, therefore, a function of the learning algorithm and the constraints which

[2] There will already be some idealization in such a picture, since "primary basis for the construction of X" is a rather poorly defined construct. We will not let this technical detail impede us at this time – as we will see, we have bigger problems to deal with. We will, however, return to this matter later.

Grammar I imposes on its PLD. While the learning algorithm is presumably a human universal, and thus invariant across the relevant time span, the PLD generated by each of the intermediate grammars which must have stood between Grammar I and Grammar II in the line of descent is going to have been different than that generated by Grammar I.

The traditional literature on diachronic linguistics often uses "change" to refer both to the single-generation events of the type schematically represented in figure 3.1 and the multigeneration events of the type represented in figure 2.6. This literature in fact uses the word "change" in three fundamentally distinct senses:

1) single-generation "change," driven by aspects of the PLD of the source grammar (and learning theory);
2) multigeneration "change," driven by the effects of iterated type (1) processes; and
3) diffusion of changes of type (1) through a "speech community."

Some of the most hotly-debated issues in traditional historical linguistics (is change "gradual" or "abrupt"?, is phonological change "regular"?, etc.) have arisen and remained obscure because, when reasoning through an argument regarding these issues, it is not unusual for the author to switch between these three distinct senses of "change" several times. Logical argumentation requires that the terms used have the same referent throughout, if the conclusions drawn are to be valid.

To give just one example, there is a wealth of literature on the question of what is a possible phonological change. One of the most oft-cited cases in debates on this question is the development of Proto-Indo-European initial **dw-* sequences into Modern Armenian *erk-* [**jerək-**].[3] Is this development a possible "sound change"?

When we ask the question without distinguishing between the three distinct uses to which many linguists put the term "change" cited above, the matter is understandably muddled and complex, giving rise to differences of opinion. However, we can ask the same question of each of the event types given above. Is the Proto-Indo-European to Armenian development of initial **dw-* a possible change in sense (1) of the term? Clearly not – the learning algorithm does not allow an acquirer to construct a grammar which generates [**jerək-**] for an input [**dw-**] in his or her target source(s). Is the development a possible "change" in sense (2) given above? Of course it is – in principle, given sufficient time (i.e., given a sufficiently lengthy chain of intermediate events), any segment or set of segments can probably become any other. In any event, we know of no restrictions on developments spanning arbitrarily long time depths other than those which arise from the fact that any stage in the development of a *human* language must be licensed by UG. Some phonological developments take place more frequently than others,

3 Cited cases include **dwoHu* > *erku* "two" and **dweHros* > *erkar* "long." See Hock (1991: 583–9) for an informal discussion.

for reasons we will explore in the next chapter, making some sequences of events much more or less likely than others, but no principled limits exist for arbitrarily long time depths in this domain.[4] Is the Armenian development a possible "change" in the sense of (3) above? That is, if someone had [**jerək-**] < **dw-* could that person's pronunciations diffuse to the rest of his or her "speech community"? Of course – diffusion appears to be highly unconstrained, linguistically.[5] That is, if you called something a [**dwi**] and I called it a [**jerəki**], it's hard to imagine that you would somehow be unable to adopt my word for that object. If you were to adopt it, the form would have successfully diffused.

Traditional historical linguistics has long recognized the need for "intermediate stages" in diachronic developments such as the Armenian one discussed above.[6] At least in the phonological domain, it is generally incumbent upon one who posited a particular development that they provide a "path" whereby the original segment (or segments) can have, through a plausible series of intermediate stages, ended up having the form that it does. As long as the initial and final stages stand in a lineal descent relationship by means of the chain of intermediate steps, this would seem to be unproblematic.

Even in the less direct sense of "descendant" intended by the concept of "lineal descent" it is, in principle, going to be a rare case that given two historical artifacts, we will be in a position to confidently assert that the grammars of the composers of the texts on these artifacts stand in such a relationship. This is well known to historical linguists, though occasionally overlooked. For example, in discussions of the syntactic changes between Old and Middle English, many authors do not appear to recognize that the provenance of surviving texts from the two periods typically used in such research is, in general, quite distinct.[7] Significant difference in provenance makes even "lineal descent" as defined above extremely unlikely.

The signifance of the issues surrounding "line of descent" should be clear: if two grammars stand in a chronological sequence such that G_1 preceeds G_2, we can be confident that the differences between G_1 and G_2 are due to *change* only if G_2 descends from G_1, rather than from some other grammar, contemporary with, but distinct from, G_1. This can be seen in figure 3.2.

[4] It makes perfect sense that the Armenian case, involving regular metathesis, two prothesis events, and some unusual consonantal changes, should be rare – only a particular ordering of a sequence made up of steps which are, on their own terms, rare could have given rise to it.

[5] It is, of course, constrained by social factors.

[6] Indeed, much of the literature on the Armenian developments, quite legitimately, concerns precisely the question of what the best path between PIE **dw-* and Armenian *erk-* might be.

[7] This is, in part, because prose texts – frequently the sole genre investigated in such research projects – are quite rare for certain "dialect areas" at various periods of the history of English.

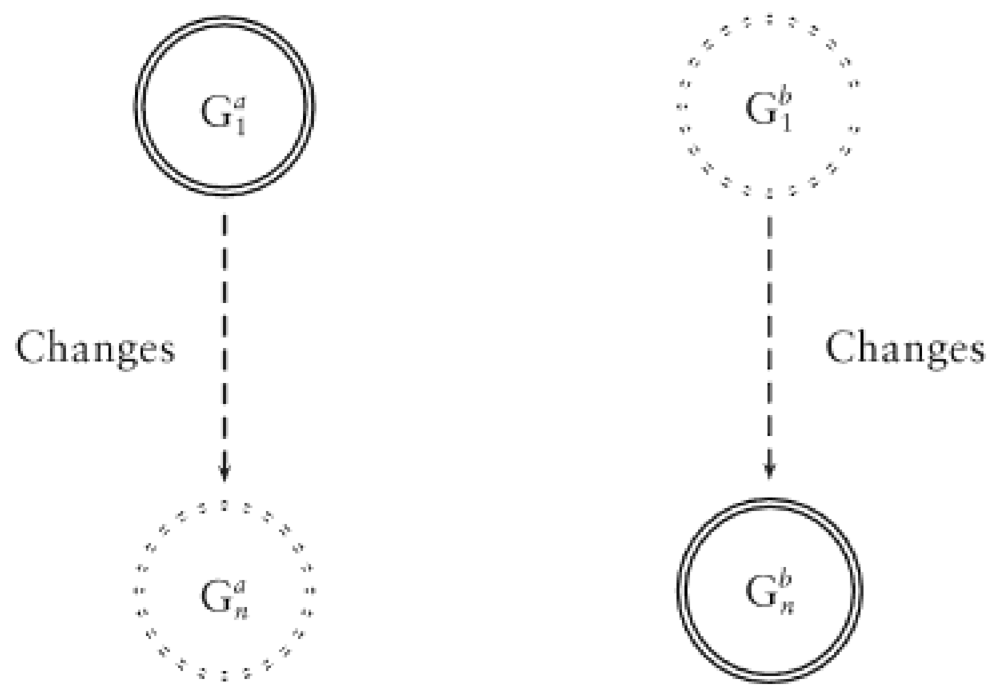


Figure 3.2: The problem of nonlineal descent

In figure 3.2 we see two lines of lineal descent (intermediate grammars have been omitted): G^a and G^b. Grammars for which we have attested historical evidence are encircled in solid double-circles. Grammars which we assume may have existed, but for which we have no historical evidence, are indicated by dotted double-circles. The two attested grammars (G_1^a and G_n^b) do not stand in a lineal descent relationship and are not linked by a series of "changes." Since some of the differences between G_1^a and G_n^b may be due to the differences between contemporary grammars G_1^a and G_1^b (which we assume do not stand in a descent relationship with one another), "explaining" features of G_n^b as a series of changes from G_1^a would be seriously mistaken.

There are two well-established, and probably ultimately related, procedures for dealing with this quite common situation (which is, in fact, the norm) in historical linguistics. The first is to show, preferably on the basis of evidence from other grammars contemporary with G_1^a and G_1^b , that the features of G_1^a which are *relevant* to the explanation of the changes to G_n^b are likely to have been shared by the unattested G_1^b . For example, it is a feature common to all modern North American English-type grammars that the definite article precedes its head noun. If, at some future date, an English-type grammar were to emerge in North America which had postposed definite articles, it would be generally safe, even if we did not know the details of the particular grammar antecedent to this new variety, to assume that its antecedent grammar also had preposed definite articles. The degree to which this procedure works is strongly dependent on the richness of attestation from the relevant area at the relevant time.

The second procedure is sometimes easier to implement, and is, I think, the one most often used, although the literature is frequently not too explicit on the matter. The first thing to note is that the attested grammars in figure 3.2 must be similar in some meaningful sense, or the existence of G_1^a would be simply irrelevant to the features of G_n^b. In fact, this "similarity" must be more than superficial – it

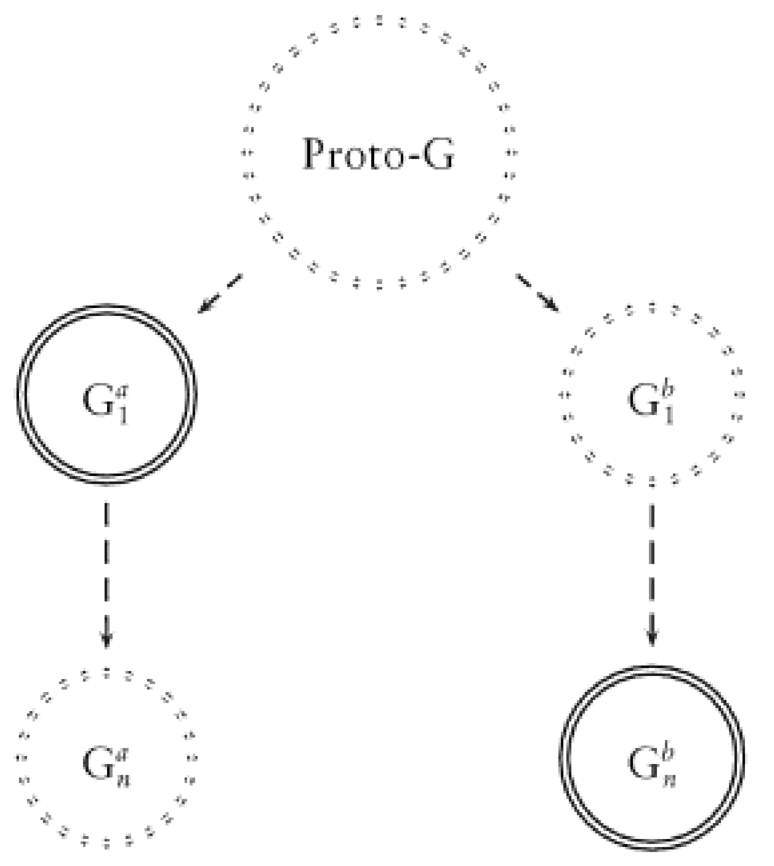


Figure 3.3: The "family" tree of the G^a and G^b descent lines

must be precisely the type of nontrivial, consistent correspondence which is required for assuming that two grammars are "related" in the technical sense.[8] It follows, for this is the meaning of "related," that the attested grammars G_1^a and G_n^b share a common ancestor, which we may call Proto-G. This is illustrated in figure 3.3.

The linguistic features of Proto-G can be determined by reconstruction. In the majority of cases of the type under discussion, much of Proto-G will be trivially reconstructable (because G_1^a and G_n^b will normally be quite closely related, their common ancester thus lying at not too great a time-depth).[9]

Let us call the attributes of grammar G_1^a which are alleged to be involved in understanding diachronically some feature of G_n^b the *relevant* features of G_1^a. If Proto-G turns out to be identical to G_1^a with respect to these relevant features, then the development from G_1^a to G_n^b involves the same development as that which we would posit if we used Proto-G, rather than G_1^a, as our starting point. Since Proto-G had these features (just as G_1^a does), they have to have changed into whatever features G_n^b has (because G_n^b is a lineal descendant of Proto-G). It is thus a harmless simplification of matters to treat the attested G_1^a as if it were the antecedent of G_n^b, rather than using the reconstructed Proto-G in this function.[10]

[8] The details of "relatedness" are discussed in some detail in chapter 10 below.

[9] Of course, there may be other data from other artifacts which bears on the features of Proto-G. But if G_1^a and G_n^b were not very closely related, the evidence of those other artifacts would be being used in this case rather than one of the grammars under discussion!

[10] Harmless, if one is correct about the properties of Proto-G and correct about what the "relevant" set of features is. This is of course not always the case.

In addition, if G_1^a represents a highly plausible intermediate stage between Proto-G and G_n^b with respect to the features under discussion, it can make for a much simpler analysis if we have only to account in detail for developments from this intermediate stage to the resultant grammar, G_n^b. Once again, provided we are correct about the "intermediate stage" status of G_1^a (which may require, in turn, that we be right about what the "relevant" set of features is), no harm is done.

It should be clear from the foregoing that in order to work with historically attested data in a manner which has any hope of being reliable we must be very confident of our ability to determine (1) "intermediate" stages along a path of changes and (2) which attributes of a given grammar are "relevant" to subsequent diachronic development. Progress in both of these domains is highly dependent upon our understanding the basic mechanism of "change."[11] Only if we can come to understand what is a possible, and what is an impossible, change can we limit the set of "intermediate" stages a given path of changes might have passed through.

3.2 Further Remarks on "Change"

It is apparent that language does not "change" in the same sense as, e.g., the physical structure of the universe. In the latter case, we are dealing with the modification, under a variety of forces, of essentially the same substance over long periods of time. There are no discontinuities (though there may be catastrophic events of various types which change gross morphological features in some particularly salient way). By contrast, in the case of language change, we must confront the fact that there is, in a very real sense, a different object (a different grammar) with each new generation.[12] The grammar of my mother did not change into my grammar: I engaged in an ultimately successful process of grammar construction,[13] using her output (in part) as the basis for the construction of my grammar. This process of grammar construction gave rise to an entity in my brain, physically distinct from the entity in her brain, which underlies my first-language linguistic competence. It is inevitable, in my view, that this process will give rise to a grammar which differs in some ways from the grammar which my mother constructed on the basis of her analysis of the input she received during her own early life. Quite aside from the "messiness" of output (which forms a potentially nondeterministic basis for my grammar construction), it

[11] In the "single generation" sense, outlined above.

[12] As will be made clear in what follows, I do not intend to imply by this statement that first-language acquisition is the only locus of "change" – in its various meanings.

[13] "Successful" in that I ended up with a grammar, not in the sense that I ended up with a grammar identical to that of my mother.

seems unlikely that she will have received the evidence for her grammar in the same order as I did, or that she would have been exposed to various features of the grammar with the same frequency that I was. All three of these factors – noise in the channel (introduced by the body of the speaker I used as the basis for the construction of the grammar, or by my own body, or by the environment), the order of data presentation, and the frequency (and thus salience) of various constructions could, or so it seems *a priori*, influence the ultimate shape of the constructed grammar.[14] Indeed, it seems likely that the inevitability of language change is to be derived from this fact: since "perfect" grammar transmission is precluded by these factors, it is inevitable that my grammar will differ from that of my input sources. Such differences are change.

One might object at this juncture that I have pushed my generally reductionist attitude to the point of absurdity. After all, let us take a simple event in the physical world (slightly idealized, but well within the confines of normal scientific idealization): a rock rolls down a hill.[15] It is clearly true that we can take any arbitrary number of "time-slice" views of this event and at each point the rock occupies some position *vis-à-vis* the hill and has such-and-such a set of (measurable) forces operating upon it. Indeed, it is possible to write a nice equation which will predict within normal scientific margins of error what kind of force it will take to get the rock to roll and what its ultimate resting spot will be, given forces of various types operating upon it. The position I have taken above regarding "language change" as an event would seem to imply that positing an event such as "the rock rolled down the hill" is really just an arbitrary cutting up of these time-slices into a "beginning" and "ending" point, and that such notions as "the rock rolled down the hill" should not be made part of any empirical investigation.

This is clearly not a desirable result. If the proposed parallelism between, for example, a sound change such as [iː]>[əj] and a rock rolling down a hill were valid, then speaking of such a phonological event as a "change" which extended over perhaps several hundred years could be every bit as useful, in constructing a model, as positing any type of "motion" in a physical theory. I believe that for much of its history this has been the conception of language change which formed the basis for the study of change processes in linguistics.

However, the proposed parallel between a change such as the diphthongization of [iː] and a rock rolling down a hill is not valid. Imagine, instead of a rock rolling down a hill, the following scenario. A large rock sits upon a hill. Every 20 years or so an individual with instructions to place an identical rock next to the rock they see on the hill comes along and does so. This individual is, however, barred from using anything other than visual inspection of the original rock to determine its features – they may not weigh it, examine its internal structure,

14 This issue will be discussed in greater technical detail in chapter 4.

15 One will notice the Zeno-like nature of the argument which follows. I am not myself attempting to invoke a Zeno paradox; my point is substantive, not philosophical.

take measurements, etc. Moreover, they have not been given any instruction as to precisely which aspects of the rock they should pay most attention to in the determination of "identity" for replacement purposes. These replacements proceed, at regular intervals, and 400 years later there is rock at the bottom of the hill which bears some resemblance to the original rock which sat at the top of the hill 400 years earlier.

Historical linguists have been looking at events which very much resemble this "replacement" scenario and have been concluding that the rock rolled down the hill. They were seeking, as is appropriate in the "rock rolls down the hill" scenario, to uncover those forces which were acting upon the rock which caused it to roll along the course it did, with the force it did, and to end up precisely where it did. But note that, in the second scenario sketched above, the rock never rolled; indeed, we are not discussing "a rock" at all, but rather a series of rocks. The critical changes in the rock were triggered not by forces acting upon the long-since replaced original rock, but rather by the mechanisms guiding the instances of replacement. Crucially, I restricted detailed access to the original features of the rock being replaced (this was to virtually force "change," just as it appears to be virtually forced in the case of language transmission). The key to understanding "change" in such a scenario is to develop a model of constraints on possible misanalyses of the structural features of the rock being replaced, bearing in mind that after each replacement, we are dealing with a (somewhat) different rock, with different possibilities for misanalysis, rather than studying the original "structural" forces operating upon the first rock. There is a good reason why historical linguists have not been able to develop physics-style equations which will predict the development of vowel systems, for example, with a high degree of accuracy: the mechanisms at work are fundamentally distinct from those in the world of physics.

3.3 Diffusion

Given figure 3.1, how are we to interpret statements, common in both traditional and theoretical works on language change, of the type "change X began in the thirteenth century but was completed only in the late sixteenth"? Clearly, under the "single-generation" conception of change being developed here, an assertion of that type is not coherent. Similarly, the widespread sociolinguistic concept of "sound change in progress" would seem to demand that change have some temporal dimension. However, the sets of differences depicted in figure 3.1 cannot be "in progress" in any meaningful sense – they come into being instantaneously, at the moment stage G_2 is reached. Before that point, they do not exist. Both the notion of chronologically extended "sound change" and that of observing "sound change in progress" crucially involve the sociopolitical conception of language (or "speech community"[16]), rather than the notion of "language" as "grammar" which we have seen above is so critical as a foundation for modern empirical investigation of language phenomena. The statement I opened this paragraph with is presumably intended to mean that the change is found in some grammars as early as the thirteenth century but is not widespread in the language (sociopolitically defined) until much later. "Sound change in progress" similarly refers to the spread of a sound change from some speakers to others, which, naturally, does take time. In both instances we are dealing not with change, as defined above, but with the *diffusion* of linguistic variants (Hale 1992).

If we return to the acquisition context we can see clearly what the differences between these two phenomena are. In the case of change, there has been imperfect transmission of some feature of the grammar. The acquirer's input sources had features X, Y, and Z and the acquirer constructed a grammar which had features X, Y, and W. The difference (W instead of Z) represents a change (Z > W). In the case of diffusion, an acquirer has *accurately* adopted a linguistic feature from some speaker. Accurate adoption is a form of successful transmission, *not* a type of change. Because we are interested in developing a theory of "change" precisely for the purpose of restricting possible diachronic paths of development, and because such restrictions appear to be most readily imposed at the "single-generation" change level, it seems useful to restrict the concept of "change" in the following manner:

- Change results when transmission is flawed with respect to some feature. When transmission is not flawed (with respect to some feature), there has been no change in the strict sense.

The notion of "flawed" transmission hides some rather complex issues. I assume that the grammar comes into being through the acquirer's processing of the input data deterministically. That is, given the same input, presented in the same order, any acquirer will converge upon the same grammar. Change can thus only be triggered by a difference between the input data received by the source speakers and that received by the acquirer from those speakers. How then do we get the data to be "different" for the acquirer than it was for his/her input sources, given that it is being generated by a grammar developed to match the source speaker's original input? Figure 3.4 may make this confusing issue somewhat clearer.

This figure is a simple iteration of figure 3.1 over a second generation. The source grammar for the first learner is labeled G_1, the grammar acquired by that learner is G_2. Having acquired his/her grammar, this second-generation speaker's grammar (G_2) became the source for a learning path which terminated when the new acquirer fixed his/her grammar as G_3. The issue which we must try to

[16] Note the absence of an empirical definition of this concept in the sociolinguistic literature in spite of its fundamental significance to much of the sociolinguistic enterprise (see, e.g., Romaine 1982).

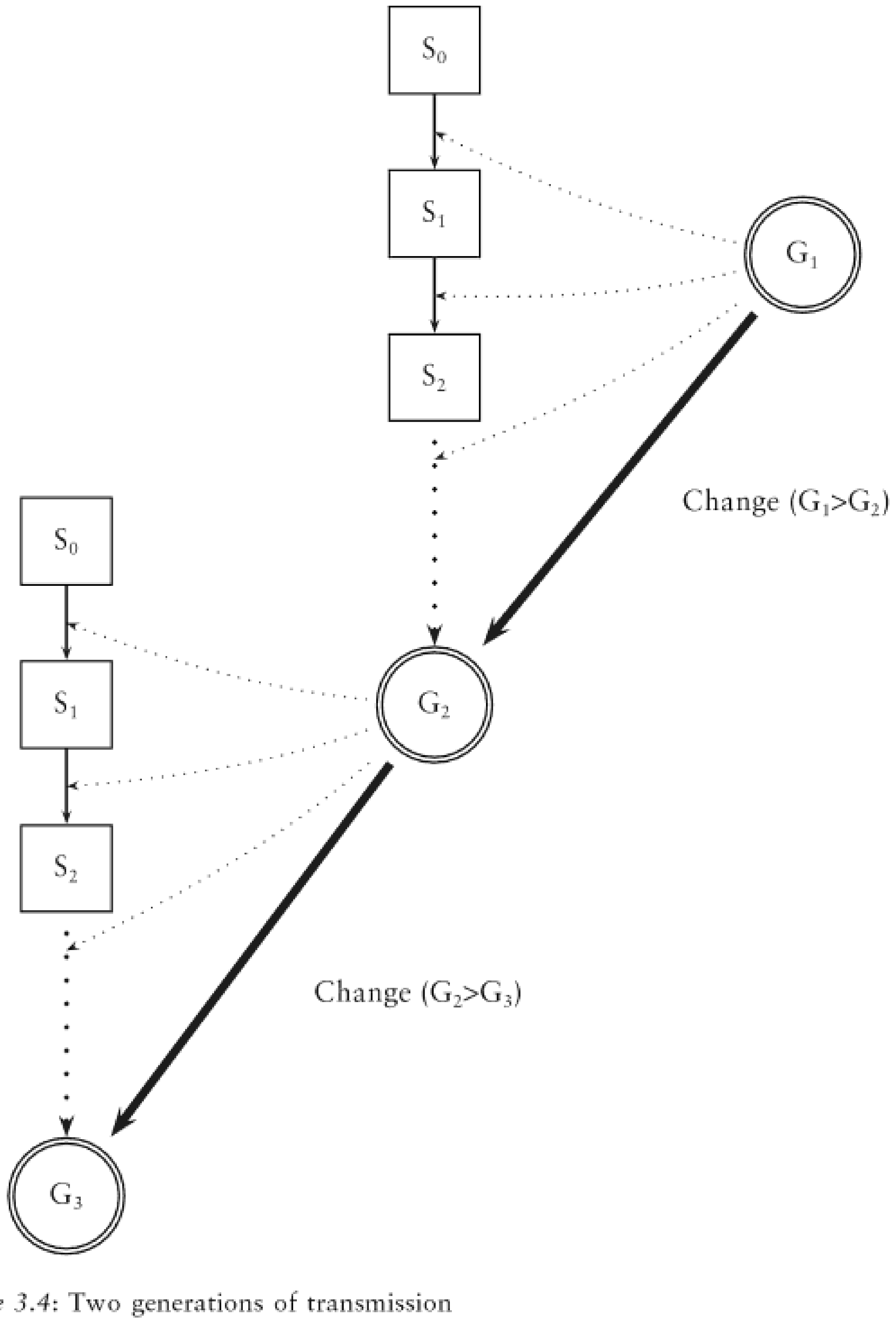


Figure 3.4: Two generations of transmission

understand concerns the dotted arrows which, in the first generation, represent the Primary Linguistic Data (PLD) which is presented to the acquirer by a speaker with G_1 in his/her head. That PLD triggers modifications to the acquirer's current hypothesis regarding his/her target grammar, thus "moving" the acquirer along the S_0, S_1, S_2 . . . S_n path to G_2. This PLD is in part[17] a function of G_1. This

[17] The reason for this qualification will become apparent as one works one's way through this book.

is why G_2 typically bears such a strong resemblance to G_1, in fact. To get G_3 to be different from G_2, we need the dotted arrows emanating from G_2, which serve as the PLD for the acquirer who will eventually converge on G_3, to be different in some relevant respect from the dotted arrows emanating from G_1, even though the evidence which allowed the construction of G_2 came precisely from those G_1 arrows.

Two factors are critical here. The first, discussed in general terms by David Lightfoot as the problem of the "triggering experience," concerns the crucial difference between the grammar and the use of the grammar. A simple example will make this clear. It is well known that, in Maori, passive clauses are far more common than they are in, e.g., Western European languages. Indeed, the default response to elicitation of a simple transitive clause from a Maori speaker is said to be a passive version of that clause, rather than an active one. It seems likely that the high statistical predominance of passive clauses was one of the factors in the reanalysis of such clauses as active transitive clauses with "ergative" case marking in a number of Polynesian languages, which may have once been like Maori with respect to the frequent use of the passive (see Chung 1978: ch. 6). It is clear that the *grammar* of Maori generates both active and passive transitive clauses. However, speakers use that grammar to produce passive transitive clauses far more frequently than active ones (thus giving acquirers rather different evidence for the status of such clauses than an acquirer of, e.g., English gets). Thus from grammars with structurally similar features (e.g., mechanisms which generate active and passive transitive clauses) different "robustness" of evidence for the two structures can be produced.

The second factor is more complicated and has not been extensively discussed in the literature, to my knowledge. It has to do with what the acquirer takes as evidence for the specific grammar s/he is attempting to construct. Since, in my view, most speakers have multiple grammars – used to generate, for example, local dialect vs. more or less "standard" speech – the total linguistic output of a single individual may not present the acquirer with the type of evidence which would lead to the construction of a single, coherent grammar. This is clear in the case of multilinguals, of course. In this case, the child must learn at some presumably relatively early point in the acquisition process that s/he is receiving evidence for multiple grammars. It must also be clear to the child that s/he is receiving evidence for multiple grammars in the case of multidialectal output, or s/he would not end up with grammars of both dialects. The precise mechanism used by the acquirer to determine which utterances are to be generated by which grammar is not known, but clearly if the acquirer mistakenly attributes utterance X to grammar A (when in fact it could only be generated by grammar B), the acquirer's version of grammar A may end up differing from that of his/her source for that grammar. As far as I can tell, the attribution of specific output strings to specific grammatical competences in the speaker is a complex and perhaps not solely linguistic task: it may involve parsing the context and possibly aspects of the communicative intent of the speaker. Thus while I would maintain that once a given form is taken as evidence for the grammar being constructed, its role in the construction is deterministic, it may be that the process as a whole is, given the intractable nature of many of the factors involved, rather nondeterministic for us as contemporary scientists. The factors which lead the acquirer to accept a given form as evidence for a particular grammar may include nonlinguistic features of the context.

In instances in which multiple source grammars are used for the construction of a single acquirer's grammar without any of the features undergoing "change" (i.e., misanalysis), there may be a new constellation of features in the grammar – a constellation previously unattested – even though this new grammar is the result of the acquirer's accurate analysis of the specific structural properties of his or her input sources. We may find "new grammars" – i.e., grammars with configurations of features we have not seen before, whose etiology is nevertheless irrelevant to the task of constraining "change."

The general contrast between change and diffusion must necessarily be maintained if we are to limit our attention to relevant phenomena. That the two types of phenomena really contrast can be seen quite clearly from the fact that changes need not diffuse: it is entirely possible – indeed, in my view, the norm – that many of the differences between a given acquirer's input source grammar(s) and the grammar he or she constructs will never spread to others (precise phonetic output representations for the vowel /æ/, for example, or the detailed semantics of an individual lexical item). They will die with this acquirer and probably remain absent from the linguistic record of his or her existence altogether. It is clear that if we were trying to develop a set of constraints on possible change, changes which do not diffuse (because, e.g., the individual in which they are manifested does not occupy the type of sociolinguistic nexus which leads others to adopt his/her linguistic features) are every bit as relevant as those that do. Of course, virtually the entire record of changes used in historical linguistics consists of examples of changes which have diffused. Since I believe that diffusion is a highly unconstrained process – i.e., that any possible "change" could just as easily diffuse under the proper sociolinguistic conditions for diffusion – this fact should not introduce distortion into the study of language change.

A simple potential sound change example illustrates the importance for our purposes of carefully distinguishing between "change" and "diffusion." In early "Middle English" the word for "pure" was *lutter*. No historical linguist would posit that its replacement by the "French" loanword *pure* represents a case of sound change (though note that the sounds associated with meaning "pure" have changed). If one were to accept it as an instance of sound change, one would have to include within one's theory of sound change the possibility of changes of the type [**l**]>[**p**], for example. No constrained theory of sound change will result from such a broad definition. Instead, as is standardly assumed (since before the Neogrammarian era), one must take the replacement of *lutter* by *pure* as an instance of "borrowing," i.e., in our terms "diffusion." Such examples make it clear that, while there may be linguistic constraints on diffusion, they will have to be very weak, licensing many diachronic events which do not represent instances of possible change in the sense outlined above.

This is not to say that the study of diffusion is not a worthy or valuable domain of linguistic inquiry. Nothing could be further from the truth – I am quite confident that there are interesting and valuable correlations, for example, between the type of language contact involved in a given diffusion instance and the various types (given by an explicit and well-defined typology of diffusion) of diffusion events (see, e.g., Thomason & Kaufman 1988 for discussion) and that working out these correlations will be of significant help in the enterprise of historical linguistics. I do not, however, think that including diffusion events in the data being accounted for in our theory of "change" will help us in our efforts to develop a constrained theory. It is critical to the development of a theory from which we can derive the constraints on change, as well as to the development of a theoretical account of diffusion, that we distinguish between these two fundamentally distinct events.[18]

Much of what I have said in the last two sections is not particularly idiosyncratic – it builds directly on ideas which are widespread in the theoretical linguistics community. That it is directly at odds with the perspective taken by some contemporary historical linguists can be seen by assertions such as the following, from Hopper and Traugott (1993: 38): "Methodologically it is certainly preferable to recognize change only when it has spread from the individual to a group . . ." The authors nowhere ground this methodological preference. In their own work, Hopper and Traugott go on to discuss reanalyses and "abductive" changes (in the sense of Andersen 1973) – clearly actions of individuals. Is an event of "grammaticalization" to be recognized (and accounted for by diachronic linguistic theory) only if the individual responsible for the reanalysis happens to be in the kind of sociolinguistic context from which spread of his/her innovation takes place? The questions of what is a possible change and under what circumstances a change diffuses are fundamentally distinct, as I have argued above. It thus works against us to allow diffiusion to act as an *a priori* filter on what phenomena we consider in developing our theory of "change."

It is not an *a priori* given that "change" rather than "diffusion" is the point in the process where constraints are best stated. Grace (1969: 110) has asserted precisely the contrary:

> it seems entirely possible that it is in the diffusion process, rather than in the innovation itself, that the constraints on sound change are enforced.

Grace summarizes at some length the model of language change proposed in Halle (1962). Halle's model of change does not depend upon acquisition and,

[18] The possibility of borrowing from closely related dialects, or from one's own linguistic ancestor, neither of which event is at all rare, can make it challenging to maintain this distinction. This, of course, does not lessen the importance of striving to do so.

given that it involves changes in adult native-speaker grammars, is almost certainly building upon diffusion events.[19]

> The hypothesis holds that (1) adults are capable of making only certain kinds of changes in their grammars, (2) that these kinds of changes define the set of possible linguistic changes (or at least a subset of a particularly significant sort – at least many of the traditional "sound laws" would typically be members of the subset), (3) that in some cases the grammatical changes made by adults produce suboptimal grammars, in which cases a restructuring is carried out by children so as to produce optimal grammars, and (4) that "linguistic change" (or some important subclass thereof) is most profitably conceptualized in terms of the form of the change in the grammars of adults (primary change), rather than in the grammars of children (which possibly involves restructuring). (Grace 1969: 105)

Contrary to the implication, though not the letter, of the first assertion in Grace's summary, I believe that adults may be highly constrained in the types of changes they can make to their grammars (certainly they can add lexical items, for example, but it is much less clear to me that they can modify the computational components of the grammar). It seems quite unlikely to me that adults can "make changes" in their grammars to give rise to anything like our "sound laws" (i.e., regular sound changes). What types of phenomena might mislead scholars into believing that such direct adult modification of grammatical knowledge was a major factor in change? I believe that there are several quite complex issues which arise in this regard. We may be in a position to productively deal with these at this time – in any event, clarifying their status now will allow us to avoid several conceptual confusions which often plague the consideration of issues to be treated in the coming chapters.

First, it is important, in considering these matters, to distinguish adult innovation from the production, for the first time, by an adult of a form generated by the grammar they constructed as an acquirer. For example, it is possible that a particular child might never have heard the plural *formulae* of *formula*, thus storing the noun with no lexical exception feature, in spite of the fact that all of the child's sources had the irregular plural for this noun. If, for the first time, as a 30-year-old adult, the person in question produces *formulas*, there may be a tendency to see the moment of utterance as the moment of change – this would then represent a change in an "adult" grammar. This would be a mistake: the change took place when the lexical item "formula" failed to be stored with

[19] This is reminiscent of Andersen's (1973) contrast between "adaptive" and "evolutionary" change. Unfortunately, much of the rest of the machinery posited in this widely-read paper is, in my view, misguided. The possibility of "adaptive" rules (marked as such, according to Andersen) which parents apparently construct and then carefully avoid using when evaluating the speech of acquirers is completely ungrounded, as is the parents' direct access to their own "underlying forms" (before the application of the adaptive rule) required to get the model to work.

the lexical exception feature responsible for generating *formulae* – the moment of utterance is irrelevant.

Secondly, in real-time production, an adult may fail to access a lexical exception feature (apparently). Thus, although they may have *formula* stored with an indication that it makes an irregular plural, *formulae*, in real-time production they may produce a form which the speaker would normally identify as a "speech error." Since normally the form produced in such a situation involves default morphology for that lexical class (thus, in our example, *formulas*), the speech-error form will be parsable and seem well-motivated. But of course it does not represent a change (when produced), any more than any other inadvertent misproduction.[20]

The third issue is a serious and complicated confound, but it is worth a rather extensive digression. It is perhaps best introduced somewhat obliquely, in the following manner. It seems easy enough to establish that I can train some random English speaker with no prior knowledge of or exposure to, e.g., Hungarian, to utter a Hungarian sentence. With sufficient training, the sentence could certainly be uttered to the satisfaction of any native speaker of Hungarian. But once the trainee has reached the point where s/he can fool a native speaker of Hungarian, is it in fact correct to say that they have "uttered a Hungarian sentence"? They may believe they are saying "I am vacuuming the floor" when in fact they are saying "I am choking the alligator." They may have no idea what the word order of the "sentence" is, or even of where the word boundaries are. If we take a sentence to be a *linguistic object* of a certain type, with certain properties (e.g., a phrase structure, a tense specification, a predication, etc.), then clearly training a human to parrot what sounds like a grammatical string of Hungarian need not involve that human saying a "sentence" at all. While it would appear, when they utter that sound sequence, as if they were saying a Hungarian sentence, it would only take relatively trivial psycholinguistic investigation to determine that, in fact, they are not.

One of the implications of this simple thought experiment is the following: it is possible for a human to appear to be using a grammar as the foundation for their utterances when, in point of fact, they are not. In the Hungarian story, determining that we are dealing with an illusion would not be difficult, but I believe that there are cases of considerably greater subtlety and complexity, involving precisely the same confusion. A fairly easy way to see what I am concerned about is the following: when one is at the very beginning of one's study of a foreign language, one may learn to utter a sequence (e.g., the "German" sentence *Ich weiss, dass ich ein Student bin.* "I know that I am a student.") under more or less appropriate circumstances, with, let's say, relatively accurate pronunciation. In one's early second-language (L2) dialogues, it seems clear that the process at work looks something like that in figure 3.5.

[20] If someone takes the form as the intended target of the speaker and adopts (i.e., acquires) it, that is of course change of the usual sort (just as when physiology triggers an ambiguity in the acoustic stream).

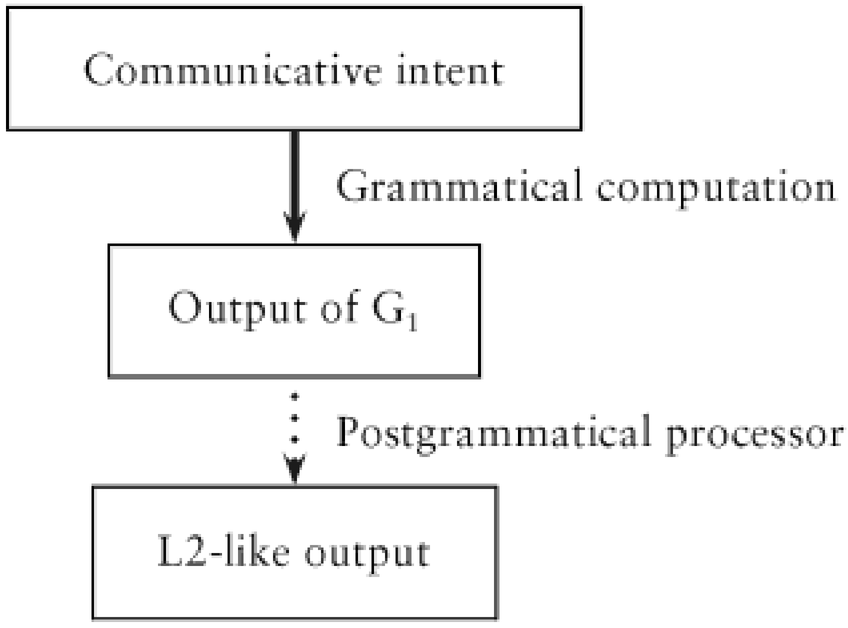


Figure 3.5: Early L2 output generation

Thus if I, as a beginning German student, want to construct a sentence which conveys the meaning that I know that I am a student, I first feed that communicative intent into my first-language grammar, which produces an output like "I know that I am a student." I then perform a series of slow, error-prone, and higher-order cognitive resource-demanding processes (such as "replace *I* by *ich*," etc.) whose output is something like *Ich weiss dass ich ein Student bin*. I then attempt to pronounce that "sentence." I have put "sentence" in scare quotes because I think there is good reason to believe that I have not, in fact, uttered a sentence at all. I certainly made my early subordinate clauses in German by "moving the verb to the end" (as I was instructed to do by my teachers) – but of course this is not a syntactic operation. Syntactic movement targets specific structural positions. In German subordinate clauses, in particular, it may well be the case that the verb has undergone no movement at all (that it moves in *main* clauses seems clear). I was merely doing roughly word-for-word replacements of my first-language sentences, and performing operations on the resultant strings which are *not permitted by Universal Grammar* (UG) (such as "move the verb to the end of the sentence"). Since the strings I ultimately proudly uttered as if they were German sentences were produced by processes not possible in a human language, I obviously was not saying German sentences, which are produced *solely* by UG-licit processes. The case is different from the parroting case only by the fact that rather than rote-learning the specific utterances I would be saying, I had internalized a set of processes to convert my L1-output into something that would seem German-like. The impression that I was in fact speaking German was correspondingly stronger, both for myself and for others, but it was, in the end, illusory. Only once one begins to generate sentences in a language using an actual *grammar*, can one be said to be uttering sentences at all.[21]

How is this tale relevant to the issue of whether adults can "change" their grammar? We have doubtless all observed cases, perhaps even involving ourselves, in

[21] We will return to the issues surrounding the positing of a postgrammatical processor in our discussion of variationist approaches to syntactic change in chapter 8.

which the output produced by an individual changes over time. I myself grew up speaking a strongly nonstandard variety of English, with multiple negation, invariant *a* as the indefinite article, and numerous other stigmatized features. As an adult, in most settings in which I find myself in my day-to-day existence, my speech appears to lack these properties. However, if I spend time with my mother and my siblings, these features reemerge almost immediately, without any conscious intervention on my part. It seems clear that at the very least when I first began to adopt "standard" dialect features, I did so using a translation + postprocessor technique not unlike my early German-learning days. That is, in general, it makes little difference whether one's target for an L2 learning task is another "dialect" or another "language": if it represents a different *grammar* (which of course it does by definition), one will begin by producing pseudogrammatical, postprocessor output long before one will actually have constructed a grammar to produce the output in question (if one ever takes the latter step). Extensive use of this postprocessor can make it very fast and very efficient, but because of the nature of its operations, it will never *become* a grammar.[22] The fact that my output, when using this postprocessor, has changed, perhaps dramatically, cannot therefore be taken as an instance of "change" in the historical linguistic sense – after all, even if I spent a great deal of time speaking Pig Latin, we would not say that my first language *changed* into Pig Latin, because Pig Latin is not a possible human language. Note that, even if Pig Latin *were* a possible human language, to claim that my first language "changed" into it would entail that I no longer could speak my first language (which would have "changed" into Pig Latin and thus be gone). As the cases of German and of my learning some standard English constructions make clear, the process involved is not one of "changing" the first language, but rather of building a postprocessor which operates (when I opt to use it) on the output of my first language.

All of the examples given in the first three "issues" above have involved *apparent* adult grammar change, but have turned out to have alternative, nongrammar-based explanations. However, I do believe that adult language learners can successfully construct grammars, i.e., I do not believe in what is sometimes called the Critical Age Hypothesis. It seems clear that if an adult constructs a new grammar which is similar in many ways to their first grammar, and they begin to use the new grammar extensively, it may *appear* that they have changed their first grammar into their second. But the question of whether such adults have "changed" their grammar, or if this is perhaps better conceived of as them having constructed a new grammar of the "prestige" dialect they have opted to imitate, is an empirical one. My suspicion is that they do not normally lose the ability, in other social situations, to produce their original forms (as in my

[22] I believe it is not difficult to experimentally assess the difference between the output of an (L2) grammar and the output of an L1 + postprocessor system. The latter suffers severe degradation of output under circumstances of fatigue, distraction, or intoxication – the former does not.

reversion to my native dialect forms when speaking to my siblings mentioned in the last paragraph): this would mean that their original grammar is intact and they have, no doubt borrowing heavily from their first grammar (taking over everything from it they think is identical to that in the prestige target – a judgment they could easily get wrong), constructed a grammar of the new dialect. Once again, to speak of "change" in this case seems misguided. We would certainly not want to say, if I learned some variety of a "German"-type grammar through this same sequence of events, that my "English" "changed into" my new "German."

To return to the Grace quote, the assertion in (2) indicates that Grace and Halle believe that adult speakers suddenly, without any contact-induced reason (else it would be diffusion, of course), start uttering, say, [**f**] for what they learned as children was [$\mathbf{p}^{h}$]. Note that if he has in mind diffusion from closely related dialects, then he is merely ducking the question of where the first grammar which had [**f**] (or some [**f**]-like segment) for earlier [$\mathbf{p}^{h}$] got that feature. There is a certain tradition in treating "sound change" as equivalent to borrowing, which builds critically on avoiding the question of where the *variants* present in a speech community might have come from in the first place. Hoenigswald (1960: 55) makes the argument as follows:

> It is hard to escape the conclusion, speculative though it is, that sound change is generally the result of internal stresses and strains within one speech community and that its mechanics is fundamentally that of borrowing with sound substitution. With the aid of such a view a great obstacle to the acceptance of sound change can be overcome: the inability to conceive of a process so contrary to ordinary speech activity as the elimination of existing contrasts. Naïvely, any speaker of a language can imagine that he will at some future time carry out, along with his fellow speakers, lexical borrowing . . . But no speaker of English can easily see himself giving up the contrast between, say, *clip* and *lip*, *click* and *lick*, *clock* and *lock*. Yet that is more or less what happened to *knight* and *night, knit* and *nit, knot* and *not* a few centuries ago. If it could be shown that that change began with a purely phonetic disparity, and with a subsequent effort (a "misunderstanding" . . .) on the part of some members of the community to reproduce what is a less prominent (non-released? voiceless-nasal?) phone in the /kn/ of the source dialect of their high-prestige neighbors and substitute for it their own /n/, an important difficulty would disappear . . .

While I would not deny that there may be many interesting "sound change"-like diffusions of this type, the question which is being avoided here is this: what is the source of the "phonetic disparity" in the realization of /kn/ if it is not "sound change"? Note, additionally, that the "great difficulty" raised by Hoenigswald does not arise if we locate the misanalysis which led to the [**knot**]:[**not**] merger in first-language acquisition: the acquirer never had the contrast and therefore had nothing to lose.

A hypothesis of "spontaneous adult modification" of the grammar such as Grace and Halle would appear to be advocating seems seriously undermotivated, as a general theory of change. Since no mechanism is suggested, it is completely unclear to me how we would preclude an adult suddenly producing [i] for [p], for example.

Point (3) above, that adults may change their grammars into "suboptimal" ones, seems to have widespread currency in contemporary theoretical approaches to historical linguistics. For example, Lightfoot (1979) includes as a key component of his model of syntactic change the notion that "less highly valued grammars" are liable to undergo reanalysis. The model implies some kind of steady decay from an ideal ("unmarked") state until a catastrophic change ("reanalysis") sets things right again. Leaving aside for the moment whether we are talking about adults changing their grammars (which I do not actually believe happens) or adults constructing new grammars (which they may come to use as their primary grammar over time, depending on what kinds of social situations they find themselves in), do we really want to license the notion of "suboptimal" grammar at all? The explicit claim is that it is possible for the human organism to construct and use for their day-to-day communicative functions (e.g., with their children, crucially in this context) a grammar which is less than optimal. If the human cognitive system is capable of constructing such an entity, and such an entity will serve all the purposes of real-time functioning required of human language, what is to prevent the child from constructing such suboptimal grammars as well?[23] Put another way, why should the optimization process – clearly a critical part of acquisition – optimize beyond necessity during acquisition but not later on? The principles of UG must constrain all grammar construction (at any time) in any human organism. If "optimization" is required, as I assume it is during first-language acquisition, it must be required by principles of UG (i.e., suboptimal grammars would have to violate some principle of UG, otherwise they could be constructed). I assume that human beings cannot use computational systems which violate the principles of UG as languages, by definition,[24] and that all human languages are "optimal," given the data upon which they are constructed.[25] The notion "suboptimal" grammar seems to me therefore to be a very costly and counter-intuitive mechanism to account for change.

[23] Licensing "suboptimal" grammar construction during first-language acquisition creates a dangerous theory, in my view, in which some languages are "better optimized" than others.

[24] That is, the principles of UG must license any possible human language.

[25] It seems to me entirely possible that one of the resources available to the child growing up in multilingual (or multidialectal) contexts which allows him/her to determine that the data s/he is receiving comes from different grammars – not necessarily from different individuals, of course – is that no single grammar can be constructed according to the principles of UG to generate, for example, simultaneously a variety of "Spanish" and one of "English" (or indeed a variety of "Yorkshire English" and a variety of "RP English"). Since there is no reason to believe, as far as I can see, that this inability is computational (all of the necessary computations to get both Spanish and English are possible), I suspect that what is at issue is the inability to "optimize" the grammar one would have to construct to a degree which would make it an acceptable instantiation of UG.

Finally, even if we accepted the argument as far as point (4), there is nothing in the discussion which would indicate that the instances of adult change are in any way more "valuable" for the development of a theory of language change. The restructuring – optimization – which children do would seem to be the more interesting place to look for constraints on change. The epistemological status of "suboptimal" grammars hardly makes one want to focus the attention of a constrained theory of change in that area. Unless Grace (and Halle, and others adopting this view) believe that grammar transmission is flawless, at least some changes are clearly to be located in the transmission process.

3.4 Conclusion

Given the definition of change advocated above, what types of change do we expect to find in the history of language? I am not in a position to present a detailed theory of constraints on possible change – we have not yet reached the point in the development of historical linguistics to know what such a theory would look like in any significant detail. Instead, in the coming chapters, I will attempt to outline further considerations which will play a fundamental role in the development of this constrained theory of change, assuming a particular model of the grammar. Obviously, under different assumptions concerning the nature of grammatical representations, a different set of concerns might emerge. However, as has hopefully become somewhat clear from what precedes, and will perhaps become even clearer in what follows, it is imperative that historical linguists make explicit what model of grammar they are operating with: the types of change they posit will follow directly from their assumptions about the grammar. An inexplicit theory of the structure of the grammar will be unclear in what constraints it imposes on possible changes, and thus what types of change it permits. As in any science, only by being maximally explicit about the content of our posited constructs can we hope to move the agenda of historical linguistics as a scientific pursuit forward.

3.5 Discussion Questions and Issues

A. Discuss the contrast between "change" and "diffusion" advocated in this chapter. Can you see practical difficulties with attempting to apply the distinction systematically to a body of data? Can you imagine the general properties of a theory which did not maintain such a distinction, and what a failure to do so might mean for a general theory of language change?

B. The "postgrammatical processor" (which will also be referred to simply as a "postprocessor") is introduced in this chapter and will come up again on occasion in what follows. Discuss what the concept is, what role it plays in the theory being developed, and, again, any practical difficulties you envision with its invocation in specific instances.
C. Discuss the difference between "direct descent" and "lineal descent" established in this chapter and consider the arguments provided for how the problem of a lack of direct lineal descent might be plausibly dealt with. Do the arguments seem compelling? Can you think of limitations on those arguments, or additional ways of dealing with the problem?

Part II Phonological Change

4 Galilean-Style Phonology

> "Sound change" is unfortunately a very bad and misleading term for the phenomena to which it is supposed to refer for the simple reason that what really changes is not sounds but grammars.
>
> *Postal (1968: 270)*

Before we can discuss the various types of phonological change, a number of definitional issues must be resolved. First, it is necessary to be as clear as possible about the nature of phonology itself. Of particular interest to us in the present context, as we shall see, is the precise synchronic relationship between phonology and phonetics – a matter neither conceptually simple nor uncontroversial. Once we have established a reasonable degree of terminological (and, hopefully, conceptual) clarity in this domain, we will be in a position to leverage this enhanced conception in developing a clearer notion of the issues which arise in the corresponding diachronic domain.

The title of this section refers to the matters raised by Chomsky in the following quote (2002: 98):

> What was striking about Galileo, and was considered very offensive at the time, was that he dismissed a lot of data; he was willing to say "Look, if the data refute the theory, the data are probably wrong" . . . But the Galilean style . . . is the recognition that it is the abstract systems that you are constructing that are really the truth; the array of phenomena is some distortion of the truth because of too many factors, all sorts of things. And so, it often makes good sense to disregard phenomena and search for principles that really seem to give some deep insight into why some of them are that way, recognizing that there are others you can't pay attention to.

In our brief discussion of the relationship between diachronic linguistic analysis and inscribed historical artifacts and manuscripts in the previous chapter, I am sure that it became clear that what appears, on the face of it, to be a relatively straightforward matter, turns out, upon deeper examination, to be quite complex. In particular, dealing with the complexity of the wide range of factors which shape the observed phenomenon demands a careful division of the analytical tasks into the archeological, philological, and linguistic domains. Segmenting the component research tasks which confront us in a particular scientific enterprise in this manner is one of the key mechanisms for dealing with the types of issues raised by Chomsky's discussion of Galileo. That careful consideration of the matter led us to believe that it was more complex than it first seemed is hardly surprising – it is, of course, a normal property of scientific investigation, which, throughout its history, has generally taught us that things are not as simple as they often seem under pretheoretical consideration. It should be equally unsurprising, then, that matters of considerable complexity are hidden behind the relatively trivial and seemingly uncontroversial definition of "phonology" as the study of "the sound systems of languages" (thus, e.g., Crystal 2003, s.v.).

In fact, the parallelism with the artifacts discussion above is, in my view, quite far-reaching. The "speech sounds" with which phonology is widely alleged to concern itself would seem highly concrete, measurable objects with a wide range of empirically determinable properties (much like the artifacts discussed in the last chapter) that should make them ideal candidates for scientific investigation. Indeed, the science of phonetics, coupled with numerous significant engineering breakthroughs in the acoustic and, perhaps equally importantly, computational domains, has been able in recent decades to provide a far richer body of quantitative material regarding human speech than has ever been available to scientists before. Interestingly, this increasingly sophisticated understanding of the physical nature of "speech sounds" has taken place largely independent of, and without direct impact upon, the development of models of human phonological systems. For example, movement towards "underspecified" phonological representations in the mid-1980s (see, e.g., the collected papers in *Phonology 5* – a special issue dedicated to this topic) was not in any significant sense a product of what was by that time a massive increase, relative to the state of the matter in the early days of generative phonology, in the sophistication of phonetic tools, nor, indeed, has the recent development of Optimality Theory been so motivated. Research into the architecture of the phonological system appears to have developed on its own, in spite of the enrichment of our knowledge of the phonetics of human speech during the relevant period.[1] It turns out that this separation between the detailed measurements of the phoneticians and the development of phonological theory – much like that between the archeological investigation of inscribed

[1] There are some apparent exceptions to this doubtless overly-broad claim. In rule-based phonology, "feature geometry" was often conceived of as arising directly from phonetic facts. However, the relevant "phonetic facts" were well-known, basic aspects of articulatory phonetics, not recent discoveries about acoustic or auditory phonetics. Similarly, so-called "phonetic grounding" in the work of Archangeli and Pulleyblank (e.g. 1994), and subsequently in Optimality Theory, is orthogonal to the machinery introduced by that theory, and to the motivation for the development of its apparatus.

objects and research into the linguistic history of a linguistic system – is appropriate and necessary, in my view. I will try to show this in what follows.

4.1 The Grammar, Production, and Perception

A sketch is presented in figure 4.1 of certain aspects of an event in which a speaker – one with a linguistic system much like that of the author – utters the word "cat" and is heard by another individual with a linguistic system like that of the author, in the relevant respects. In spite of the convoluted nature of the figure, it represents a gross oversimplification of what is involved in any actual speech event, though it is designed so as to capture those aspects most relevant to our present concern. It will be helpful to go through the diagram with some care, beginning with the "speaker" on the left.

In the upper left-hand corner of the figure (at ❶) we see a (partial) lexical representation, /kæt/ (note that the listener has stored in his or her mind the very same lexical representation, see ❾, to the extent details are provided in the figure). In keeping with normal practice in the field, a "phonemic" representation of this type is given between "slashes." Also in keeping with widespread practice in the field, the phonological feature bundles which are assumed to actually be used in the mental representation of phonemic segments have been abbreviated into symbols of the International Phonetic Alphabet.[2] Lexical representations such as this, in adults, are assumed to consist of only that information required to generate all of the allomorphs of a given morpheme.[3] They are stored in long-term memory, in what is generally called the "lexicon," to which items may be added throughout the lifetime of the speaker.

This underlying representation is subjected to, or serves as the input for, phonological computation (which may be of any of various types – ordered rules, an Optimality-Theoretic system, etc.), the result of that computation being a generated phonetic output, or surface, representation, usually included within square brackets (❷). The *representational alphabet* (i.e., the entities used in the construction) of these output representations is, it appears, the same as the

[2] This latter practice introduces the possibility of serious confusion – confusion which can be observed with regularity in the phonological literature, in my opinion – in that one and the same IPA symbol may, and in my opinion far more frequently than is generally assumed, does, represent what are in fact distinct phonological feature bundles. The matter will arise in some detail below, so I will postpone consideration of it at this time.

[3] See Hale and Reiss, forthcoming, for a more exhaustive discussion of these matters.

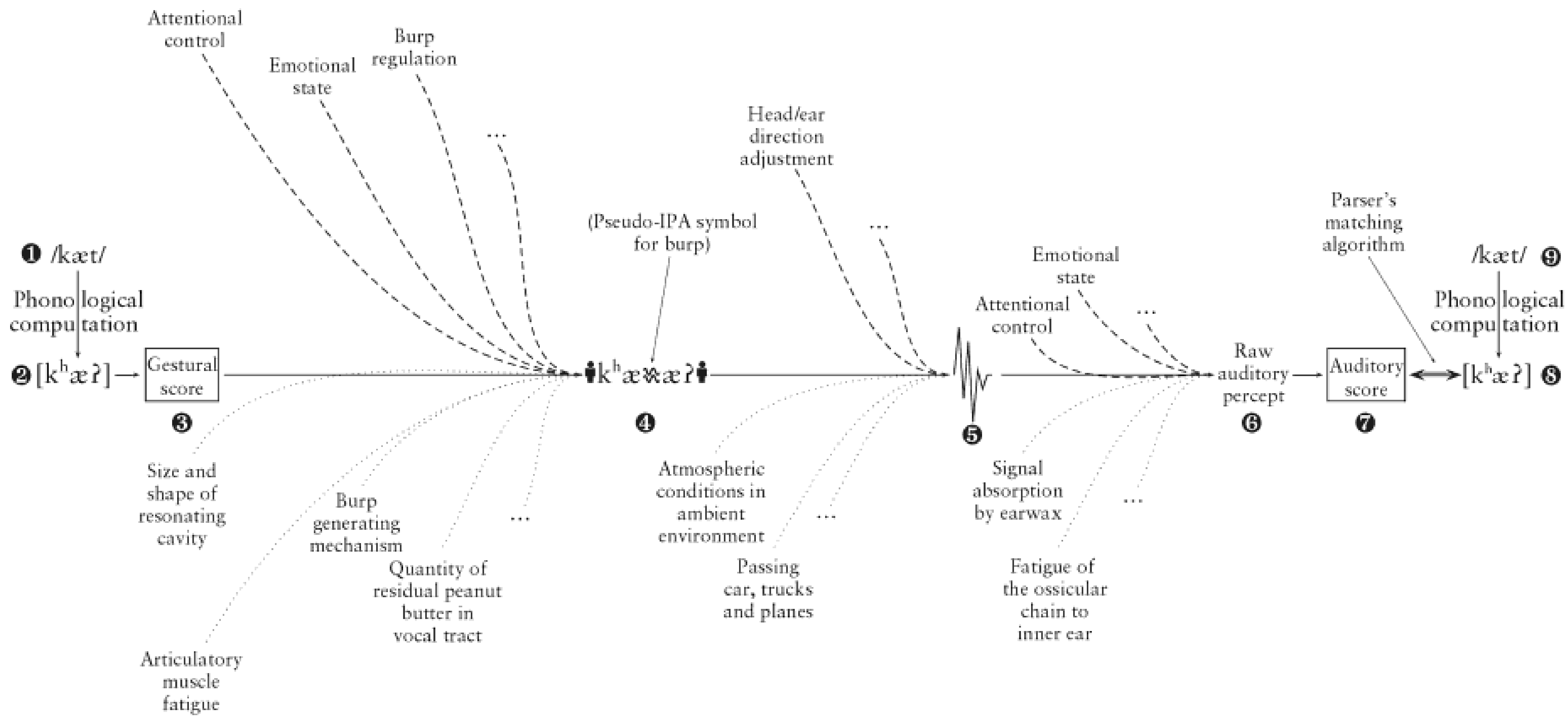

❶ /kæt/
Phonological computation
❷ [kʰæʔ]
Gestural score
❸
Attentional control
Emotional state
Burp regulation
…
Size and shape of resonating cavity
Articulatory muscle fatigue
Burp generating mechanism
Quantity of residual peanut butter in vocal tract
…
(Pseudo-IPA symbol for burp)
kʰæææʔ
❹
Head/ear direction adjustment
…
Atmospheric conditions in ambient environment
Passing car, trucks and planes
…
❺
Attentional control
Emotional state
…
Signal absorption by earwax
Fatigue of the ossicular chain to inner ear
…
Raw auditory percept
❻
Auditory score
❼
Parser's matching algorithm
[kʰæʔ] ❽
/kæt/ ❾
Phonological computation

Figure 4.1

representational alphabet of underlying forms.[4] Such output representations should thus consist of phonological features, metrical/prosodic structure representations, and the like.

In discussing this figure I will attempt to distinguish carefully between *computation*, such as that seen in the mapping from phonemic representations to phonetic representations, and what I will term *transduction*.[5] Computation involves the manipulation (reordering, regrouping, deletion, addition, etc.) of the elements present in the input *without a change in representational alphabet*. By contrast, transduction involves the mapping of an entity in one form onto a distinct form – the classic example is the transduction of air pressure differentials ("sound waves") into a stream of electrons by a microphone.

The distinction between computation and transduction provides a useful means of conceptualizing the broadly assumed modularity of the computational mind. A "module" can be thought of as a device which takes input representations (in some representational alphabet) and computes over these representations, generating thereby an output in the same representational system. The modules of the computational mind must be linked by a set of *transducers*, which convert material in one form into a form required by the computational module fed by the conversion process. We will see a concrete example of transduction when we turn to details of articulation and speech perception below.

The "phonetic" output representation (❷) is then subjected to *transduction* to a distinct representational system: the so-called *gestural score* (❸). This representation maps out, much in the manner of a musical score, the *relative* durational and dynamic properties of the intended articulatory target. The actual timing and, e.g., loudness, will arise through a combination of this relative information and other aspects of the behavior-generating system (e.g., the emotions of the speaker), as we shall see. It is assumed, as indicated by the fact that this mapping has been labeled a "transduction," that the gestural score does not consist of sets of phonological features bundled into abstract segments.

The process of getting from this "gestural score" to an actual articulatory act is one of tremendous complexity, involving numerous factors which arise in what I would assume to be a rather large number of intervening computational systems.

[4] This is not to say that a *particular* output representation may not contain information not present in the specific input form which gave rise to it, obviously. A typical output representation will contain, for example, an indication of the position of primary stress and syllable structure, both of which, if predictable, may be absent from the relevant input representation. However, in principle – i.e., as a property of the architecture of the system – such representational properties are not precluded from underlying representations (e.g., when not predictable, or, perhaps, in early stages of acquisition).

[5] The term *transduction* as used here is extended somewhat from its use in Cognitive Science by, e.g., Zenon Pylyshyn, his use itself an extended version of the way the term is employed in physics.

Little is known about the modules involved, and no exhaustive listing of relevant factors is possible, so I have satisfied myself with giving a list of a relatively small number of factors, which, however, covers a considerable conceptual range, and which must in some manner be involved. Doubtless some of the intervening modules involve computation, others certainly must involve transduction (since the output is some set of electrochemical neuromuscular signals). I have attempted to separate what one might think of as "cognitive," as opposed to more purely physical, factors, placing the former above, the latter below, the line which indicates the course of the computation.

For example, it is clear that the degree to which a speaker successfully focuses his or her attention on the act of articulation itself will affect quite directly certain aspects of the actual physical act. The modules of the mind responsible for attentional control (as well as all aspects of the mind/brain which interact with that module – e.g., the electrochemical effects of alcohol consumption) thus must play a role in what ultimately befalls the "gestural score" representation. Similarly, the emotional state of the speaker and, as indicated in the figure, whatever strategies the speaker uses to attempt to control an imminent burp ("burp regulation") – which may involve increasing the muscular tension in the vocal tract, for example. The reader can doubtless trivially expand the set of such cognitive factors which play some role in determining the properties of the ultimate bodily output of the speaker.

In addition to being embedded in this cognitive context, the generation of an actual physical signal will be determined in part by the noncognitive, physical context within which the utterance event takes place. At this point, we are concerned with physical effects *internal* to the speaker. These include relatively stable properties of the speaker (e.g., the size and shape of his/her resonating cavities, tongue, vocal folds, lungs, etc.) as well as more transient physical properties (e.g., the current fatigue of the relevant muscles, quantity of saliva or peanut butter or whatever in the vocal tract, etc.). Such factors thus vary both from individual to individual and from time to time, in the same individual. That these variables will play a role in the acoustic shape of the output is not a matter of linguistic speculation, but is rather given by the nature of the physical universe, of which the speaker's body forms a part.

The combined effects of these various factors is a *potential* acoustic output – the *actual* acoustic output, as we shall see, is a function of yet further factors. The form in "human body" brackets (❹) is meant to represent what the output of the speaker's body would be, ignoring all external influences. This is much like the notion of the rate of a falling body in a perfect vacuum – actual falling bodies will match this only to some vague approximation, since they are not in a perfect vacuum, but to understand how such bodies fall *outside* the vacuum, it is valuable to formulate a hypothesis about how they *would* fall, absent the accidental effects of being in an atmosphere. It is just such an idealized representation that the symbols between the "human body" brackets is intended to represent. This representation differs significantly in its basic properties from that of the "gestural score" (e.g., it includes, which the latter excludes, speech rate information, speaker-specific acoustic effects, and the like) – the overall process of getting from the gestural score to the idealized bodily output is thus one of transduction, rather than computation.[6]

At this point in the figure our concerns shift from the speaker – from whom the signal has now become fully independent – to the listener. For example, an actual acoustic wave (indicated by the vaguely wave-like graphic in the figure) will differ based on the listener's position relative to, and his/her distance from, the speaker. The acoustic wave (❺) is thus a *listener-specific* representation, as will be all following representations in our figure.

The actual factors influencing the form of this acoustic wave are again too complex to list in any detail. As with the speaker-specific considerations, I have attempted to separate cognitive from more purely physical considerations. In the latter category, it is again a matter of physics that the properties of the medium through which the signal is being transmitted (the "atmospheric conditions in the ambient environment") must influence the form of the acoustic wave which reaches the listener, as, indeed, must the interference patterns produced by other ambient acoustic waves. On the cognitive side, the listener has attention control systems which include the ability to manipulate the orientation of his/her auditory receptors relative to the sound source, which may be invoked at this point. The combined effect of these various factors will be some waveform reaching the ear of the listener – that waveform is indicated by the wavy graphic at ❺.[7]

This waveform is then subject to a range of physical and cognitive effects within the auditory system of the listener. The cognitive factors again include how much of his/her attentional resources the listener gives over to this acoustic signal, the emotional state of the listener, and the like. On the physical side, the signal will be modified by the ambient environment of the outer and inner ear, as well as by response fatigue in the relevant movable parts of the audition system. The representation which results from the effects of these various physical and cognitive factors I have called the "raw auditory percept" (❻). This representation must then be broken down into its component elements. This decomposition includes not only separating those aspects of the representation which are taken by the listener to be due to a speech signal in the input (as opposed to the sound of a passing truck, for example), but also an analysis of the speech signal

[6] Of course, since this process actually consists of numerous mapping events, any number of these mappings may in fact be computations, as long as at least one of them involves transduction. The point is merely that if we collapse these into a single "system," and even one of them involves transduction, then the representational alphabet of the output will be different from that of the input and the entire, simplified and collapsed, "system" will thus perform a transduction.

[7] I abstract away from the fact that biaural listeners get, in fact, two distinct acoustic signals and exploit the difference between them in the course of processing.

itself into "speaker voice quality," "speech rate," "speaker emotional state," and "linguistic content" components.[8] We are concerned at this point only with the "linguistic content" component of the "raw acoustic percept," which I will assume takes the form of an "auditory score" (❼) – similar, in many respects, to the "gestural score" on the production side. For example, the "auditory score," like the "gestural score," does not include rate information (but will include *relative* temporal durations, since these may be linguistically relevant), or speaker-identification cues and the like.

Parsing involves the establishment of a link between this auditory score and an appropriate output representation of the grammar, the parser being the device which evaluates matched pairs for suitability. Numerous complications arise at this point, most of which lie well beyond our narrow concerns here, as well as well beyond my competence, but the rough approximation in our figure should suffice for our relatively limited purposes. In this figure, candidate output representations (❽) are generated by the grammar from the listener's stored phonemic representations (❾) and checked against the auditory score,[9] presumably by means of an algorithm, which produces an auditory score from the grammar's output representation, not unlike that which generates the gestural score from the grammar's output representation.[10] As we saw on the speaker side of the figure, the relationship between the "phonetic" representation and the auditory score is assumed here to be one of transduction. This follows from some basic properties of the auditory score – e.g., the fact that it encodes information about relative temporal duration and timing relations (matters which a bundle of features do not directly encode).

The result of this process is the establishment in the mind of the listener of a phonemic/phonetic pair, linked by the grammar. The phonetic representation having been selected for its ability to transduce to an appropriate "auditory score," the phonemic representation for its ability to map, via phonological computation, to that phonetic representation. The phonemic representation thus posited can then be exploited by "higher-level" grammatical analysis.

Armed with this sketch of what could go on when one utters /kæt/ – a sketch which on the one hand seems almost ridiculously overly detailed but which, on the other hand, falls far short of an even vague claim to comprehensiveness – we can turn to the question of which aspects of this figure linguistics proper concerns

[8] As with most of the matters discussed regarding our seemingly elaborate figure, this list is not intended to be exhaustive, nor are the factors necessarily independent of one another.

[9] This is an "analysis by synthesis" model. Other possibilities exist – the details are not critical for our concerns here.

[10] Again, countless other possibilities exist. To give just one example, it may be that a gestural score is generated from the linguistic output representation, just as is done when one is speaking, and that gestural score is then "converted" to an acoustic score by some transductive and/or computational process(es).

itself with. It is important to bear in mind – as we noted in the last chapter when we discussed philology and linguistics – that there is a distinction between the sources of evidence which a scientific enterprise may make use of and the *object of study* of that field. Physics is not *about* linear accelerators, but understanding of and access to linear accelerator data has played a key role in the progress of modern physics. The question I am interested in at this point is that of what the object of study of linguistics, in the phonological domain, is.

Answering this question involves attempting to isolate narrowly linguistic concerns from the many and diverse factors which play a role in speech behavior. As linguists, it is not within our domain of responsibility to investigate why one speaker talks a great deal about dogs and another less so, or why one individual yells more than the average and another whispers, as interesting as these questions may appear. Nor are such questions answerable within the context of the types of explanations linguists are prepared to provide: the *grammar* does not tell you whether now is the time to tell that amusing anecdote about your favorite pet or loudly chastise your children. There is a difficulty, widely recognized in the philosophy of science literature, with conceptually isolating systems for investigation which do not in fact function in isolation. Nevertheless, as Lawrence Sklar (in part, paraphrasing Stephen Weinberg) has recently argued, there is reason to believe that the systems which interact to give us the world are isolable as a matter of fact, rather than simple methodological convenience. He notes that (Sklar 2000: 54–5)

> without a sufficient degree of isolability of systems we could never arrive at any lawlike regularities for describing the world at all. For unless systems were sufficiently independent of one another in their behavior, the understanding of the evolution of even the smallest part of the universe would mean keeping track of the behavior of all of its constituents. It is hard to see how the means for prediction and explanation could ever be found in such a world . . . it can be argued that unless such idealization of isolability were sufficiently legitimate in a sufficiently dominant domain of cases, we could not have any science at all.

The determination of what the "object of study" of linguistics, in the phonological domain, is thus requires that we examine our figure for some object or objects which could be subjected to serious scientific investigation *as if isolated*, i.e., without regard for "extralinguistic" considerations. No "isolation" lines are given for the phenomena pictured in that figure by the world itself, nor, indeed, are we provided by nature with a division between what is narrowly "linguistic" and what is "extralinguistic." As a result, these issues are essentially definitional, though, following Sklar, it seems sensible that the most useful method of breaking up our figure into isolated subsystems will be that which corresponds most closely to actual computational independence of the real-world systems involved.

4.2 What is a "Phonological Object"?

From Crystal's definition of "phonology" cited above – which explicitly mentions "sound" – it might appear that we should select, as the part of our figure which contains "phonological objects," something one could in principle hear (i.e., a "sound"). There is, in fact, only one such entity in the figure – the waveform representation (for a specific listener) indicated by the wavy graphic (❺). It would appear, as well, that there would be some concrete advantages to adopting this representation as the relevant one – e.g., it is, to a reasonable approximation, amenable to measurement.[11] However, in establishing just what one would have to attempt to remove from the waveform to achieve "isolability" it seems clear that the factors which lead from the "idealized bodily output" form, 🚹kʰæ✲æʔ🚹, to the waveform are precisely *not* linguistic factors: the cognitive processes by which one may adjust one's head position to optimize audition are psychologically interesting, but there is certainly no reason to believe – and no linguist has ever advocated it – that such adjustments result from *grammatical computation*.

In addition, it seems clear that the ambient atmospheric conditions and interference from passing sound-wave producing entities play a role in our deliberations only insofar as they are something we want to *exclude* from consideration in determining the linguistic properties of a waveform. But if the factors which get us to the waveform from the "idealized bodily output" form (at ❹ in the figure) are precisely nonlinguistic ones, then in the interest of isolating those components of our figure which are linguistic objects we would surely be better off targeting the idealized bodily output form. Selecting this as our object of study would already eliminate many factors which we want to exclude from narrow linguistic consideration in any event.

However, if we examine the factors that got us to the idealized bodily output form from the gestural score in ❸, those factors appear once again to be precisely of the type which we would not want to include within the scope of our *linguistic* investigation. It is doubtless fascinating just what computational and memory systems are leveraged to try to get yourself *not* to burp while saying "cat," and someone should surely be investigating such matters, but just as surely

[11] I say "to a reasonable approximation" because, on the one hand, acoustic measurements of a speech signal may contain far more information than a human can use in acoustic processing (and thus may contain irrelevant information), while on the other hand, these measurements generally fail to capture in any detail effects due to stereoscopic audition, head/ear orientation, and the like. Finally, such measurements are usually, at least in linguistic studies, taken in contexts quite different from those of normal speech transmission (e.g., they are made in echo-resistant rooms, using head-mounted microphones and headphones, etc.).

that someone is not the phonologist.[12] The same holds for the physical level of the mapping from ❸ to ❹: peanut-butter/saliva ratios in the vocal tract are, again, fascinating topics for scientific study, but they are not phonological topics.

So, if the factors which give rise to the "idealized bodily output" from the "gestural score" are nonlinguistic, then we do not want to include them within the scope of our object of study. This leaves only the phonemic representations (in ❶ and, for the listener, ❾), the phonological computation system (which isn't blessed with a number in our figure), the phonetic output representation (at ❷ and, for the listener, ❽), and the gestural and auditory scores (❸ and ❼). Since the "speaker-oriented" phonemic and phonetic representations are assumed to be the same formal object as the "listener-oriented" phonemic and phonetic representations, we'll ignore the listener side in what follows.

There is, I assume, no real controversy over whether or not it falls within the purview of the phonologist to concern him/herself with the phonemic representation, the phonological computation system, and the (epiphenomenal) phonetic representation. The only remaining issue in the delimitation of phonology thus concerns the "gestural" and "auditory" scores. For this question, my competence in this domain falters, so I will have to satisfy myself with a mere statement of the issues. Let us take the "gestural score" as our example, though presumably parallel arguments hold for the auditory side of things. I can envision two distinct possibilities. Under the first, the gestural score is generated by the same type of "action plan" processes which give rise to any coordinated physical activity. Imagine I have formulated some intent – presumably a mental representation of some type – to scratch my nose. Some systems of the mind/brain must convert that representation into a "nose-scratching" score, which contains the relevant key inflection points for the planned action, the relative timing of those inflection points (so that the slight lowering of my head, to which my nose is conveniently attached, will be timed to meet my rising hand, thus avoiding overly rough contact between the relevant objects), and the like. Similar considerations would hold for raising my arm, rolling over, and other useful and willfully incited physical acts. If the gestural score arises from the same modules of mind which are responsible for, in general, converting intentional representations into coordinated "scores," then the fact that they happen in this instances to be operating over linguistic, rather than nose-scratching, representations is of no scientific import, and the "gestural score" would remain outside the scope of the definition of a "linguistic object."

12 Again, although I've probably already said it too many times, this does not mean that the phonologist may not need or want to take into consideration the gains we've made in understanding the "burp regulation mechanism," if any, when considering the masses of empirical data which enter into his/her considerations. It just means that if I called my book about the burp regulation mechanism in my home town Ypsilanti – where it seems a little less active than it does in some other communities – "A Survey of Ypsilanti Phonology," there'd be something incredibly disingenuous about that title.

If, on the other hand, the process of generating the "gestural score" from the phonetic representation involves considerations which are unique to language – not unique because of properties of the phonetic representation, but unique because of the manner in which the transduction to a gestural score treats the objects of such a representation – then the gestural score would fall within the scope of linguistics proper.

The distinction may be of little practical significance in any event, for it would appear unlikely that the transduction processes involved in giving rise to the gestural score involve linguistic learning. We know that the acquirer must construct underlying representations and a phonological computation module in the course of the acquisition process. The evidence for this construction operation comes from data which has been made complex and messy by the intervention of "too many factors, all sorts of things." The learner must attempt to correct for these various factors (e.g., not take too seriously the acoustic output of people with a mouth full of crackers), but the target of learning in the phonological domain is limited to the relationship between two representations: the phonemic and the phonetic, and that relationship is heavily constrained by the restricted set of possible human phonological systems. Imagine that we were to assume that the conversion of the phonetic representation to a gestural score also required learning on the part of the acquirer. We would then have three elements which the learner must link by his/her positing of (1) an underlying representation, (2) a phonological computation system, and (3) a transduction-to-gestural score system. Without *a prioristic* knowledge of (3), the acquirer cannot know what the output of the phonological computation should be, and without that information, the acquirer cannot construct an underlying representation and computation system pair. Perhaps there are ways around this problem of which I am unaware, but I do note that at least in Minimalist circles in syntax, the argument that the articulatory/perceptual interface systems are invariant is a common assumption.

In conclusion, it would seem that the object of study of phonology, and thus of diachronic phonology, should be the underlying, phonemic representations and the phonological computation system (which are together responsible for all of the properties of the phonetic output representation). Valuable evidence regarding the nature of these entities and processes can come from a wide range of sources, including the study of phonetics, the study of sound change, the study of acquisition, etc. I will attempt to show in the rest of this part of the book how this definition impacts the pursuit of diachronic phonology.

4.3 Phonological Change

In table 4.1 we see a fairly typical example of a rather short sound-change exercise. It compares forms in so-called "written Tibetan" and Lhasa Tibetan. It

Table 4.1: A typical sound-change problem

Written Tibetan	*Lhasa Tibetan*	
drug	**ʈ^{h}uː**	'six'
bod	**p^{h}œː**	'Tibet'
t^{h}og	**t^{h}ɔɔ**	'roof'
ston	**tœ̃**	'autumn'
nub	**nuː**	'west'
lus	**lyː**	'body'
spos	**pœː**	'incense'

is assumed that "written Tibetan" represents a reasonable approximation of the linguistic ancestor of spoken Lhasa Tibetan.[13]

It is of some interest to consider just what skills are required to come up with the "right" answer to a problem such as this. A possible solution can be trivially produced by a simple recording of the differences between the forms in the left column and those in the right, and positing a set of "changes" – i.e., systematic symbol substitutions – which will produce the Lhasa forms from the written ones. Such solutions are commonly found in the answers provided to such problems by beginning students. I point out that noticing what is different between two forms which are placed side-by-side in a table such as 4.1 follows relatively straightforwardly from general human pattern recognition skills and requires no particular training, in linguistics or in any other discipline.

To develop an "interesting" solution to such a problem – where "interesting" means of some linguistic insight – more is demanded of the student than simply noting the differences in the forms in the two columns. For example, a typical "descriptive" solution to the problem may include statements such as:

- **d** is lost at the end of words
- **g** is lost at the end of words
- **n** is lost at the end of words, etc.

whereas we would normally expect the more linguistically sophisticated student to provide the single statement

- final consonants are lost

[13] In table 4.1, /ʈ^{h}/ represents an aspirated, retroflex voiceless stop, /œ/ represents a front, rounded vowel at the /ɛ/ height, and **ː** indicates vowel length. These data have often been discussed, insightfully, by John Ohala (e.g. 1981).

or some formal equivalent thereof. It is rarely necessary to point this out in great detail to students in your average historical linguistics course, since these students will have, in the phonology portion of their introductory linguistics courses, gotten used to making "insightful" generalizations of this type. An "insightful" solution to this problem might include, possibly in some more sophisticated formal notation (usually taken over from the currently fashionable theory of phonology), the changes:

- **o** becomes ɔ
- back vowels are fronted before coronals
- final obstruents are lost with "compensatory" lengthening
- final nasals are lost
- initial **s** is lost in onset clusters
- voiced (nonnasal) stops became voiceless aspirated stops
- **tʰr** became **ʈʰ**.

As is typical in problems involving highly restricted quantities of data, there are many indeterminacies regarding "the" solution. In the solution above, for example, several possibly false assumptions have been made. The first statement assumes that lowering preceded fronting (rather than fronting **o** then lowering both **o** and its fronted equivalent), though there is no evidence that this is the correct sequence. The third statement collapses final obstruent loss and the "compensatory" lengthening into a single event, though of course the vowels could have lengthened before final obstruents, with a more general final consonant loss (thus eliminating the need for the next statement) being an independent event. The assertion about voiced stops preceding the statement about **tʰr** assumes – if the stated ordering is to be of significance – that the original **dr** onset underwent the change of voiced stops to voiceless aspirates *before* retroflexion of the stop by **r**, though of course a sequence of the type **dr** > **ɖr** > **ɖ** > **ʈʰ** can by no means be excluded. There is a conflict, as we can see from this simple example, between being "insightful" – which requires that the solution make claims which hold over data not yet seen, and perhaps never to be available – and being narrowly accurate. It of course follows quite naturally that if "insightful" solutions go beyond the limits of the data presented, the researcher will often have to choose between various ways of doing this, hopefully guided by a general theory of possible, impossible, likely, and unlikely change events.

In general, students use insights gained in their phonological training to posit events which seem "plausible" from the perspective of traditional phonological concepts such as "natural classes" and processes like "assimilation."[14] There is something inherently suspect in this transfer of skills from the synchronic to the

[14] Historical linguists often do the same, though they tend to depend rather more heavily on the existence of parallels in the diachrony of other languages they may have happened to come to know.

Table 4.2: Not a typical sound-change problem

Middle English	*The author's English*	
/fæt/	**/væt/**	'vat'
/fæt/	**/fæt/**	'fat'
/fyksən/	**/vɪksn̩/**	'vixen'
/foks/	**/faks/**	'fox'
/fanə/	**/ven/**	'vane'
/fan/	**/fæn/**	'fan'

diachronic domain: why should the constraints on the kinds of processes which can go on within that part of the mind responsible for phonological computation be the same, or indeed, be related in any way to the set of possible relationships that can exist between an acquirer and his/her source? The latter is an observed set of similarities and differences between two minds (since grammars are knowledge states), and that set of similarities and differences can be catalogued and subjected to analysis by the problem-solving component of the scientist's mind (rather than, e.g., by his/her phonology). It seems clear that that general problem-solving skill is not constrained by the limits imposed by phonological computation.[15]

One of the most striking properties of sound-change problems of this type – a property which makes their solutions mechanical in a manner which readily invites comparison to the process of phonological derivation – is the "regularity" of the processes they provide evidence for. We do not generally find sound-change problems in our historical linguistics textbooks of the type seen in table 4.2, for example, although the data is just as "real" as the Tibetan data cited above.

In the table 4.2 data, there is no way to state a solution to the relationship between Middle English /f/ and my own labiodental fricatives by positing a regular, rule-like set of phonological events. There are various ways to deal with such data. For example, in a recent textbook Lyle Campbell (1999: 17) says:

> Sound changes are usually classified according to whether they are *regular* or *sporadic*. *Sporadic* changes affect only one or a few words, and do not apply generally throughout the language.

Confronted by the data in table 4.2, and the assertion quoted above, a student could simply posit that the change of /f/ to /v/ evidenced in some forms in the

[15] Of course, one uses problem-solving skills in analyzing phonological problems as well – the point is that the *solutions* developed by the synchronic phonologist must be consistent with the representational and computational capacities of the phonological portion of his/her mind, whereas the diachronic phonologist is not constrained in this manner.

Table 4.3: Also not a sound-change problem

Middle English	*The author's English*	
/θoxtə/	**/θɔt/**	'thought'
/broxtə/	**/brəŋ/**	'brought'
/lʊtər/	**/pjr̩/**	'pure'

table was simply an example of "sporadic sound change." However, if the student read further on the same page of Campbell's text, s/he would doubtless become somewhat confused about whether or not such a move was, in fact, acceptable. Five sentences after the passage cited above, Campbell's textbook says: "In fact, the most important basic assumption in historical linguistics is that sound change is regular, a fundamental principle with far-reaching implications for the methods that will be considered in later chapters." It is difficult to imagine how the regularity of sound change can be "the most important basic assumption in historical linguistics" if "sound changes are usually classified according to whether they are *regular* or *sporadic*."

Campbell is of course correct that the regularity of sound change, the so-called Neogrammarian Hypothesis, is a *necessary* assumption for much of the work that historical linguists do. It follows, of course, that if there is a process of "sporadic sound change," as he seems to entertain in the quoted passage above, the methods which depend on the regularity of change – e.g., the Comparative Method used in linguistic reconstruction – must be fundamentally flawed.

At first blush it may appear that the data in table 4.2 speak for themselves: sound change is not "regular." That such a facile conclusion cannot be drawn without further consideration can be seen from some similarly "irregular" data, such as that in table 4.3. The data in this table show that the fact that the phonological form associated with a given meaning comes to differ from what it was earlier is not in itself evidence that "sound change" has occurred. The existence of **/brəŋ/** in my dialect is not the result of phonological development (as shown by **/θɔt/**), but is instead to be attributed to "morphological" change of a fairly well-studied, but not at all well-understood, type. Similarly, the fact that I now say **/pjr̩/** where my linguistic ancestors would have said **/lʊtər/** is obviously to be attributed to the borrowing of a lexeme from some variety of French, rather than to the effects of "sound change."

The key question that this data gives rise to is this: is the event which gave rise to the **f:v** correspondence in table 4.2 of the same type as those reflected in table 4.1, or is it rather like some of the processes responsible for the data in table 4.3? Put another way, is it meaningful, as part of the scientific enterprise of historical linguistics, to distinguish between a set of events which display the "regularity" of table 4.1 and a set of events which do not, such as those whose effects can be seen in table 4.2 and 4.3?

The Neogrammarians themselves clearly posited a critical distinction between the type of processes involved in producing the data in table 4.1 and those responsible for table 4.3. The Neogrammarian Hypothesis regarding the "regularity" of sound change has in fact come under a great deal of fire recently – interestingly, not because of the superficially blatant exceptions offered by data such as that in table 4.2 – from a variety of corners. We will deal with such issues in considerable detail later in this chapter. Before doing so, let us expand the empirical basis of our investigations by turning to a more fully elaborated sketch, in the traditional style, of the historical phonology of a language.

4.4 Discussion Questions and Issues

A. The "Galilean" approach requires that we attempt to separate the various factors which give rise to observable data into distinct domains of research inquiry. A major issue which confronts any field is then the dividing of explanatory responsibility. Discuss how the distinction between, e.g., "computation" and "transduction" introduced in this chapter might be helpful (or unhelpful) in this pursuit.

B. The issue of "sporadic" vs. "regular" sound change is broached for the first time in this chapter – it will come up again as things proceed. Discuss ways in which it might make sense to establish and maintain a distinction between the two phenomena, or, alternatively, reasons why distinguishing between them seems like a bad idea to you.

5 The Traditional Approach

5.1 Marshallese Historical Phonology

In this section, we will walk through the analysis of some of the available data regarding the historical phonology of Marshallese.[1] We will pursue the analysis in a relatively superficial, unquestioning manner at the outset, using our initial treatment, which will be very similar to what you would be expected to produce on a typical "Introduction to Linguistics" examination, as the foundation for an increasingly deep exploration of the methods involved in pursuing diachronic phonological analyses.

We will consider a limited pool of data regarding the development of Proto-Micronesian (Pmc) forms into those of some variety of contemporary Marshallese (Mrs).[2] The consonant inventory of Proto-Micronesian is given in table 5.1, its vowel inventory can be seen in table 5.2.[3]

[1] The data is for the most part based on the Micronesian cognate material presented in Bender et al. (2003), though I have not hesitated to supplement the material there with lexical material and reconstructions from my own notes. The goal of working through this material is, as is typically the case with "problem sets" discussed in textbooks, to survey the methods involved, rather than to present a comprehensive sketch of the historical phonology of Marshallese. We could just as easily use constructed (i.e., "made-up") data sets for our purposes. I will thus gloss over the occasional complex problem, appropriate only for Micronesian or Oceanic specialists, which arises from the data. That said, I believe the analysis which will be developed is for the most part quite accurate.

[2] I realize that there are those who consider reconstructed forms overly hypothetical for such an enterprise, and who would therefore prefer that only developments between *attested* languages be used at this stage. It will be apparent when we come to consider the detailed issues concerning Marshallese historical phonology, as well as when we turn our attention to reconstruction methodology near the end of this book, that I do not share the anxiety these scholars feel about reconstructed data.

[3] The precise contrast between reconstructed *s and reconstructed *S, as well as that between *t and *T, is not completely decided. The phonemic properties of *c and *Z also await more precise specification. The contrasts play no role in the superficial history of Marshallese we are developing in this section.

Table 5.1: Proto-Micronesian consonant phoneme inventory

	Labial	*Labiodental*	*Labiovelar*	*Dental*	*Palatal*	*Velar*
Oral stops	*p	—	*p^{w}	*t *T	*c	*k
Fricatives	—	*f	—	*s *S	*Z	*x
Nasals	*m	—	*m^{w}	*n	*ñ	*ŋ
Liquids	—	—	—	*r *l	—	—
Glides	—	—	*w	—	*j	—

Table 5.2: Proto-Micronesian vowel phoneme inventory

	Front	*Central*	*Back*
High	*i	—	*u
Mid	*e	—	*o
Low	—	*a	—

In contrast to the PMC inventories, both the consonant and vowel systems of Marshallese seem somewhat complex. We will consider the details in depth later in this chapter; for now, the following superficial sketch should suffice. Following Bender (1968), we can consider the consonants of Marshallese as falling into three phonological classes: palatalized (which Bender terms "light"), velarized (which Bender calls "heavy") and round velarized (which Bender labels simply "round"). The segments actually found in each of these categories can be seen in table 5.3.

The Marshallese vowel system is, depending on your perspectives on such matters, either much more complex (in the sense of rare, or bizarre) or simpler (in the sense of number of segments) than its PMC source. A full consideration of the nature of this system must wait until we turn to the detailed discussion of Marshallese historical phonology in the next section. For the time being, we merely point out that the vowel phonemes of this language contrast only along the height dimension (not along the other common vocalic dimensions of backness and roundness).

Table 5.3: The Marshallese consonant phoneme inventory

	Labial	*Dental*	*Velar*	*Labial*	*Dental*	*Velar*	*Liquids*	*&*	*Glides*
"light"	p^{j}	t^{j}	—	m^{j}	n^{j}	—	l^{j}	r^{j}	j
"heavy"	p^{ɯ}	t^{ɯ}	k	m^{ɯ}	n^{ɯ}	ŋ	l^{ɯ}	r^{ɯ}	ɰ
"round"	—	—	k^{w}	—	n^{w}	ŋ^{w}	l^{w}	r^{w}	w

Table 5.4: The Marshallese vowel phoneme inventory

"high"	$\mathbf{V^i}$
"upper mid"	$\mathbf{V^{é}}$
"mid"	$\mathbf{V^e}$
"low"	$\mathbf{V^a}$

There are no official IPA symbols for "underspecified" vowels such as we find in Mrs. By convention, in discussions of Mrs the symbols for front (except for *a*) vowels are often used, /**i**/ representing the highest vowel, /**é**/ representing the upper-mid vowel, /**e**/ representing the lower-mid vowel, and /**a**/ representing the lowest vowel – none of them, of course, having any specification along the back/round dimension. I have found that it is incredibly difficult to bear in mind myself, and to get others to bear in mind, that when writing Marshallese forms the symbol /**i**/, for example, represents a high vowel with no specification for back/round, while in the same discussion, writing Proto-Micronesian ***i** is intended to represent a vowel which is high, like the Marshallese one, but *also* contrastively nonback and nonround. In an attempt to avoid this type of confusion, in earlier work I sometimes used /☕/ for the high vowel, /☎/ for the upper mid vowel, /☯/ for the lower mid vowel, and /⚽/ for the lowest vowel. While I'm certain that this allowed my readers to bear in mind that the Marshallese vowels were not of your normal, IPA-representable type, it appears to have given rise to a great deal of anxiety for readers and publishers. So in this book we will represent the Marshallese vowels with the standard symbols, but we will print those symbols as a superscript to the underspecified-looking symbol V, so the reader will be reminded that an /$\mathbf{V^i}$/ is not an /**i**/. The vowel inventory of Marshallese is thus as given in table 5.4.

Let us begin our diachronic investigation by focusing our attention on a relatively well-defined problem. Marshallese has both a round voiceless velar stop /$\mathbf{k^w}$/ and a nonround voiceless velar stop /**k**/. In addition, it has both a round velar nasal /$\mathbf{ŋ^w}$/ and a nonround velar nasal /**ŋ**/. As noted above, Proto-Micronesian is generally reconstructed with only plain, i.e., nonround, velar nasals and voiceless velar stops. If you are a student, you should now examine the data below (table 5.5) and attempt to determine the conditions under which the Marshallese contrast arose. (If you are a regular person, you may keep reading.)

I cite a rather lengthy list of forms here, although the pattern will doubtless become apparent relatively rapidly, so that we will have these forms at our disposal for subsequent discussion of other developments in Marshallese historical phonology.[4] I will generally provide, parenthetically, the somewhat more transparent

[4] By convention, a reconstruction such as ***fiŋi[sS]aki** 'be twisted' represents an indeterminacy between ***fiŋisaki** and ***fiŋiSaki** – there is no reflex of the form in any daughter language which distinguishes between *s and *S.

Table 5.5: Some evidence regarding the history of velars in Marshallese

Proto-Micronesian	*Marshallese*
***ciki** ‘*small*’	**r^{j}V^{i}k** ‘*small*’ (dik)
***falikuri** ‘*ignore, turn away*’	**jV^{a}l^{j}k^{w}V^{i}r^{ɯ}** ‘*turn one’s back on s.o.*’ (ālkur)
***fiŋi[sS]aki** ‘*be twisted*’	**jV^{i}ŋt^{ɯ}V^{é}k** ‘*be twisted*’ (iñtōk)
***ika** ‘*fish*’	**jV^{é}k** ‘*fish*’ (ek)
***iŋi** ‘*dorsal fin*’	**jV^{i}ŋ** ‘*spines on a fish*’ (iñ)
***kaŋi** ‘*eat (trans.)*’	**kV^{a}ŋ** ‘*eat (trans.)*’ (kañ)
***kapisi** ‘*anoint*’	**kV^{a}p^{j}V^{i}t^{ɯ}** ‘*anoint s.o.*’ (kapit)
***kiep^{w}u** ‘*spider lily*’	**kV^{i}jV^{é}p^{ɯ}** ‘*Crinum asiaticum*’ (kieb)
***kona-ni** ‘*his catch (of fish)*’	**k^{w}V^{e}n^{ɯ}V^{a}n^{j}** IIISg-poss. class. for ‘*catch*’ (koṇan)
***kup^{w}e** ‘*be bent*’	**k^{w}V^{é}p^{ɯ}** ‘*be bent*’ (kob)
***laka** ‘*constellation in Pegasus*’	**l^{ɯ}V^{a}k** ‘*constellation in Pegasus*’ (ḷak)
***lako** ‘*away*’	**l^{ɯ}V^{a}k^{w}** ‘*away*’ (ḷọk)
***laŋi** ‘*sky*’	**l^{j}V^{a}ŋ** ‘*sky*’ (lañ)
***laŋo** ‘*fly*’	**l^{ɯ}V^{a}ŋ^{w}** ‘*fly*’ (ḷọñ)
***loŋu** ‘*ant*’	**l^{w}V^{é}ŋ^{w}** ‘*ant*’ (ḷoñ)
***maŋi** ‘*have in mind, think*’	**m^{j}V^{a}ŋ** ‘*know better*’ (mañ)
***maŋo** ‘*top of head*’	**m^{j}V^{a}ŋ^{w}** ‘*top of head*’ (mọñ)
***noko** ‘*coconut leaf midrib*’	**n^{ɯ}V^{e}k^{w}** ‘*coconut leaf midrib*’ (ṇok)
***ŋii** ‘*tooth*’	**ŋV^{i}j** ‘*tooth*’ (ñi)
***ŋoro** ‘*snore*’	**ŋ^{w}V^{e}r^{ɯ}-** ‘*snore*’ (ñor-)
***pakewa** ‘*shark*’	**p^{j}V^{a}kV^{e}w** ‘*shark*’ (pako)
***pekopeko** ‘*cough*’	**p^{j}V^{e}k^{w}p^{j}V^{e}k^{w}** ‘*cough*’ (pokpok)
***piko** ‘*entangle*’	**p^{j}V^{é}k^{w}** ‘*entangle*’ (pok)
***p^{w}ukua** ‘*knee*’	**p^{ɯ}V^{i}k^{w}V^{é}j** ‘*knee*’ (bukwe)
***p^{w}uŋu** ‘*handle*’	**p^{ɯ}V^{i}ŋ^{w}** ‘*spear handle*’ (buñ)
***rakuraku** ‘*scoop up*’	**r^{ɯ}V^{a}k^{w}r^{ɯ}V^{é}k^{w}** ‘*scoop up*’ (rọkrok)
***roŋo** ‘*hear*’	**r^{ɯ}V^{e}ŋ^{w}** ‘*hear*’ (roñ)
***soko** ‘*hither*’	**t^{ɯ}V^{e}k^{w}** ‘*hither*’ (tok)
***takuru** ‘*back (of body)*’	**t^{j}V^{a}k^{w}V^{i}r^{ɯ}** ‘*turtle/crab shell*’ (jọkur)
***tokolau** ‘*northerly*’	**t^{j}V^{e}k^{w}l^{ɯ}V^{a}j** ‘*north wind*’ (jokḷā)
***toŋo** ‘*mangrove*’	**t^{j}V^{e}ŋ^{w}** ‘*mangrove*’ (joñ)

orthographic representations used in the Marshallese dictionary (Abo et al. 1976); the reader can easily familiarize him- or herself with how the two transcriptions relate.

A useful heuristic when considering a sound change problem such as this is to bear in mind that it is very frequently the case that sound changes are assimilatory in nature. In addition, conditioning factors (i.e., the causes of divergent development of a protosegment in different linguistic contexts) are typically local. Since you have been told that Mrs has innovated round velars, while preserving inherited unround velars as well, a useful guess going into your analysis would be that the relevant conditioning segments themselves have the property of being round, and of being adjacent to the velars under discussion. Since this is merely a heuristic, not a constraint on diachronic phonological development, one must, however, be prepared to deal with developments not consistent with this general approach. In this particular case, the velars of Pmc which end up round in Mrs are in fact all followed in Pmc by round vowels.[5] In addition, the velars of Pmc in table 5.5 which are followed by nonround vowels are, in Mrs, not round. We can summarize these developments with a sound change such as the following:[6]

(1) **Velar rounding:** [+velar] > [+round] / _ [+round]
- Velars were rounded before round segments.

The sound change in (1) looks very much like a phonological rule of the normal type, taking the Pmc forms as "underlying," or inputs, and the Mrs forms as their output.[7] The techniques for discovering diachronic "rules" of the type in

[5] The astute reader might have noticed that there is no data in table 5.5 which presents Pmc velars in an environment in which they are preceded by a round vowel and followed by a nonround one. The development of the velars in this environment, for which we have considerably less evidence than for the velars preceded by nonround vowels, remains in my view somewhat equivocal. For our purposes in this text, this need not detain us.

[6] Since there are no consonant clusters in Pmc, and no final closed syllables, all velars in that language were followed by vowels. The only immediately following round segments that could trigger velar rounding *en route* to Mrs were thus vowels. The sound change therefore need not specify this property.

In addition, it must be noted here that there are a healthy number of exceptions to the velar rounding process, given the reconstructions of Bender et al. (2003), almost all of them involving velars before ***u**. Some of these simply result from the reconstruction choices of those authors. For example, they reconstruct only ***kuli** 'skin, bark' in Proto-Micronesian for the ancestor of Mrs /**kV^{i}l^{j}**/, though many Oceanic languages point to the clear existence of a doublet ***kili** with the same semantics (the authors themselves cite Proto-Polynesian ***kili**, as well as numerous Micronesian forms from the Trukic languages which continue ***i** rather than ***u** in the first syllable). The problem is thus not uniquely a Marshallese one. In addition, many Micronesian languages, especially in the Trukic languages of that family, show centralization and even unrounding of ***u** under conditions which remain somewhat unclear (at least to me). It would appear that Marshallese may have shared some of these developments, and that the resulting reflexes of Pmc ***u** did not trigger rounding. The problem obviously is of sufficient complexity to stand outside the scope of this book.

[7] This fact has, in my opinion, given rise to a great deal of confusion, as I will argue in detail in the next chapter.

Table 5.6: Proto-Micronesian ***c** in Marshallese

Proto-Micronesian	*Marshallese*
***cam^{w}a** '*forehead*'	$\mathbf{r^{j}V^{a}m^{w}}$ '*brow, gable*' (daṃ)
***cam^{w}icam^{w}i** '*lick (intr.)*'	$\mathbf{r^{j}V^{a}m^{w}r^{j}V^{\varepsilon}m^{w}}$ '*lick (intr.)*' (daṃdeṃ)
***cam^{w}iti** '*lick (tr.)*'	$\mathbf{r^{j}V^{a}m^{w}V^{i}t^{j}}$ '*lick (tr.)*' (daṃwij)
***cowu** '*hand net*'	$\mathbf{r^{j}V^{\varepsilon}w}$ '*net for washing arrowroot*' (do)
***cuji-ni** '*his bone*'	$\mathbf{r^{j}V^{i}jV^{i}\text{-}n^{j}}$ '*his bone*' (diin)
***fici-ki** '*snip, snap, flip (tr.)*'	$\mathbf{jV^{i}r^{j}V^{i}k}$ '*shake (a hand), spear (tr.)*' (idik)
***kace** '*throw*'	$\mathbf{kV^{a}r^{j}}$ '*throw (a net)*' (kad)
***koca** '*coconut fiber*'	$\mathbf{k^{w}V^{e}r^{j}}$ '*coconut husk, fiber*' (kwōd)
***ŋaco** '*palate, gums*'	$\mathbf{\text{ŋ}V^{a}r^{j}}$ '*gums*' (ñad)
***p^{w}oca** '*turtle shell*'	$\mathbf{p^{\text{ɯ}}V^{e}r^{j}}$ '*turtle shell*' (bōd)
***p^{w}uce** '*foolish, stupid*'	$\mathbf{p^{\text{ɯ}}V^{\varepsilon}r^{j}}$ '*mistake, wrong*' (bōd)

(1) are also very similar to those techniques used in the study of synchronic phonology, with, however, some significant provisos. We will turn to these types of concerns later in this part of the book. For the time being, let us attempt to get a more complete version of what we might call the "traditional solution" to the historical phonology of Marshallese.

Not all sound changes show developments which are conditioned by their phonological context such as we observed above in the case of the velars, as we will now see from a consideration of the development of Pmc ***c** in Marshallese. The Pmc word for 'small,' ***ciki**, corresponds to Mrs /$\mathbf{r^{j}V^{i}k}$/. The development of the velar is consistent with our Velar Rounding rule. It would appear that ***c** corresponds to Mrs /$\mathbf{r^{j}}$/, though there is no additional evidence in table 5.5 which bears on this question. Further relevant material is presented in table 5.6.

This data reveals quite clearly that the normal development of Pmc ***c** in Mrs is /$\mathbf{r^{j}}$/. Interestingly, given our discussion of round velars in Mrs, the palatalization seen in the reflex of Pmc ***c** appears to be completely independent of the properties of adjacent segments – in fact, it appears to be an unconditioned, invariant development. For example, we find the palatalized development before back (***cuji-ni**) and nonback vowels (***ciki**), as well as after both back (***p^{w}uce**) and nonback (***ficiki**) vowels. This development can be captured by a sound change "rule," by convention written as follows:

(2) Pmc ***c** > Mrs /$\mathbf{r^{j}}$/

Note that, while we could state the development in a format which looks much like that of a normal, synchronic phonological rule, we do not expect to find unconditional rules of this type in the phonological system of a human language. This is because there would be no evidence available to an acquirer that would lead him/her to posit anything other than /**r**^{j}/ underlyingly in such a case (since /c/ would be *invariably* realized as [r^{j}]).

***ciki** also shows some vowel developments which, while relatively simple looking when considering only the case of ***ciki** itself, will in the end require some extensive consideration. These developments play a key role in shaping the overall structure of Mrs lexemes and thus warrant our careful attention at this juncture, although detailed discussion of their implications will await our more technical treatment of the history of Marshallese phonology in the next section.

The first ***i** in ***ciki** might seem to be preserved as such, as would many ***i**'s in the data in table 5.5 (e.g., the first ***i**'s in ***fiŋi[sS]aki**, ***iŋi**, ***kiep**^{w}, and ***ŋii**; the medial ***i**'s of ***kapisi** and, from table 5.6, ***cam**^{w}**iti**). However, we must remember that a Marshallese /**V**^{i}/ is not the same thing as an /**i**/ (a vowel phoneme which does not exist in Marshallese). So, the Pmc ***i** is preserved in the sense that there is still a vowel in the Marshallese daughter, not in the sense that that vowel has precisely the same phonological properties. By contrast, the final ***i** of ***ciki** is lost in Mrs, part of a very general pattern of final vowel loss, as can be seen from virtually every form in tables 5.5 and 5.6. The conditions on both the preservation and the loss of ***i** are a bit more complicated than this simple case might lead one to believe, as we will now show.

Let us turn first to the case of "preserved" Pmc ***i** in Mrs. Although, as indicated above, there are many instances in which Pmc ***i** is preserved as a high vowel (the only feature of ***i** which really can be maintained in Mrs, given that Mrs vowels are not contrastive along the backness and roundness dimensions), there are also cases already to be found in table 5.5 which indicate that, even when a vowel survives in the position of Pmc ***i**, that vowel is not always Mrs /**V**^{i}/. For example, ***ika** shows up in Mrs as /**jV**^{ɛ}**k**/ and ***piko** has the form /**p**^{j}**V**^{ɛ}**k**^{w}/. Further examples of this development can be seen in table 5.7.

Although it has been mentioned above that a general aspect of our heuristic in approaching sound-change problems is that conditioned sound changes tend to involve *local* conditioning, there is a well-known special exception to this generalization when it comes to the development of vowels. It is not at all unusual for vowels to be affected by the vowels of adjacent syllables, often regardless of the

Table 5.7: *i-lowering in Marshallese

Proto-Micronesian	*Marshallese*
***ila** '*come to land*'	**jV**^{ɛ}**l**^{j} '*nest*' (el)
***lima** '*bailer*'	**l**^{j}**V**^{ɛ}**m**^{j} '*bailer*' (lem)
***m**^{w}**aTie** '*sneeze*'	**m**^{ɯ}**V**^{a}**t**^{j}**V**^{ɛ}**j** '*sneeze*' (m̧aje)
***tiro** '*peer at*'	(**kV**^{a}**p**^{j}-)**t**^{j}**V**^{ɛ}**r**^{w} '*mirror*' ((kap)jer)
***tisi-ŋa** '*(to) point or face*'	**t**^{j}**V**^{i}**t**^{ɯ}**V**^{ɛ}**ŋ** '*point it in a certain direction (tr.)*' (jitōn̄)

Table 5.8: ***i** Preserved in Marshallese

Proto-Micronesian	*Marshallese*
***anitu** '*god, spirit*'	**ɰV^{a}n^{j}V^{i}t^{j}** '*god*' (anij)
***manifi** '*thin*'	**m^{j}V^{a}n^{j}V^{i}j** '*thin, flimsy*' (māni)
***taŋi-si** '*cry for s.o. (tr.)*'	**t^{j}V^{a}ŋV^{i}t^{ɯ}** '*cry for s.o. (tr.)*' (jañūt)

Table 5.9: Some ***u** developments in Marshallese

Proto-Micronesian	*Marshallese*	*From table:*
***falikuri** '*ignore, turn away*'	**jV^{a}l^{j}k^{w}V^{i}r^{ɯ}** '*turn one's back on s.o.*'	5.5
***p^{w}ukua** '*knee*'	**p^{ɯ}V^{i}k^{w}V^{ɛ}j** '*knee*'	5.5
***p^{w}uŋu** '*handle*'	**p^{ɯ}V^{i}ŋ^{w}** '*spear handle*'	5.5
***takuru** '*back (of body)*'	**t^{j}V^{a}k^{w}V^{i}r^{ɯ}** '*turtle/crab shell*'	5.5
***cuji-ni** '*his bone*'	**r^{j}V^{i}jV^{i}-n^{j}** '*his bone*'	5.6
***kup^{w}e** '*be bent*'	**k^{w}V^{ɛ}p^{ɯ}** '*be bent*'	5.5
***p^{w}ukua** '*knee*'	**p^{ɯ}V^{i}k^{w}V^{ɛ}j** '*knee*'	5.5
***p^{w}uce** '*foolish, stupid*'	**p^{ɯ}V^{ɛ}r^{j}** '*mistake, wrong*'	5.6

presence or nature of intervening consonants. If we examine the data in table 5.7, as well as the relevant data from tables 5.5 and 5.6, in light of this special exception it immediately becomes clear that in all of the instances in which Pmc ***i** shows up as /V^{ɛ}/ in Mrs it was followed in the next syllable in Pmc by a nonhigh vowel (i.e., either ***a**, ***e**, or ***o**). Note that this development is assimilatory in nature, though the assimilation takes place "across" intervening material. As expected given this lowering, in the many instances in which ***i** is preserved as a high vowel in Mrs, it is followed in the following syllable by a high vowel (either ***i** or ***u**). A few additional examples of this development are given in table 5.8.

Since we know that Mrs does not have a contrast between /**i**/ and /**u**/, having only one high vowel, it is of some interest to consider at this juncture whether Pmc ***u** might not behave identically, for the purposes of being lowered by mid and low vowels, to Pmc ***i**. Relevant data include several forms which have already been mentioned, repeated for your convenience here in table 5.9. This evidence clearly supports the idea that the collapse of ***i** and ***u** can precede the height changing events. The ***i**- and ***u**-lowering process can be captured by a sound change as in (3).

(3) **High vowel lowering:** *V[+hi] > $/V^{e}/$ / _ C V[–hi]
- ***i** and ***u** were lowered when the vowel of the following syllable was not high.

We will also need a general sound change which strips specifications for backness (this includes both [+back] and [–back] features, of course) and roundness (again, including both [+round] and [–round]) from all Marshallese vowels, converting PMC ***i** into MRS $/V^{i}/$. This same process would lead ***u** to collapse with ***i** (since they differ from one another only in their specifications on the backness and roundness dimensions), as MRS $/V^{i}/$. In addition, PMC ***e** and ***o** should also collapse, in this case as MRS $/V^{e}/$. Finally, PMC ***a** should, under this analysis, yield MRS $/V^{a}/$. Ignoring for the moment the height-altering effects of processes we are considering right here, one can confirm in its general outlines the validity of this understanding of the vocalic developments by a cursory examination of the data already cited.

Directing our attention now to the loss of ***i** in MRS, we recall that the loss of the final ***i** of ***ciki** "small" reflects a very general pattern of loss of final vowels, attested throughout the data. We can characterize this process as in (4).

(4) **Final vowel deletion:** V > Ø / _ #
- Word-final vowels are lost in MRS.

While the addition of the change in (4) essentially completes our explanation of the development of PMC ***ciki** to MRS $/r^{j}V^{i}k/$, there are two issues which arise concerning the loss of ***i** which should be discussed before we move on to consider additional data. First, there are other instances of PMC ***i** which show loss in MRS even though the ***i** in question is *not* in word-final position. We need to make sure that the loss in ***ciki** is not to be attributed to this other ***i**-loss process. Secondly, there is some evidence that other diachronic phonological events must be ordered before the various processes which trigger the loss of ***i**. We will deal with these issues in turn.

We already have some evidence for the loss of nonfinal ***i** in the data we have cited above. For example, PMC ***falikuri** and ***fiŋi**[sS]**aki** from table 5.5, and, from table 5.6, ***cam^{w}icam^{w}i**, all show loss of their medial ***i**. Additional evidence is provided in table 5.10.

In table 5.10, I have separated the first four instances, which involve loss of an ***i** in the first syllable of the word, from the final four, which involve, like those cases cited in our earlier tables, loss of a medial ***i**. Parallels to the first two instances involving other vowels are readily available, as can be seen from the data in table 5.11.

These cases contrast rather strikingly with other trisyllabic forms of PMC which show preservation of the vowel of the initial syllable rather than its loss. We have also already seen numerous forms of this vowel-preserving type: ***kapi-si, *kona-**

Table 5.10: Some further instances of ***i**-loss in Marshallese

Proto-Micronesian	*Marshallese*
***ŋiŋiri** '*growl, rumble*'	**ŋŋV^{i}r^{ɯ}** '*groan, rumble, grunt*' (ññūr)
***ŋiŋiTi** '*chant, laugh*'	**ŋŋV^{i}t^{j}** '*groan, moan*' (ñij-)
***pipia** '*sand, beach*'	**p^{j}p^{j}V^{ɛ}j** '*sandbank*' (ppe)
***TiTiri** '*spurt, squirt*'	**t^{j}t^{j}V^{i}r^{ɯ}** '*slippery, lubrication*' (jjir)
*[fØ]ani[fØ]ani '*to bail*'	**jV^{a}n^{j}jV^{ɛ}n^{j}** '*to bail*' (ānen)
***fiŋifiŋi** '*to be twisted*'	**jV^{i}ŋjV^{i}ŋ** '*be kinky (of hair)*' (iñiñ)
***p^{w}alili-ni** '*its covering*'	**p^{ɯ}V^{a}l^{j}l^{j}V^{i}-n^{j}** '*its clothing, covering*' (ballin)
***talitali** '*rope*'	**t^{j}V^{a}l^{j}t^{j}V^{ɛ}l^{j}** '*to roll up, coil*' (jāljel)

Table 5.11: Vowel loss in initial syllables in Marshallese

Proto-Micronesian	*Marshallese*
***kakaŋi** '*be sharp*'	**kkV^{a}ŋ** '*be sharp*' (kkañ)
***mamasa** '*be low tide, dry*'	**m^{j}m^{j}V^{a}t^{ɯ}** '*emerge from water*' (mmat)
***pepei** '*build stone structure*'	**p^{j}p^{j}V^{ɛ}j** '*build a rockpile*' (ppe)
***pepeti** '*to float*'	**p^{j}p^{j}V^{ɛ}t^{j}** '*float*' (ppej)
*[sS]o[sS]owu '*to dig*'	**t^{ɯ}t^{ɯ}V^{ɛ}w** '*to dig taro*' (tto)

ni, *pakewa, *p^{w}ukua,[8] and ***takuru** of table 5.5, ***cam^{w}i-ti, *cuji-ni,** and ***fici-ki** of table 5.6, as well as ***m^{w}aTie** and ***tisi-ŋa** of table 5.7. It is not difficult to discern the contrast in conditioning environment for preservation vs. loss of the vowel of an initial PMC syllable: the vowel is lost only if it is between identical consonants.[9]

The second pattern seen in table 5.10 involves the loss of the ***i** of the second syllable of a word of at least four syllables. That this pattern as well involves not just ***i**, but all of the vowels of PMC, can be seen from the development of forms such as ***pekopeko, *rakuraku,** and ***tokolau** from table 5.5, as well as from the additional data in table 5.12.[10]

[8] Note that in PMC it is assumed that all vowels head their own syllable – i.e., no orthographic vowel–vowel sequences are intended to represent diphthongs. Thus ***p^{w}ukua** and ***m^{w}aTie** each have three syllables.

[9] In fact, in at least the clearest cases known to me, the lost vowel was always part of an initial CV reduplication. Closely related Micronesian languages still have the vowel in this reduplicative syllable.

[10] I would like at this juncture to remind the reader that none of the data sets presented in the figures in this chapter aims for comprehensiveness. Much more data could be cited in virtually every case.

Table 5.12: Vowel loss in the second syllable of four-syllable words

Proto-Micronesian	*Marshallese*
***faSofaSo** '*to plant; plant*'	**ɰVᵃtᶭɰVᵃtᶭ** '*plant, vine*' (atat)
***manamana** '*have spiritual power*'	**mʲanᶭmʲVᵃnᶭ** '*haunted*' (mo̧n̄mo̧n̄)
***mʷakumʷaku** '*arrowroot*'	**mᶭVᵃkmᶭVᵉ̇k** '*arrowroot*' (m̧akm̧ōk)
***mʷaremʷare** '*necklace*'	**mᶭVᵃrᶭmᶭVᵃrᶭ** '*necklace*' (m̧arm̧ar)

Adopting the general assumption that stressed vowels are never lost while their unstressed neighbors are preserved, it seems likely that the general vowel loss patterns we have now observed point us towards some aspects of PMC stress placement. The patterns we have seen, ignoring for the moment the loss of the vowel in the reduplicating syllable, can be summarized as in (5).

(5) **Attested patterns of V-loss between Proto-Micronesian and Marshallese:**
$^*C_1V_1C_2V_2 > C_1V_1C_2$ ($^*V_2$ lost)
$^*C_1V_1C_2V_2C_3V_3 > C_1V_1C_2V_2C_3$ ($^*V_3$ lost)
$^*C_1V_1C_2V_2C_3V_3C_4V_4 > C_1V_1C_2C_3V_3C_4$ ($^*V_2$ and $^*V_4$ lost).

If we interpret this pattern in terms of stress placement, it seems clear that final vowels were never stressed (they are always lost, regardless of syllable count). In addition, the penultimate syllable, which would be syllable 3 in a four-syllable word, syllable 2 in a three-syllable word, and the first syllable of a disyllabic word, is never lost. Moreover, leaving aside the CV-reduplication case mentioned above (to which we will return), the vowel of the first syllable is never lost. The data thus points to either penultimate or initial stress. Since secondary stresses are normally (speaking in cross-linguistic terms) found every other syllable away from the main stress (in either direction), and since secondarily stressed vowels are also maintained in preference to fully unstressed ones, we can test out which stress placement matches best the vowel developments under discussion. I will assume that so-called *posttonic* unstressed vowels were lost.[11] In (6) I sketch the predictions of the theory that Proto-Micronesian had initial stress, in (7) those which would follow if Proto-Micronesian had penultimate stress.

(6) **Expected stress patterns of PMC if initial stress:**
$^*C_1\acute{V}_1C_2V_2$ should lose unstressed, posttonic V_2 (and does), should preserve stressed V_1 (and does)
$^*C_1\acute{V}_1C_2V_2C_3\grave{V}_3$ should lose only unstressed, posttonic V_2 (but does not!), should preserve stressed V_1 (and does) and secondarily-stressed V_3 (but does not!)

[11] Posttonic unstressed vowels are those whose syllables immediately follow a stressed syllable – doubtless the actual facts involve foot structure.

Table 5.13: Words of more than four syllables

Proto-Micronesian	*Marshallese*
***ma[sS]ali[sS]ali** '*smooth*'	$\mathbf{m^{j}V^{e}t^{ɯ}V^{a}l^{j}t^{ɯ}V^{ɛ}l^{j}}$ '*smooth*' (metaltal)
***matoatoa** '*firm, strong*'	$\mathbf{m^{j}V^{a}t^{j}V^{e}wt^{j}V^{e}w}$ '*firm, solid*' (mājojo)

$^*C_1\acute{V}_1C_2V_2C_3\grave{V}_3C_4V_4$ should lose unstressed posttonic V_2 and unstressed, posttonic V_4 (and does in both cases), should preserve the remaining vowels (and does).

(7) **Expected stress patterns of PMC if penultimate stress:**
$^*C_1\acute{V}_1C_2V_2$ should lose unstressed, posttonic V_2 (and does), should preserve stressed V_1 (and does)
$^*C_1V_1C_2\acute{V}_2C_3V_3$ should lose unstressed, posttonic V_3 (and does); should preserve stressed V_2 (and does) and pretonic unstressed V_1 (and does)
$^*C_1\grave{V}_1C_2V_2C_3\acute{V}_3C_4V_4$ should lose unstressed posttonic V_2 and unstressed, posttonic V_4 (and does in both cases), should preserve the remaining vowels (and does).

While it is difficult to find certain examples of longer words – and any such words will certainly be morphologically complex – the data from the secure examples of this type appears to point in the same direction as the data above, as can be seen from the forms in table 5.13.[12]

The two proposed stressed patterns would work as follows in this data:

(8) **Initial stress hypothesis: *mátoàtoà** > pre-MRS ***matata**
Penultimate stress hypothesis: *matòatóa > pre-MRS ***matoto**

Given that we want the mid vowels to survive (to give the /V^e/'s of the Marshallese form), rather than the low vowels, penultimate primary stress, with a secondary stress every other syllable to the left of the primary stress, would appear to conform to the attested diachronic record better than the assumption of initial stress does.[13] If we make such an assumption, and throw in a rather *ad hoc* (but not counterindicated) sound change for our initial CV-reduplication cases, our vowel loss processes can be summarized as in (9ab).[14]

12 The vowel alternations in the MRS reflex of ***ma[sS]ali[sS]ali** will be discussed shortly.

13 In rough terms, this corresponds to Rehg's (1993) reconstruction of Proto-Micronesian prosody.

14 To simplify the graphics of the rule I have marked the stress as $\acute{V}$ for both primary and secondary (and tertiary, etc.) stress.

Table 5.14: A Marshallese morphological alternation

Proto-Micronesian	*Marshallese*
***cúji** 'bone'*	$\mathbf{r^{j}V^{i}j}$ '*bone*' (di)
***cují-ni** 'bone of'*	$\mathbf{r^{w}V^{i}jV^{i}\text{-}n^{j}}$ '*bone of*' (diin)
***tanísi** 'finger/toe'*	$\mathbf{t^{j}V^{a}n^{j}V^{i}t^{\text{ɯ}}}$ '*claw, finger*' (jānit)
***tànisí-ni** 'finger/toe of'*	$\mathbf{t^{j}V^{a}n^{j}t^{\text{ɯ}}V^{i}\text{-}n^{j}}$ '*claw of/finger of*' (jāntin)

(9a) **Unstressed posttonic V-loss**: V > Ø / V́C_

- Delete vowels in unstressed syllables which follow a stressed syllable.

(9b) **Reduplicated pretonic syllable reduction**: V > Ø in a CV reduplicant.

The process in (9a) subsumes our rule of final vowel deletion in (4), and thus replaces it in our analysis.

The processes in (9ab) also appear to provide an account for some interesting morphological properties of Marshallese, seen in table 5.14. In the first two examples, we see that because Pmc has a penultimate stress system, the addition of the "possessive suffix" ***-ni** induces a shift of the stress from the first syllable of ***cúji** to the second in ***cují-ni**. In the simple form, then, the final vowel (posttonic and unstressed), which is the second vowel of the noun, is lost in Mrs. However, in the suffixed form, that final vowel is protected from deletion by the stress shift. In addition, the first syllable of the noun does not undergo deletion (because it is not posttonic). So in possessives the full set of vowels of a disyllabic root survive (with qualitative changes, of course) into Mrs.

In the second pair of forms the alternations are more extreme. Because the initial word form is trisyllabic, its first two vowels survive in Mrs. The final vowel, being, as usual, unstressed and posttonic, is lost. However, in the suffixed form, the stress is shifted to that original final vowel (which will of course now survive), and a secondary stress will now be located on the initial syllable. This subjects the vowel of the second syllable to deletion, since it is now both unstressed and posttonic (in this case following a secondary stress). This pattern is somewhat rare, since in general Pmc roots were disyllabic (rather than trisyllabic).

Let us turn now to the second issue alluded to above – the fact that there appear to be some processes which must have applied before the vowel losses covered by the developments sketched in (9ab) took place. We begin with developments surrounding the loss of ***i** – the vowel we started this long discussion with. We have already seen some of the relevant data, which I repeat in table 5.15. To round out the picture, we will also consider the data in table 5.16.

It seems clear from the data in tables 5.15 and 5.16 that ***i** is implicated in the coming into being, in Marshallese, of the "fourth" vowel height (/$V^{\acute{e}}$/). We saw this earlier with the ***i**-lowering rule in (3). In the data we are now considering,

Table 5.15: Some effects of ***i**-loss

Proto-Micronesian	*Marshallese*	*From table:*
***fiŋi[sS]aki** '*be twisted*'	**jV^{i}ŋt^{ɯ}V^{é}k** '*be twisted*'	5.5
***cam^{w}icam^{w}i** '*lick (intr.)*'	**r^{j}V^{a}m^{w}r^{j}V^{é}m^{w}** '*lick (intr.)*'	5.6
*[fØ]**ani**[fØ]**ani** '*to bail*'	**jV^{a}n^{j}jV^{é}n^{j}** '*to bail*'	5.10
***talitali** '*rope*'	**t^{j}V^{a}l^{j}t^{j}V^{é}l^{j}** '*to roll up, coil*'	5.10
***pepei** '*build stone structure*'	**p^{j}p^{j}V^{é}j** '*build a rockpile*'	5.11
***pepeti** '*to float*'	**p^{j}p^{j}V^{é}j** '*float*'	5.11
***ma[sS]ali[sS]ali** '*smooth*'	**m^{j}V^{e}t^{ɯ}V^{a}l^{j}t^{ɯ}V^{é}l^{j}** '*smooth*'	5.13

Table 5.16: Further evidence for the effects of ***i**

Proto-Micronesian	*Marshallese*
***ñoñi** '*Morinda citrifolia*'	**n^{j}V^{é}n^{j}** '*Morinda citrifolia*' (nen)
***peni** '*coconut meat*'	**p^{j}V^{é}n^{j}** '*grated coconut*' (pen)
***pinati** '*stop up, plug (tr.)*'	**p^{j}V^{i}n^{j}V^{é}t^{j}** '*hide, cover (tr.)*' (pinej)

it is not ***i** itself which develops into /V^{é}/ – instead, ***i** appears to be responsible for the raising of mid and low vowels, under some conditions, to this height.

As we saw above concerning the process of high vowel lowering (stated in (3)), ***i** and ***u** were treated alike for that vowel–vowel interaction process. It is thus useful to consider now whether ***u** shows effects on nearby vowels similar to those triggered by ***i**. Relevant data is given in table 5.17. From this data it

Table 5.17: Some effects of ***u**-loss

Proto-Micronesian	*Marshallese*	*From table:*
***kiep^{w}u** '*spider lily*'	**kV^{i}jV^{é}p^{ɯ}** '*Crinum asiaticum*'	5.5
***loŋu** '*ant*'	**l^{w}V^{é}ŋ^{w}** '*ant*'	5.5
***rakuraku** '*scoop up*'	**r^{ɯ}V^{a}k^{w}r^{ɯ}V^{é}k^{w}** '*scoop up*'	5.5
***cowu** '*hand net*'	**r^{j}V^{é}w** '*net for washing arrowroot*'	5.6
*[sS]**o**[sS]**owu** '*to dig*'	**t^{ɯ}t^{ɯ}V^{é}w** '*to dig taro*'	5.11
***m^{w}akum^{w}aku** '*arrowroot*'	**m^{ɯ}V^{a}km^{ɯ}V^{é}k** '*arrowroot*'	5.12
***marewu** '*thirsty*'	**m^{j}V^{a}r^{ɯ}V^{é}w** '*thirsty*' (maro)	new
***m^{w}onu** '*squirrel fish*'	**m^{ɯ}V^{é}n^{j}** '*squirrel fish*' (m̧ōn)	new
***m^{w}otu** '*ended, finished*'	**m^{ɯ}V^{é}t^{j}** '*finished*' (m̧ōj)	new
***woñu** '*turtle*'	**wV^{é}n^{j}** '*turtle*' (wōn)	new

does indeed appear to be the case that ***u*** also triggers raising of mid and low vowels to Mrs /$V^{é}$/, just as ***i*** does.

Let's turn first to the mid vowel raising cases. For ***i**, the relevant examples include (***pepei, *pepeti, *ñoñi,** and ***peni**). For ***u**, we find ***kiep^{w}u, *cowu, *[sS]o[sS]owu, *loŋu, *marewu, *m^{w}onu, *m^{w}otu,** and ***woñu**. The context for this raising seems quite clear from these examples: the mid vowel must occupy the syllable immediately preceding the high vowel trigger. In all of the examples cited above, the high vowel has been deleted. This is hardly surprising. For the raising of the mid vowel to show up in Mrs, the mid vowel must avoid deletion. Normally, this requires that it be stressed. Since the high vowel trigger must occupy the immediately following syllable, that syllable will itself be unstressed and posttonic, and thus will undergo deletion by the process in (9a).

We can summarize the process of mid vowel raising as in (10).

(10) **Mid vowel raising:** V́[–hi,–lo] > /$V^{é}$/ / _ C V[+hi] (before (9a))
- A stressed mid vowel is raised to /$V^{é}$/ before a high vowel in the following syllable.

We must next consider the cases of the raising of ***a** by high vowels. That the conditioning is not precisely the same as that seen above for the raising of mid vowels can be seen from examples such as ***kakaŋi > /kkV^{a}ŋ/** from table 5.11. We do not find raising of the ***a** here, though in the structurally similar ***pepeti** > **/p^{j}p^{j}V^{é}t^{j}/**, also cited in table 5.11, raising is found. Clearly, more than just a high vowel in the following syllable is required to trigger the raising of low vowels.[15] In fact, what appears to be distinctive about the raised ***a**'s is that they are *both* preceded by a syllable which contains a high vowel, *and* followed by such a syllable. As was the case with mid vowel raising, the affected ***a** is stressed, but the development of ***pináti** (which becomes Mrs **/p^{j}V^{i}n^{j}V^{é}t^{j}/**) shows that at least the preceding high vowel need not delete for the raising to be triggered. We could state the rule as in (11) at this point.

(11) **Low vowel raising:** V́[+lo] > /$V^{é}$/ / V[+hi] C _ C V[+hi] (before (9a))
- A stressed low vowel is raised to /$V^{é}$/ if it is both preceded and followed by a syllable containing a high vowel.

Note, however, that in order for the vowel to get from its original low position to the height of the /$V^{é}$/, it must *pass through* the mid vowel space. It is quite easy to see that, since the context for the raising of the low vowels *includes* the context for the raising of the mid vowels – the low vowels require a high vowel on each side, while for the mid vowels a high vowel on the right suffices – we could raise ***a** between high vowels only as far as /V^{e}/, ordering this raising

[15] This conclusion is supported by many forms we have seen above, including, but not limited to, ***p^{w}alili-ni > /p^{ɯ}V^{a}l^{j}l^{j}V^{i}-n^{j}**, ***tanisi-ni > /t^{j}V^{a}n^{j}t^{w}V^{i}-n^{j}**, and ***kaŋi > /kV^{a}ŋ/**.

before the raising of mid vowels by the sound change in (10), and vowel would end up in the /$V^{\text{ė}}$/ position in any event. Thus the rule in (12) could account equally well for the data. In the more detailed and technical considerations later in this chapter we will discuss whether (11) or (12) is to be favored, and why.

(12) **Low vowel raising:** V́[+lo] > /$V^{\text{ė}}$/ / V[+hi] C _ C V[+hi] (before (10))
- A stressed low vowel is raised to /$V^{\text{ė}}$/ if it is both preceded and followed by a syllable containing a high vowel.

With this, we have completed our consideration of the first form, ***ciki**, of table 5.5. You will be doubtless happy to hear that consideration of the remaining forms will not involve a discussion anywhere near as protracted as the one we have just come through. A number of processes of course remain – Marshallese has an interestingly complex historical phonology, which is why it was selected for expository purposes here – but the basics have been sketched in the above treatment.

The next form in table 5.5 is Pmc ***falikuri** 'ignore,' which shows up in Mrs as /**j$V^{a}l^{j}k^{w}V^{i}r^{\text{ɯ}}$**/. Given the analysis of pre-Marshallese stress placement developed above, we can revise our reconstruction to ***fàlikúri**.[16] To this form the velar rounding change (stated in (1)) would apply, giving up a form ***fàlik^{w}úri**. When we turn to consider the treatment of Pmc ***f**, an interesting problem arises. At first glance (see some of the relevant data in table 5.18) it would appear that ***f** develops into a glide in Marshallese.

Table 5.18: Pmc *f

Proto-Micronesian	*Marshallese*	*From table:*
***falikuri** '*ignore, turn away*'	**j$V^{a}l^{j}k^{w}V^{i}r^{\text{ɯ}}$** '*turn one's back on s.o.*'	5.5
***fiŋi[sS]aki** '*be twisted*'	**jV^{i}ŋ$t^{\text{ɯ}}V^{\text{ė}}$k** '*be twisted*'	5.5
***fici-ki** '*snip, snap, flip (tr.)*'	**j$V^{i}r^{j}V^{i}$k** '*shake (a hand), spear (tr.)*'	5.6
***manifi** '*thin*'	**$m^{j}V^{a}n^{j}V^{i}$j** '*thin, flimsy*'	5.8
***[fØ]ani[fØ]ani** '*to bail*'	**j$V^{a}n^{j}$j$V^{\text{ė}}n^{j}$** '*to bail*'	5.10
***fiŋifiŋi** '*to be twisted*'	**jV^{i}ŋjV^{i}ŋ** '*be kinky (of hair)*'	5.10
***faSofaSo** '*to plant; plant*'	**ɰ$V^{a}t^{\text{ɯ}}$ɰ$V^{a}t^{\text{ɯ}}$** '*plant, vine*'	5.12
***faroka** '*hold tightly*'	**ɰ$V^{a}r^{\text{ɯ}}V^{\text{ė}}$k** '*greedy, stingy*' (arōk)	new
***faSu** '*eyebrow*'	**j$V^{a}t^{j}$** '*eyebrow*' (āt)	new

[16] I will not discuss the question of whether the stress pattern we have posited on the basis of the Marshallese data holds for Proto-Micronesian itself. It is sufficient for our purposes that it holds of some relatively remote ancestor of Marshallese. But see Rehg (1993) for a treatment of Proto-Micronesian prosody which looks close to what we have posited.

There are, unfortunately, no good cases of PMC initial ***f** before round vowels in the data. Bender et al. (2003) cite PMC ***fou** 'feel cold' as having the Marshallese reflex which they give as /(**p**^{j}**V**^{i}-)**jV**^{a}**w**/.[17] There are two problems here – I cannot find any justification for the segmentation of the Marshallese form offered here, there being no element /**p**^{j}**V**^{i}-/ with the relevant semantics to my knowledge, and the vowel of the Marshallese form, /V^{a}/, is not the expected outcome of PMC ***o**. Given the irregularity of the vowel correspondence in particular, even if we were to accept the etymological connection, it is hard to work out the implications for a discussion of the treatment of initial ***f** before various following vowels – it isn't clear that the MRS form reflects *initial* ***f** (since it is preserved only in a compound, if that analysis is right) and we don't know the quality of the following vowel at the time the glide developed.

The complication regarding ***f** which we must consider now arises because Marshallese has no vowel-initial words. Proto-Micronesian words which began with a vowel all have undergone glide prothesis on the way to Marshallese.[18] The palatal glide (/**j**/) is inserted at the onset of words beginning with front vowels (PMC ***e** and ***i**), the round labiovelar glide (/**w**/) before word-initial round vowels (PMC ***o** and ***u**). Before PMC ***a** we find *either* Marshallese /**j**/ or Marshallese /**ɰ**/, under conditions which we will discuss below. This means that one possibility is that PMC ***f** was simply lost in Marshallese, and that the glides which we see in the above forms represent the normal treatment of onsetless words in Marshallese. That vowel-initial words which began with front vowels do indeed show /**j**/-prothesis, and those which began with round vowels get a prothetic /**w**/ can be seen from the data in table 5.19. Thus, the evidence of the development of word-initial ***f** before nonlow vowels, and that of no consonant at all in that position, appear to point in the same direction, and it seems entirely possible that ***f** was simply lost without a trace.

The evidence regarding the treatment of ***f** word-initially before ***a** and of word-internal ***f** is more complicated, however, as is the now relevant question of the

Table 5.19: Prothesis before word-initial nonlow vowels

Proto-Micronesian	*Marshallese*
***ena** '*that*'	**jV**^{e}**n**^{ɯ} '*that*' (eņ)
***iŋi** '*dorsal fin*'	**jV**^{i}**ŋ** '*spines on a fish*' (iñ)
***oro** '*be*'	**wV**^{e}**r**^{ɯ} '*be*' (or)
***upa** '*derris vine*'	**wV**^{é}**p**^{j} '*Barringtonia asiatica*' (wōp)

[17] I have modified their transliteration to make it consistent with the one used in this book.

[18] "Prothesis" is the name for a process which inserts a new segment in word-initial position.

treatment of *Ø (i.e., the lack of a consonantal syllable onset) in those positions. To determine whether ***f** was completely lost in all positions, we need to know the outcome both ***f** and *Ø in these positions as well. Let us start with the position word-initial before ***a**. For ***f**, the outcome is invariably Mrs **/j/** except in a single context – namely, when there is an ***o** in the following syllable.[19] When there is an ***o** in the following syllable, initial ***f** is reflected in Marshallese as **/ɰ/**, thus ***faroka** became Mrs **/ɰV^{a}r^{ɯ}V^{e}k/** and ***faSo** 'to plant' became Mrs **/ɰV^{a}t^{ɯ}/**. We can capture these developments with the sound changes in (13a–c).

(13a) ***f** > Ø / # _ $V_{[-lo]}$
- ***f** was deleted before a following nonlow vowel.

(13b) ***f** > **ɰ** / # _ a C o
- ***f** became Mrs **/ɰ/** in word initial position before ***a** if there was an ***o** in the following syllable.

(13c) ***f** > **j** / # _ $V_{[-rnd]}$ (after (13a) & (13b))
- Word-initial ***f**'s which remain after (13b) became **/j/** before nonround vowels.

It is possible that the rules in (13) could be further simplified, depending of course on what happens to the glide-prothesis process in the case of word-initial ***a**. If it matches the ***f** developments exactly, we can get by with just rule (13a), modified so as to delete initial *f before all vowels, plus whatever glide-prothesis rule we will need for the cases which do not involve ***f**.

The data involving word-initial ***a** is presented in table 5.20. I have divided the data up based on the quality of the vowel in the syllable after the initial one since we see from (13b) that this factor was relevant in the treatment of ***f**.

The clear generalization from the data in table 5.20 is that an initial ***a** before an ***a** of the following syllable gets a prothetic **/j/**, and that otherwise, initial ***a** gets a prothetic **/ɰ/**. This clearly differs from the treatment of initial ***f** before ***a**, which showed development to **/ɰ/** *only* if there was an ***o** in the following syllable. Thus we can piggyback the treatment of ***f** before nonlow vowels on the general glide-insertion process which we will need for vowel-initial words, but before the low vowel ***a**, we are going to need to explicitly change ***f** into the appropriate glide.[20] The processes given as (13b) and (13c) will do this. To

[19] It is entirely possible that the fact that ***u** of the following syllable is not relevant to this question is to be related to the general tendency for ***u** to be centralized in many Micronesian languages, including apparently some more immediate ancestor of Marshallese. Nevertheless, there are numerous quite secure forms with ***u** in their second syllable which show **/j/** for initial ***f** – the overall patterning of the data certainly indicated that this can be treated as the diachronically regular outcome.

[20] Of course, we could also delete ***f** in the context ***#_aCo**, since the general glide-insertion process evidenced in table 5.20 will trigger the insertion of the appropriate glide. Such a move seems counterintuitive.

Table 5.20: Prothesis before word-initial ***a**

Proto-Micronesian	*Marshallese*
*# a C a	
***aap^{w}a** '*no*'	**jV^{a}ɰV^{a}p^{ɯ}** '*no*' (eaab)
***ala** '*path*'	**jV^{a}l^{ɯ}** '*path*'
***aSa** '*name*'	**jV^{a}t^{ɯ}** '*name*' (āt)
*# a C e	
***ale** '*reckon, sing*'	**ɰV^{a}l^{j}** '*song*' (al)
***aSe** '*jaw, chin*'	**ɰV^{a}t^{ɯ}** '*jaw*' (at-)
***ate** '*liver*'	**ɰV^{a}t^{j}** '*liver, spleen*' (aj)
*# a C o	
***awo** '*fishline*'	**jV^{e}w** '*fishline*' (eo)
***alo** '*sun*'	**ɰV^{a}l^{ɯ}** '*sun*' (aḷ)
***aroŋo** '*pompano*'	**ɰV^{a}r^{ɯ}eŋ^{w}** '*African pompano*' (aroñ)
***aTo** '*thatch*'	**ɰV^{a}t^{j}** '*thatch*' (aj)
*# a C i	
***anitu** '*god, spirit*'	**ɰV^{a}n^{j}V^{i}t^{j}** '*god*' (anij)
***aŋi** '*wind*'	**ɰV^{a}ŋ** '*wind*' (añ)
***asiŋi** '*crab species*'	**ɰV^{a}t^{ɯ}V^{i}ŋ** '*brown land crab*' (atūñ)
*# a C u	
***au-** '*appearance, condition*'	**ɰV^{a}jV^{i}(-r^{j}V^{i}k)** '*of small appearance*' (āi(dik))
***ajuSa** '*current*'	**ɰV^{a}jV$^{\dot{e}}$t^{ɯ}** '*current*' (aet)
***aruti** '*stir (tr.)*'	**ɰV^{a}r^{w}V^{i}t^{j}** '*stir or poke (tr.)*' (aruj)

account for the treatment of initial ***a**, we will need the glide-insertion processes in (14ab). For the glide prothesis before the other vowels, we need the sound changes in (14cd).

(14a) $\varnothing > $ **ɰ** / # _ a C $\underset{[-lo]}{V}$

- Insert **ɰ** before word-initial ***a** if the vowel of the following syllable is not low.

(14b) $\varnothing > $ **j** / # _ a C $\underset{[+lo]}{V}$

- Insert **j** before word-initial ***a** if the vowel of the following syllable is low.

Table 5.21: A conditioned development of Pmc ***a**

Proto-Micronesian	*Marshallese*
***mawo** '*healed*'	**m^{j}V^{e}w** '*heal*' (mo)
***nawo** '*wave*'	**n^{ɯ}V^{e}w** '*wave*' (ņo)
***pawo** '*platform*'	**p^{j}V^{e}w** '*shelf, loft*' (po)

(14c) $\varnothing > \mathbf{j}$ / # _ $V_{[-bk]}$

- Insert **j** before word-initial nonback vowels.

(14d) $\varnothing > \mathbf{w}$ / # _ $V_{[+rnd]}$

- Insert **w** before word-initial round vowels.

When coupled with the processes in (13), we have a full account of the treatment of both initial ***f** and Pmc vowel-initial words.[21]

I have included in table 5.20 the obviously unexpected (given our current rule system) form ***awo** > /**jV^{e}w**/ 'fishline' because it involves a systematic process which we have not previously treated. Marshallese shows the effects of a diachronic process which raised ***a** to /V^{e}/ before ***wo**. Additional supporting data is given in table 5.21.

We may thus posit the rule in (15).

(15) ***a** > /V^{e}/ / _wo (before (9a) and (14))

Since this rule applies *before* the glide prothesis processes in (14), ***awo** will end up /**jV^{e}w**/ rather than /**ɰV^{a}w**/ – all initial ***e**'s, including those raised from ***a** by (15), get prothetic /**j**/.

Additional support for separating the glide outcomes of ***f** from those inserted before vowels whose syllables otherwise lack an onset consonant comes from the development of intervocalic ***f**, although here the data is quite limited, there being not many forms reconstructed with such a segment for Pmc. In the one very clear case, however, we see that once again ***f** develops into /**j**/, whereas the vowel hiatus shows a different treatment. That data is in table 5.22.

21 Note additionally that being able to distinguish between the Marshallese treatment of initial *f and word-initial vowels may help us to determine the proper reconstruction in otherwise indeterminate cases such as *[f∅]**ani**[f∅]**ani** of table 5.18. Since Marshallese shows an initial /j/ in its reflex of this word, and since, by (14a), ***aniani** would have shown prothetic /ɰ/, the Marshallese evidence clearly supports reconstructing the form with ***f**.

Table 5.22: PMC intervocalic *f vs. *Ø

Proto-Micronesian	*Marshallese*
***tafa** '*cut in pieces*'	**/t^{j}V^{a}j/** '*cut in pieces, chop*'
***Saa** '*what?*'	**/t$^{\text{ɯ}}$V^{a}ɰ/** '*what?*' (ta)

I will not formalize these developments, nor, in general, the quite complex process whereby Marshallese breaks up Proto-Micronesian vowel–vowel sequences via glide insertion. Instead, I will now survey some of the simpler developments we see in the data and with that survey we will conclude our initial consideration of the historical phonology of Marshallese.

The diachronic developments of the PMC segments ***l** and ***n** show a strong parallelism: each of the segments shows palatalized (**/l^{j}/**, **/n^{j}/**), velarized (**/l$^{\text{ɯ}}$/**, **/n$^{\text{ɯ}}$/**), and round velarized (**/l^{w}/**, **/n^{w}/**) developments. While to a certain extent this is as might be expected given what we've seen above, with velarized developments before PMC ***a**, round developments before PMC round vowels, and palatalized treatment before PMC front vowels, there are many exceptions and the entire matter seems best left to one side for a textbook such as ours. This is also generally true of PMC ***r**, which, however, only shows velarized and rounded outcomes, the MRS palatalized **/r^{j}/** arising from PMC ***c** instead.

A number of PMC segments, not unlike the case of ***c** discussed above, show an unconditioned change in their development to MRS. For example, the nonvelarized labials (***p** and ***m**) become palatalized (i.e., ***p** > **/p^{j}/** and ***m** > **/m^{j}/**) in all environments. We have seen this development in numerous examples cited above. PMC ***s** and ***S** fall together in Marshallese as **/t$^{\text{ɯ}}$/**, and PMC ***t** and ***T** merge as **/t^{j}/** (again, a cursory examination of the data cited above will reveal this).

In a few cases, it is somewhat difficult to know whether a change has occurred at all. For Proto-Micronesian (and Proto-Oceanic), one often reconstructs "round" labials – as indicated by the symbols I have used, following Bender et al. 2003 – including ***p^{w}** and ***m^{w}**. These show up, again without any particular conditioning environment, as MRS **/p$^{\text{ɯ}}$/** and **/m$^{\text{ɯ}}$/**, respectively. It is, of course, entirely possible that the Proto-Micronesian segments were themselves merely velarized, rather than rounded, in which case no change would have taken place on the way to Marshallese. Direct continuation without change, conditioned or otherwise, would also appear to be the case for PMC ***w** and ***j**,[22] which are regularly reflected in Marshallese as **/w/** and **/j/**, respectively.

[22] Usually written *y by Oceanic linguists.

5.2 Summary of Marshallese Developments from Proto-Micronesian

We can summarize the developments sketched in this section as table 5.23.[23]

Table 5.23: Major developments from PMC to MRS

PMC	MRS	PMC	MRS
***p**	**$/p^{j}/$**	***t**	**$/t^{j}/$**
***p^{w}**	**$/p^{ɯ}/$**	***T**	**$/t^{j}/$**
***f**	**/j/**	***s**	**$/t^{ɯ}/$**
***m**	**$/m^{j}/$**	***S**	**$/t^{ɯ}/$**
***m^{w}**	**$/m^{ɯ}/$**	***ʒ**	Ø
***k**	**$/k/,/k^{w}/^{a}$**	***c**	**$/r^{j}/$**
***x**	Ø	***r**	**$/r^{ɯ}/,/r^{w}/$**
***ŋ**	**$/ŋ/,/ŋ^{w}/^{a}$**	***l**	**$/l^{j}/,/l^{ɯ}/,/l^{w}/$**
***j**	**/j/**	***n**	**$/n^{j}/,/n^{ɯ}/,/n^{w}/$**
***w**	**/w/**	***ñ**	**$/n^{j}/$**
***i**	**$/V^{i}/,/V^{\acute{e}}/^{b}$**	***u**	**$/V^{i}/,/V^{\acute{e}}/^{b}$**
***e**	**$/V^{e}/,/V^{\acute{e}}/^{c}$**	***o**	**$/V^{e}/,/V^{\acute{e}}/^{c}$**
***a**	**$/Va/,/V^{\acute{e}}/^{d}$**		

[a] Before PMC round vowels.
[b] If stressed, with a nonhigh vowel in the following syllable.
[c] If stressed, with a high vowel in the following syllable.
[d] If stressed and between syllables containing high vowels.

5.3 Discussion Questions and Issues

A. Discuss the evidence arising from the forms cited in this chapter for the developments of Proto-Micronesian *l, *n, and *r into Marshallese (these

[23] Details concerning the complex development of PMC ***l** and ***n** into palatalized, velarized, and round variants, and of Pmc ***r** into velarized and round realizations, have not been dealt with above. Here I simply list the possible outcomes.

developments were not fully considered in the treatment above). If possible, develop a theory of the regular development of each of these segments, noting the relevant conditioning factors in that development. There will be problematic forms, which you should explicitly acknowledge and note.

B. Discuss all of the evidence that can be extracted from the forms cited in this chapter for the development of Proto-Micronesian vowel–vowel sequences (as in, e.g., *pep*ei*, *mat*oa*t*oa*, and the like), noting any regularities and any problematic forms.

6 In-Depth Consideration of Selected Issues

As promised at the outset of this part of the book, we have now completed a relatively traditional survey of the historical phonology of Marshallese, treating some problems in greater depth than others, as necessitated by the fact that this book is not a specialist work on Oceanic linguistics. We will now turn to a somewhat deeper consideration of issues which arise from the treatment we have just developed, bringing into the story additional data from other linguistic systems where appropriate. One of the central matters that we must confront in our consideration of phonological change is the relationship between phonetics and phonology. We will see that much of the twentieth- (and twenty-first-) century discourse on historical phonology has centered on this matter.

It is worth reminding the reader at this juncture that it is in the domain of phonology that historical linguistics is rightly proud of its accomplishments. While in the history of every linguistic system there remain interesting challenges to our understanding, the degree to which it is possible to present a coherent and compelling treatment of the historical phonology of a given linguistic system makes it virtually necessary that some fundamentally valid conception of how phonological change works must be embedded in our methods. As we have seen in some detail in chapter 1, this does not mean that historical linguists have always been able to accurately articulate how it is, or why it is, that their methods seem to do so well in this domain. We will attempt, in this chapter, to elucidate this matter.

6.1 Phonetics, Phonology, and Sound Change I: The Marshallese Velars

We began our discussion of the history of Marshallese phonology above with an investigation into the origins of the Mrs round velars (/**k**ʷ/ and /ŋʷ/), which exist side-by-side with a set of plain velars (/**k**/ and /ŋ/). Through an examination of some of the relevant data, we were able to establish the rule in (1) above, repeated here for ease of reference.

(1) **Velar rounding:** [+velar] > [+round] / _[+round]
- Velars were rounded before round segments.

On the one hand, it would seem that nothing could be more natural or sensible than such a development. As we noted when we first discussed it, the change appears to be assimilatory in nature, as are the vast majority of sound changes. Furthermore, although for other changes we may need to worry about positing a diachronic "path" for the development (e.g., it seems unlikely that PMC ***f** can have become MRS **/j/** without passing through some intermediate stage(s)), the rounding of the velars could trivially take place in a single step.

In spite of the seeming simplicity of the case, however, matters become somewhat more complex when we turn to a consideration of the details of the development. First, the change is indeed an assimilatory one – in fact, it appears to correspond to an assimilation that is very broadly attested in human languages. In my own English, **/k/** is pronounced with lip rounding before round vowels, i.e., the English word 'cool' is realized as **/kʷulᵚ/**. This appears to be generally the case in human languages, at least in those without contrastive roundness in velars. Since Proto-Micronesian was a language which did not have contrastive rounding in its velars, it would seem to follow that the velars of PMC which were before round vowels were, to a reasonably high degree of probability, already pronounced with lip rounding. The question thus arises as to whether such lip rounding on velars before round vowels is a low-level, coarticulation effect (below the level of the grammar) or a grammatical fact (either phonetic or phonological).

The contrast I have introduced here between coarticulatory (subgrammatical), phonetic (i.e., allophonic, grammatical), and phonemic status will be important in the discussion that follows, but is not always observed in either the phonological or the historical literature, and thus merits some comment. We need to be clear about these differences since, ultimately, our understanding of the mechanism of a variety of change types will depend crucially on maintaining clarity in this domain. We can, *a priori*, distinguish between three levels of analysis of linguistics-related phenomena. Separating the phonetic and the phonemic levels – a two-way split – has a long-standing tradition in twentieth-century linguistics; however, the term "phonetic" is often used in an ambiguous way. On the one hand, it is taken as referring to the output representations of phonological computation, such that the phonological rule component establishes a mapping relationship between two levels of linguistic representation. We can sketch this as in figure 6.1.

In figure 6.1, the underlying representation (**/kæt/**) is stated in some phonological alphabet, usually a set of bundled feature values. The string of IPA-like characters at the top of figure 6.1 is standardly taken as an abbreviation for just such a set of feature bundles. The phonological computation operates over this underlying representation and generates an output representation. The operations of the phonological computation are standardly assumed to involve the manipulation of the feature values (or the ordering or presence of specific features and

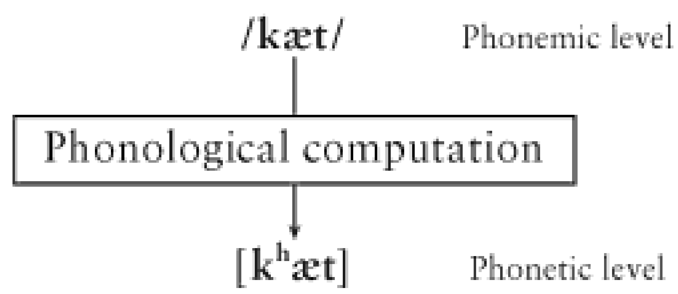


Figure 6.1: The phonemic and phonetic levels

their values) of the underlying representation to give an output representation in the same type of phonological alphabet – i.e., a set of bundled feature representations.[1] Thus the IPA-like characters at the bottom of figure 6.1 are also taken to represent a set of feature bundles. The feature for the first segment of the output representation differs from that of the first segment of the input representation in that the former contains a specification [+spread glottis] (or whatever the appropriate phonological feature for "aspiration" might be), whereas the latter does not. This specification has been provided by the application of a rule in the course of the phonological computation.[2]

Unfortunately, the term "phonetic" is also commonly used to refer to the actual acoustic, auditory, aerodynamic, and/or articulatory properties of an utterance. In such a case, the IPA-like characters used refer not to an abstract featural representation, but to the impressionistic acoustic or articulatory properties of an utterance. This is a very different thing than we intended by our use of "phonetic" above. For example, phonological contrasts in vowel length are possible, as are allophonic vowel lengthening, so both input representations and output representations in figure 6.1 can contain a representational specification for length. However, the representation capacity available to human languages appears to be quite limited in this domain, no human language offering us evidence which would lead us to conclude that human phonologies can have 10 or 20 or 2,000 different "lengths" for represented segments. By contrast, if one measures with sufficiently precise instruments the lengths of the medial segment in 10, 20, or even 2,000 actual articulations of "cat" by some speaker, one will discover that vowel lengths differ as a factual matter virtually across the board, each utterance of "cat" showing a (perhaps slightly, but nevertheless measurable) difference in vowel length. We thus clearly need to distinguish between the output representation of

[1] The representations may be, as is standardly assumed, richer than the concept "bundled" indicates. Suprasegmental information, syllable and foot structure, and the like, may also be modified or even added in the course of the computation. The relevant point is that it is possible for human languages to specify such information in underlying representations – even if in some particular language stress, for example, need not be – and thus the representational primitives for such information are "phonological" in the relevant sense.

[2] I will speak throughout in terms of "rules" and the like, rather than "constraints," without prejudice as to what the best theoretical model for phonology might ultimately turn out to be.

/kæt/ Phonemic level

Phonological computation

[k^hæt] Phonetic level

Transduction

k^hæt Output level

Figure 6.2: The phonemic, phonetic, and impressionistic output levels

phonological computation and impressionistic renderings of the actual articulation of forms. A simplified[3] version of this important observation – one which abstracts away from the complex process of getting from output representation to articulation by labeling the whole mess "transduction" – can be seen in figure 6.2.

I place the impressionistic rendering of the actual bodily output of an utterance within "human body" brackets so that it will always be clear just which type of representation is under discussion at any given moment.[4]

It is of some value to recall from our earlier discussion that, in part for learning-theoretic reasons, the "transduction" processes are generally assumed to be universal and invariant in nature. No "language specific" information need be encoded in them and they do not therefore represent "grammatical" knowledge. They reflect the fact that articulatory planning has a certain structure – that to get from, e.g., a target [**k**] articulation to a target [**i**] articulation certain "articulatory adjustments" (transition and coarticulation phenomena, e.g.) are made by the processes regulating the vocal tract. The "Transduction" box is also intended to cover the conversion of articulation events to acoustic waveforms, and the transformations induced by the auditory processing of such waveforms.

We can immediately see the utility of this three-way distinction by noting that a rounded velar may systematically emerge from our model in figure 6.2 in a number of ways, three particularly common ways being presented in figure 6.3.

In the "phonemic" column of figure 6.3, there is an underlying representation which includes, as one of its segments, a /**k^w**/. No rules in the phonological computation modify the features on this segment, which therefore emerges at

[3] "Simplified" because, as pointed out in chapter 4, there is no single mapping relationship involved in getting from the output representation of the grammar to an actual utterance. Many cognitive and physiological systems interact in complex and not well-understood ways to generate an utterance from a linguistic output representation.

[4] In the interest of gender equality, both male and female bodies will be used. Since opening and closing brackets should probably match for maximum legibility, I will use same-gender pairs to enclose these representations. This is not intended to have political consequences.

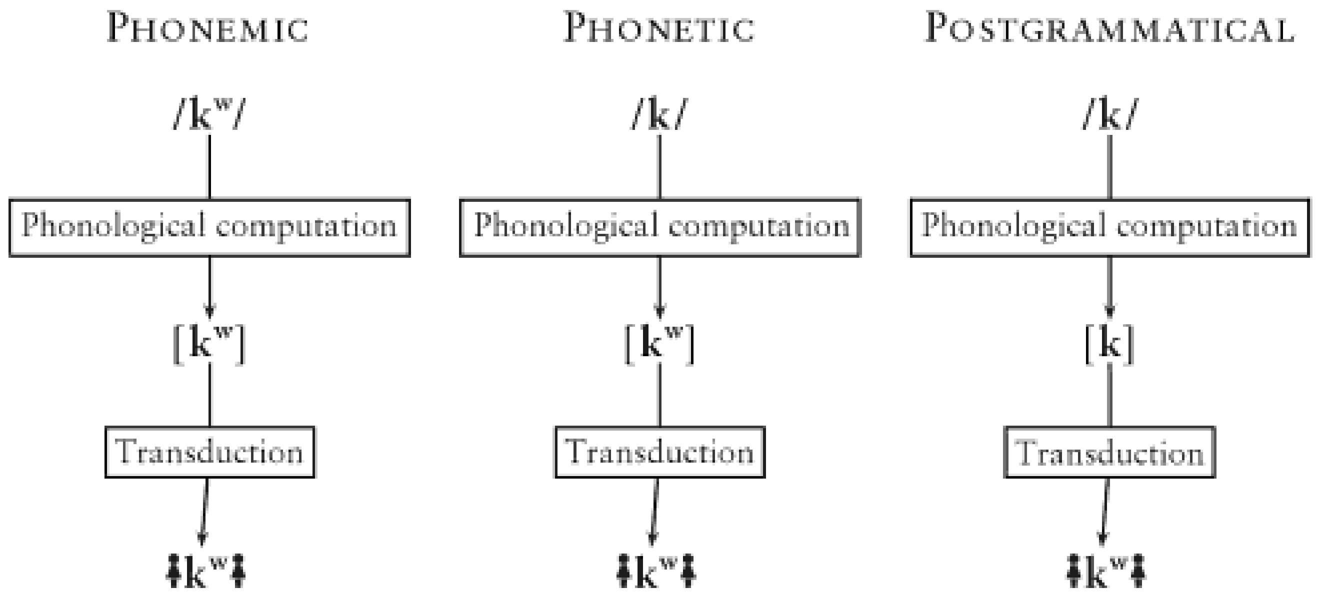


Figure 6.3: Phonemic, phonetic, and postgrammatical $\mathbf{k}^w$

the "phonetic" level as the very same feature bundle it is stored as. This [$\mathbf{k}^w$] is also unaffected by the transduction process(es) in this particular act of utterance and thus surfaces in the world as a ⁞$\mathbf{k}^w$⁞.

By contrast, in the center column of figure 6.3, we are dealing with an underlying /**k**/. A rule of the phonology of this particular language triggers a modification of the feature set of /**k**/ (under some conditions – we aren't concerned with the details at this juncture) such that, at the phonetic level, the representation of this segment now contains the feature [+round]. As in the previous case, this [$\mathbf{k}^w$] is unaffected by the transduction process(es) and thus emerges straightforwardly as ⁞$\mathbf{k}^w$⁞.

Finally, in the rightmost column, labeled "postgrammatical," we are dealing with an underlying representation without a roundness specification, i.e., with a phoneme /**k**/. Nothing in the course of the phonological computation introduces a feature [+round] into this representation, which consequently comes out at the phonetic level as [**k**]. In this instance, in the course of actually articulating the [**k**], lip rounding is introduced. We can easily envision a case such as this if we simply articulate the sequence [**uku**] without attempting explicitly to round our lips during the [**k**] closure. In my speech, and I would wager in the speech of all humans, such a [**k**] is articulated with lip rounding (i.e., as ⁞$\mathbf{k}^w$⁞), essentially inheriting that property from the articulatory roundness of the [**u**]'s on either side of it. This inheritance is not representational, i.e., not regulated by the grammar, but rather the byproduct of the fact that the string of segments is articulated as a single flowing gesture, the transitions between segments being handled by physical movement from target to target.

In the case of my own rounding of the /**k**/ in /**kul**/, it seems clear enough that we are dealing with an instance of "postgrammatical" rounding. There is, in any event, no evidence that the rounding is anything more than an anticipation of the lip rounding required for the following vowel. It seems likely that since /**k**/ itself need not specify anything about the state of the lips during its articulation (i.e., my **/k/** is neither contrastively [+round] nor contrastively [–round]), the phonetic representation leaves the lips free to adopt any roundness position, including the anticipatory one which we find in my articulation of words like 🧍**k**^{w}**ul**$^{\text{ɯ}}$🧍 'cool,' with a **/k/** realized with rounded lips, or 🧍**kil**$^{\text{ɯ}}$🧍 'keel,' with a **/k/** realized with spread lips. The process appears to be a normal one for the articulation of velars before round vowels.

If we return now to our consideration of the seemingly straightforward Marshallese development we started with – the rounding of Pmc ***k** and ***ŋ** before round vowels – we can see that things are somewhat more interesting than they may have first appeared. Proto-Micronesian did not have contrastively round velars and thus, presumably, if it was like living languages which lack that property, its velars were realized with lip rounding when they appeared before round vowels by virtue of postgrammatical articulatory transitions. Of course, since we have given the *phonemes* of Proto-Micronesian in citing forms from that language, we use a symbol which reflects the fact that at that level of representation there is no lip rounding. Since the Marshallese forms are also cited in their phonemic form, and since in the case of a word like **/k**^{w}**ép**$^{\text{ɯ}}$**/** 'bent' we see contrastive roundness on the initial velar (in clear distinction from the Proto-Micronesian form of this word, **/kup**^{w}**e/**), the actual history of this development would appear to be something like that sketched in figure 6.4. It seems likely that, as a diachronic matter, the **/k**^{w}**/** of Marshallese owes something to the putative presence, in Proto-Micronesian, of 🧍**k**^{w}🧍.

We find here rather striking confirmation of the point which was made at considerable length in chapter 1 of this book. Regarding the initial segment of the word for 'bent,' both Proto-Micronesian and Marshallese speakers produced a rounded, voiceless velar stop. If diachronic linguistics were to be about the history of the behavior of speakers, there would be no change in the velars in these words. It is only when we consider relationships at a more abstract level (the phonemic or, in this case, also the phonetic) that we detect that something of linguistic significance has taken place. This has the clear implication that historical linguists also must adopt an I-language conception of their object of study, as I argue in detail in this book.

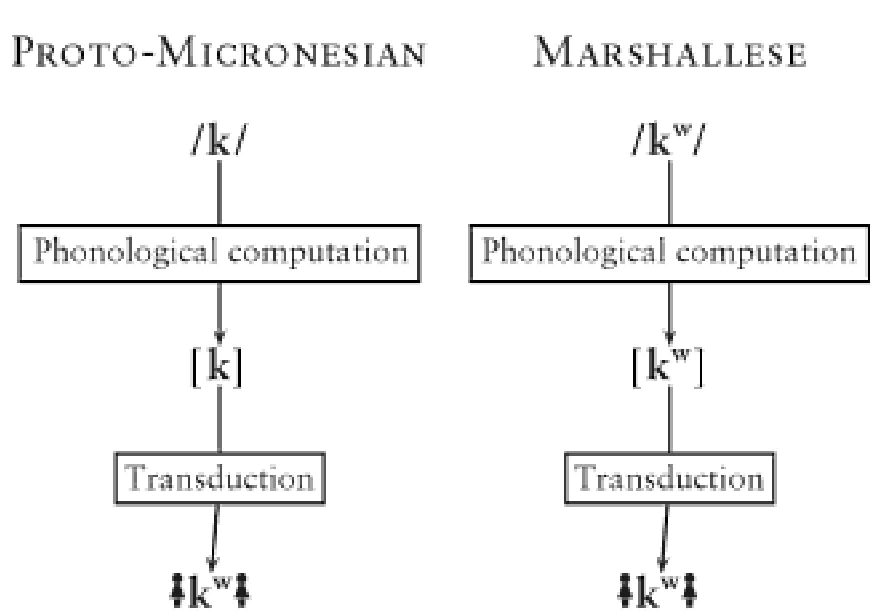


Figure 6.4: Velar rounding revisited

Interestingly, however, there is another important observation we can make regarding this case. While focusing on the "grammatical" (i.e., phonemic and/or phonetic) levels rather than on speaker output enables us to see that a change has in fact occurred regarding the Marshallese velars, if we were to restrict our attention to those levels we would be eliminating our access to data which is actually crucial to the ultimate explanation of the events involved, namely, the fact that in PMC initial /***k**/ can be assumed with a high degree of confidence to have been realized with lip rounding even though the grammar itself played no direct role in giving rise to that rounding.

6.2 A Digression on the History of Research

Varying attention to these levels and the relationships between them characterizes in many ways the history of research into diachronic phonology. For example, the Neogrammarians talked about reconstructing the "sounds" of Proto-Indo-European, and about "sound change" on the way to the daughter languages, whereas structuralists generally reconstruct the "phonemes" of Proto-Indo-European, and document the changes between these phonemes and the phonemes of the various Indo-European daughter languages. Finally, the generativist tradition in diachronic phonology tended to focus strongly on the history of rules and rule systems. It is of more than passing interest to examine just what each of these major schools dedicated their energies to, and what reasons they gave for making their particular object of investigation the focus of diachronic phonological research.

The Neogrammarians did not have access to the structuralist concept of the phoneme, nor to the generative concept of the "phonological rule," and so it is hardly a surprise to learn that they did not frame their work on historical phonology in terms of such concepts. Although they use the label "sound" for the object of their investigations, it does seem clear than in general what they had in mind was something close to the "phonetic" level of figure 6.2. Thus, for example, the Neogrammarians reconstructed for Proto-Indo-European both an ***ŋ** and a ***z**, the former an allophone of ***n** before velars, the latter an allophone of ***s** before voiced obstruents.[5] They did not, however, reconstruct the purely physiological nasalization of vowels before nasals, which surely was reflected in the speech of Proto-Indo-Europeans as it is in the speech of contemporary humans, and thus they do not appear to have been attempting to capture things at the purely physical or mechanical level which I have called "impressionistic output" above.

The historical phonologies for individual languages provided by Neogrammarians posit sound changes between these reconstructed "phonetic" elements

[5] Amongst many other "subphonemic" segments, including syllabic liquids and nasals and the like. Of course, the "allophone" and "subphonemic" labels are anachronistic in this context.

(allophones, if you will) and the (abstract, representational) "phonetic" elements of the Indo-European daughter languages. For example, Brugmann (1904), provides a brief account of the development of ***ŋ** in the various daughter languages, citing forms such as PIE ***péŋkʷe** 'five' > Latin **/kʷiːŋkʷe/** (*quīnque*), Armenian **/hiŋ/**, and Lithuanian **/peŋkì/**. Brugmann notes, of course, that Proto-Indo-European ***ŋ** is only found before the velars (rounded and unrounded) of that language – i.e., he was aware of the limited distribution of ***ŋ**, and yet one can see from the data just cited what the advantages of "subphonemic" reconstruction like this are: it seems a little silly to reconstruct PIE ***n** in the word for 'five' and then change that ***n** to **ŋ** in Armenian, for example, when, in point of fact, we do not believe that the pronunciation of the segment has changed at all. There was a change, to be sure – a change in the phonological status of the segment **ŋ** – but *that* change is not in any meaningful sense the reason why Armenian has the segment **ŋ** in the word for 'five.'

On the other hand, it does appear that reconstructing "phonetically," as the Neogrammarians did, also misses some important aspects of the story. For example, other daughters of Proto-Indo-European ***peŋkʷe** include Sanskrit **pə́ɲcə** (usually transcribed **páñca**) and Greek **pέntε** (*πέντε*). In a very real sense, the palatal nasal of Sanskrit and the **n** of Greek are exactly the same as the proto-segment: they are articulated in the same place as the consonant which immediately follows them. Since the place of articulation of the PIE labiovelar has shifted in this context in Sanskrit to the palatal place and in Greek to the dental, the nasal has shifted as well. Brugmann's method makes this, too, look like a change, when in some sense the nasal of the protolanguage has not changed at all on its way to the daughters (it was the "invariably assimilating" nasal of PIE, as it is in Sanskrit and Greek as well).

As we noted above, Brugmann and his Neogrammarian colleagues did not have the option of reconstructing phonemes for Proto-Indo-European, since the concept of the phoneme did not yet exist when they were working on these problems. However, beginning with Jakobson's "Prinzipien der historischen Phonologie" of 1931, the structuralist concept of the "phoneme" came to play a key role in diachronic work, a status which it has maintained largely unchallenged to this day. The basic idea was simple enough: the linguistic system is a phonemic one, the contrastive elements of a language are its phonemes, and thus for a change to be of linguistic significance, it must affect in some way the phonemic status of the elements involved. A modern survey of diachronic methodology which explains the origin of contemporary historical linguists' focus on the phoneme states the matter succinctly (Fox 1995: 38):

> consider what is meant by "sound change." In purely phonetic terms this can only mean that a particular articulatory gesture is replaced by another, whether in all cases where it occurs in the pronunciation of the words of the language, or just under certain conditions. From the point of view of structuralist phonology, however, this change must be evaluated differently according to whether or not it

> results in a change in the phonemic status of the sounds involved. If it merely affects how the allophones of a phoneme are articulated, then the change is, phonologically speaking, superficial, and indeed irrelevant; only if it affects the system of phonemes itself, or the grouping of sounds into phonemes, can it be considered a genuinely *linguistic* change.

This in turn has direct repercussions for what the object of phonological reconstruction should be, as Fox again notes (1995: 42):

> What are the implications of this for the Comparative Method and for the reconstruction of protolanguages? First, it means that since phonemes, rather than sounds, are regarded as the basic units of pronunciation, our methods of reconstruction must be geared towards determining the phonemes of the protolanguage rather than its sounds. In fact, determination of the latter is regarded as rather unimportant . . .

In keeping with this doctrine, modern reconstructions of the Indo-European phonological inventory do not include subphonemic segments such as Brugmann's *ŋ and *z.

It is worth noting that in discussions of phonemic vs. phonetic approaches to sound change and reconstruction, no distinction was made, in keeping with the general practice up to the present, between what I have called above the "phonetic" level of representation and the level of actual acoustic or articulatory output.[6] In addition, perhaps somewhat more surprisingly, the statements which regulated the relationship between the phonemic and the phonetic level – which would correspond to the "rules" of later generative phonology – were not felt to be of interest. The focus was on the phonemes themselves and their paradigmatic (i.e., contrastive) and syntagmatic (i.e., phonotactic) relationships to other phonemes. This is consistent with the antimentalism (or, in the case of European structuralists, somewhat more neutrally, the nonmentalism) of the structuralists: the accounts of the relationships between phonemes and their allophones were constructs of the theoretician, rather than properties of the knowledge state of the speakers, and thus did not require direct diachronic explanation.

It should not go without comment that the arguments offered up against paying attention to the phonetic (or even acoustic/articulatory) levels offered up by the structuralists are of a singularly odd type. Fox has captured them accurately

[6] I mean here *explicitly* made. In practice, the physical level – in spite of the near universal use of terminology such as that we see in the quotes from Fox – was never up for serious discussion in either synchronic or diachronic phonology. No one gave students phonology problems with a column of data from a speaker *without* a spoonful of peanut butter in his/her mouth, and another column of data from that same speaker *with* a spoonful of peanut butter in his/her mouth, and asked the student to write the rules which would get one from one stage to the other. That is, *de facto*, the exclusion of the purely physical level has always been true of actual linguistic practice, in spite of the surrounding rhetoric.

Table 6.1: A Romance labial segment (Campbell 1999: 119)

Italian	*Spanish*	*Portuguese*	*French*	*(Latin)*	*English gloss*
/kapra/	**/kabra/**	**/kabra/**	**/ʃɛvr(ə)/**	capra	goat
/kapo/	**/kabo/**	**/kabu/**	**/ʃɛf/**	caput	head, top

in his sympathetic summary in the passages quoted above. The phonetic level is "superficial," "irrelevant," and "unimportant." "Irrelevant" and "unimportant" of course make sense only with respect to some actual purpose of the enterprise, but we will argue in considerable detail below that such details are certainly *not* irrelevant or unimportant for developing a comprehensive understanding of the historical phonology of a given language, nor, indeed, for our goal of constructing a constrained theory of phonological change.

The conflicting desire to use phonemic information when treating sound change and reconstruction and to have access to as much relevant data as possible leads to rather strange methods being advocated in typical introductory textbooks on historical linguistics. For example, consider the phonemic data regarding a Romance labial segment in table 6.1, extracted from Campbell (1999: 111).[7]

In discussing what to reconstruct, and thus of course what sound changes to posit, for the sound correspondence between the labials in this data, Campbell (1999: 119) says:

> The reflexes in all four languages share the feature "labial"; the Spanish, Portuguese and Italian reflexes share the feature "stop" (phonemically). Factoring all the features together, we would expect the protosound to have been a "labial stop" of some sort.

The key here is Campbell's parenthetical "phonemically" in the quote above. As is well known, not least of all to Campbell himself, the phonetic forms of the relevant Spanish words in very many dialects are [**kaβra**] and [**kaβo**], respectively, with a fricative (like French, though differing slightly in place) rather than a stop (like Italian and Portuguese). There is no inconsistency in Campbell's treatment of these forms – he has been using phonemic representations throughout his discussion – but there is an odd disconnect between this practice and the beginning of the paragraph from which his statement immediately above is taken:

> We attempt to reconstruct the protosound with as much phonetic precision as possible; that is, we want our reconstruction to be as close as possible to the actual phonetic form of the sound as it was pronounced when the protolanguage was spoken.

[7] The Latin is not given phonemically, for whatever reason. In addition, the parentheses around the French schwa are not explained by Campbell.

It seems a foregone conclusion that, were one to actually reconstruct on the basis of the *phonemes* of the daughter languages, such a move is bound to reduce, rather than maximize, the phonetic detail available for use in establishing the phonetic segments of the protolanguage, and thus for our ability to develop a systematic theory of sound change based on developments between such protolanguages and their daughters. We will consider this matter in more detail in chapter 10, when we turn to reconstruction methodology.

The generative enterprise, in sharp contrast to both the Neogrammarians and the Structuralists, turned its attention to the diachronic development of the "phonological computation" system in figure 6.2. The approach was characterized by an assumption that phonemic representations showed, in general, remarkable diachronic stability (and thus were not the locus of most observed changes);[8] and, of course, phonetic output forms are simply the epiphenomenal result of the application of the set of ordered phonological rules to these stable underlying forms. Thus, while changes were often observed at the phonetic level, the primary mechanism of getting a different form at that level was to modify the phonological component. The set of possible modifications here is quite limited: a given rule could cease to be a part of the phonological system ("rule loss"), a new rule could be added to the phonological system ("rule addition"), the rules already available could change their order of application ("rule reordering"), or, finally, the precise targets, triggers, or environments for a given rule could be modified.

The general framework of generative diachronic explanation has already been discussed briefly at the end of chapter 1, when Grace's summary of Morris Halle's position was cited. We are now in a position to consider the matter more fully, which we will do using the example of "rule addition." Imagine that we observe some new output forms in a given linguistic tradition. Since underlying forms are assumed to be generally stable, the explanation for these new output forms will be to posit a rule added to the grammar of the speaker with the new forms. The form of the rule itself will be determined by the relationship between the old forms and the new, i.e., the rule which gets added will be one which maps the old output forms to the new output forms. We can sketch this relationship as in figure 6.5.

In this figure we have, at Stage I, a synchronic phonological rule which rounds velar stops before round vowels. Thus for an underlying phonological representation of the shape **/kiku/**, we will get an output of the phonological computation, i.e., a phonetic representation, of the form **[kik^{w}u]**. If we were to observe for

[8] See, for example, Chomsky and Halle (1968: 49): "underlying representations are fairly resistant to historical change, which tends, by and large, to involve late phonetic rules. If this is true, then the same system of representation for underlying forms will be found over long stretches of space and time." A similar orientation is found in much diachronic Optimality Theory work, which tends strongly to equate phonological change with constraint reranking – ignoring the ever-present possibility of the restructuring of underlying representations.

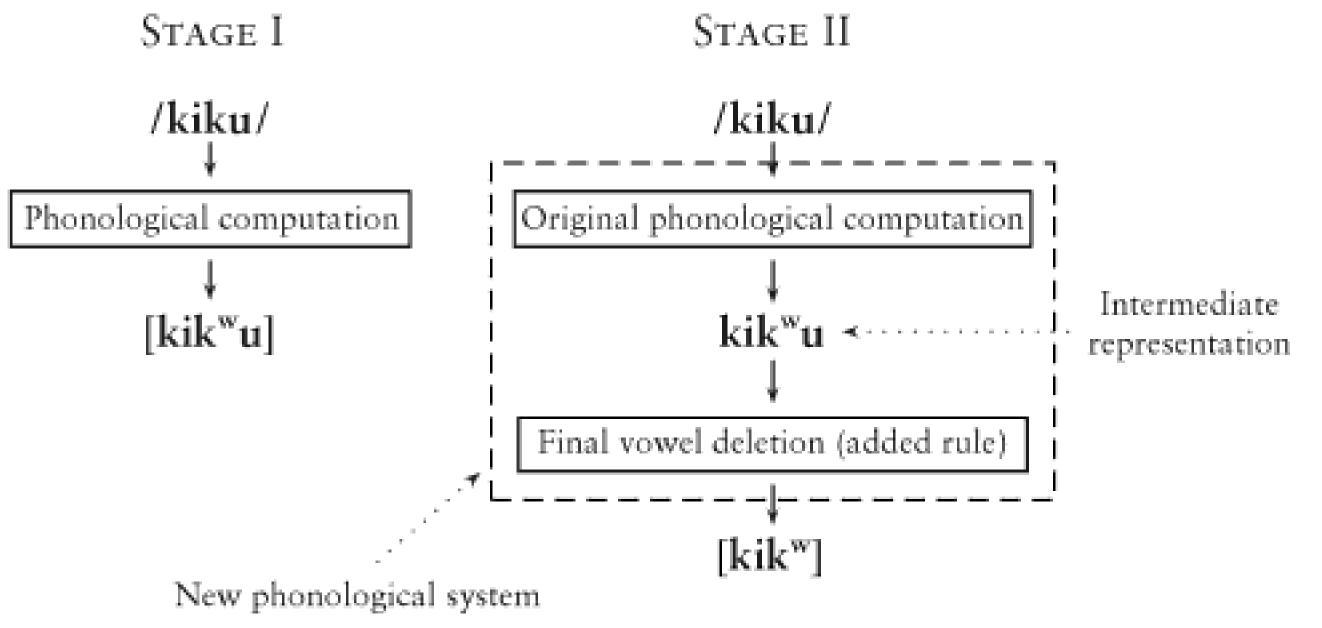


Figure 6.5: Rule addition

some subsequent speaker within this linguistic tradition that they no longer produce final vowels, we would posit a rule which maps the output form of the former speakers ([**kik^{w}u**]) to the newly observed output form which will lack a final vowel ([**kik^w**]). Since the rule is constructed by taking outputs at Stage I and mapping them onto outputs of Stage II, it is obvious that it will be added at the end of the Stage I grammar (since it generates the Stage II form from the Stage I form directly, and thus there could be no relevant rules which would apply after it). Clearly, in such a model, the "change" involved is the difference between the grammar at Stage I and the grammar at Stage II, a difference quite sensibly captured by the label "rule addition."

We can come to understand further aspects of the generative conception of phonological[9] change if we add an additional assumption to the situation sketched in figure 6.5. Imagine that the language did not have any suffixes, and that, therefore, final vowel deletion did not give rise to any alternations.[10] The grammar under Stage II in figure 6.5 will have the property that the round vowel of the underlying form /**kiku**/ will *never* surface in any word of the language. There is a fairly serious issue here: no child could ever construct such a grammar – there is simply no evidence available to a learner which would cause them to posit underlying final vowels when no such vowels ever surface. How, then, could a Stage II grammar, which is completely unlearnable by humans, ever exist?

[9] And syntactic, but that must await later chapters.

[10] If there are suffixes in a language at a time when it undergoes final vowel deletion, one generally finds data patterns such as ***kiku > kik^w** next to a suffixed version of that morpheme, let's say ***kiku-na** which would give **kik^{w}u-n**. In the suffixed form the final vowel of the original morpheme is thus preserved, and since the quality and properties of that vowel will not be predictable, the underlying form for that morpheme will usually be set up, even after the application of final vowel deletion, as /**kik^{w}u**/ – this is one of the reasons generativists spoke often of the stability of underlying forms.

The generative solution to the question is simple: the grammar was not learned. What was learned during the initial language acquisition period[11] was the Stage I grammar, the Stage II grammar resulted from an *adult* adding a rule to his/her existing grammar. The precise mechanism whereby this happened is obscure and for the most part not discussed in the generative sound-change literature.[12] The Stage II grammars are, then, the "suboptimal" grammars referred to in the discussion of Morris Halle's ideas at the end of chapter 3. An adult could speak with output generated by such a grammar to his/her child, who, constrained by the language acquisition device (LAD), will construct in his/her own mind an *optimal* system for generating **kik^w** – this optimal system will not include a final **u** in the underlying representation of this word – a segment for which the child does not receive any evidence. This grammar will thus have no use for a rule of final vowel deletion (since there will be no final vowels in underlying representations to delete). "Rule loss" will thus result, straightforwardly.

The mechanism which the LAD uses to prefer the child's constructed, optimal, grammar over the suboptimal grammar of Stage II is an evaluation metric whose responsibility it is to assign relative value to competing hypothesized grammatical systems such that the acquirer can converge upon the simplest, and thus optimal, system for generating his/her output representations. Since the child acquirer will always optimize in this way, the adult "Stage II" grammar will be a fleeting entity (it cannot be learned by the linguistic descendants of the speaker who has a Stage II grammar). The children of this speaker will also, of course, show the effects of "final vowel deletion" – i.e., the grammar they construct will also generate /**kik^w**/ for our made-up word, whereas their linguistic ancestors (two generations back) will have said /**kik^{w}u**/. The fleeting "rule addition" stage (our Stage II) is thus of very little general interest to the generative diachronic phonologist, for whom the effects of the optimization process undertaken by the child acquirer exposed to Stage II is much more linguistically significant

[11] It is useful to bear in mind that the generative enterprise strongly supported the Critical Age Hypothesis, which held that language acquisition was only possible up to a certain age, beyond which the so-called "language acquisition device" in humans ceased to function.

[12] Morris Halle (pers. comm.) has, on several occasions, told me, somewhat jokingly, that the process was due to "human perversity." While I would not want to minimize the significance of the role of human perversity in history, I would most definitely not like to be somehow responsible for developing a theory of human perversity, as enjoyable as the research behind such a theory might be. Other claims (though perhaps this is what Morris has in mind) appear to relate the change mechanism to "fashion" and changes in favorite automobile styles, and other deep and abiding mysteries. None of these claimed mechanisms for change would appear to offer much hope in our developing an understanding of why some changes – e.g., those which would appear to arise via acoustic misparsing – are found with such regularity, or why changes such as **p** > **l** are unattested. Surely humans are perverse and/or fashion-conscious enough to make whatever changes might occur to them, if perversion and/or fashionability were truly the mechanism at work.

(since the results of that process will be a learnable and potentially broadly transmitted and well-attested linguistic system – i.e., it is not fleeting). Since the LAD optimizes in the direction of simplicity only – it could never posit a *more complex* system than Stage II to generate the same data – all linguistically significant sound change must, under these assumptions, be simplification.

The claim that change is simplification is thus grounded in several core assumptions of the generative approach to sound change. Oddly, much of the literature which touches upon the issue of "change as simplification," even literature by the generativists themselves, confuses the claim that the *grammar* constructed by the child exposed to the output of a Stage II speaker will be simpler with the quite different claim that the observed output will in some way be simpler. There will be no observable difference between the output of the Stage II speaker and that of the child acquirer who constructs an optimal grammar to generate the output observed in learning from a Stage II speaker. The difference is purely in the computational system and lexicon.

The principle difficulty that this approach faces is in coming to terms with the actual mechanism of change: where the added rules come from, what, if anything, determines or limits their possible properties, and how it is that adult speakers are able to modify their existing grammatical system in this manner. With no answer to such questions, this particular approach provides little in the way of explanation for actually observed phenomena. We will have occasion in the discussion which follows in this chapter (and in subsequent chapters) to explore the shortcomings of much of the work actually carried out under these assumptions.

It is certainly worth pointing out, however, how readily this model of change can be converted into something which I most definitely believe can be and has been responsible for change events in linguistic history – one without the explanatory weaknesses of the traditional generative approach. We can reconceptualize the contents of figure 6.5 as in figure 6.6.

At first blush, this figure doesn't look very different from figure 6.5 – not surprisingly, since the differences are essentially conceptual. Instead of having an adult add a rule of unknown origins to the end of his/her grammar, which would require that we grant adults that kind of access to their generally encapsulated cognitive systems, it would require that we devise some mechanism by which the speaker might come up with the rule and add it as part of a "postgrammatical" filter on his/her linguistic output. We discussed such "postprocessors" at the end of chapter 3; the fact that humans can construct such things seems beyond question. Where, in such a story, does the added rule come from? Well, it comes from the speaker's impression that it would be prestigious, in the specific example outlined above, to produce output forms without final vowels. Why might a speaker think such a thing? Aren't we stuck in an uninspiring infinite regression (the speaker who adds the final vowel deletion postprocessor must already have been talking to some particularly cool person who has deleted final vowels – but where did *that person's grammar* come from?)? Well, not necessarily, because the speaker can be *wrong* about what a prestigious person is doing, linguistically, in

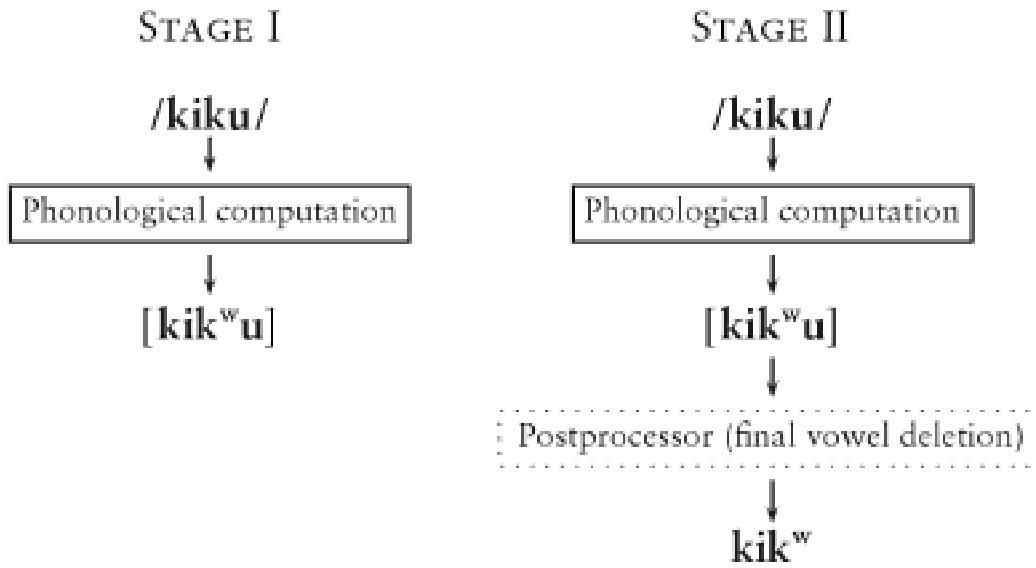


Figure 6.6: A new approach to "rule" addition

the speaker's environment. In being wrong, a postprocessor which is designed to match the output of that prestigious individual (but which will not, because the individual who added the postprocessor has incorrectly analysed what his source is actually doing) will be constructed which fails to do such matching, and thus introduces novel material into the linguistic context.

Such cases are well-known in the linguistic literature, going by the name "hypercorrection." To take a well-known nonphonological example, the stigmatization of the use of "who" in questions such as "who did Bill see?" in many varieties of North American English, and the corresponding prestige of "whom" use, has led many speakers to add a postprocessor which converts "who" to "whom" (see Lasnik & Sobin 2000 for an extensive discussion of this case). A variety of issues arise – differing of course from speaker to speaker, since different postprocessors can be constructed for this phenomenon, and various speakers can use these postprocessors more or less frequently and thus more or less adeptly – such that the postprocessor status of this conversion is quite clear. For example, speakers' behavior is highly variable in this domain – not surprisingly, given the degradation of performance on postprocessor (as opposed to grammatical) processing under distracting conditions such as everyday life. The precise conditions on conversion also tend to differ quite a bit, many speakers having a hypercorrecting postprocessor which substitutes "whom" for "who" in nominative case demanding contexts.

Thus, stripped from awkward conceptual difficulties such as the mysterious origin of added rules and the unexpected direct modification of the grammatical system by an adult speaker, we can find in the standard generative account of phonological change a reasonable model for a very restricted diachronic phenomenon: hypercorrection. That most sound change is not triggered by hypercorrection-type processes seems clear (there would be no prestige triggers for most observed changes), and thus the generative model, even so "corrected," cannot provide us with the answers we are seeking in this book.

We can see that each of these approaches – the Neogrammarian, the Structuralist, and the Generativist – has difficulties if we simply consider the matter from

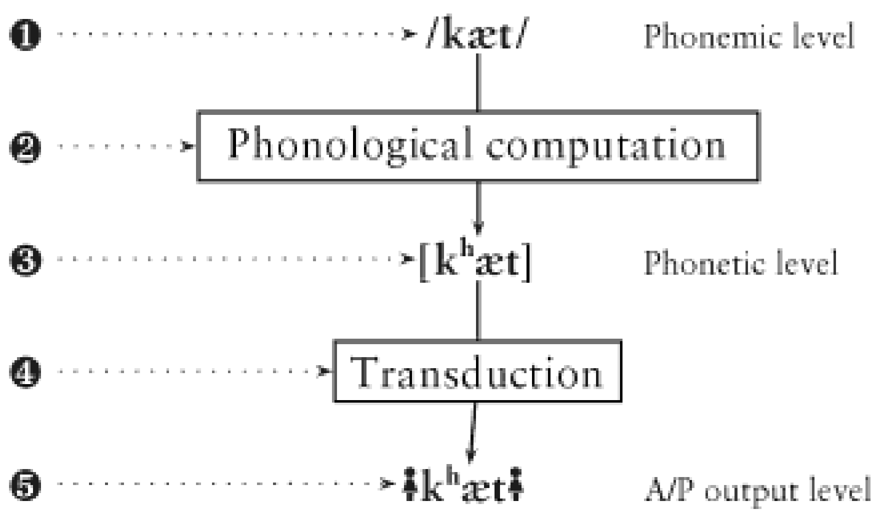


Figure 6.7: The phonemic, phonetic, and articulatory/perceptual levels

the acquisitional perspective we developed in chapter 1. We can try to find our way out of these difficulties by looking one more time at our sketch of the relationships between the levels under discussion (repeated, with the addition of numbering, from figure 6.2, in figure 6.7). We can then ask what role the various elements of that figure play in the process of grammar construction. Once we understand that, we will be able to narrow our concerns to just the relevant aspects of that figure, at least for those changes which have their origin in acquisition. The figure contains five elements: (❶) the underlying, phonemic representation, (❷) the computational component of the phonology, which is responsible for converting the phonemic representation to a phonetic one, (❸) the phonetic representation which results from running the phonemic representations through the computational system, (❹) the highly diverse set of cognitive and physical conversion routines (transducers) which take the computed phonetic representation and convert it into some type of articulatory/perceptual object, and (❺) the articulatory/perceptual object which results from the transduction process.

Let us begin with ❶. The underlying phonemic representation must be constructed by the acquirer in the course of the acquisition process. It represents a core element of the acquirer's knowledge state. It may differ from person to person and is clearly subject to modification in the course of transmission, and thus must form part of any theory of phonological change. The Structuralists were thus correct to make it part of the object of study of diachronic phonology, and Neogrammarian theory was hampered by its inability (since the notion did not yet exist) to treat this level of representation explicitly in the analysis of change events.

Turning next to ❷, the computational system of the phonology, we again recognize that this system must also be constructed by the acquirer in the course of the language acquisition process, and that it, too, represents a core element of the acquirer's resultant knowledge state. It differs across individuals and across time and thus forms part of any complete theory of phonological change. The Generativists were thus correct to introduce the rule system into the discussion of diachrony in phonology, and the Structuralists (and of course the

Neogrammarians as well) were hampered in their work by their failure to recognize that this element of the linguistic systems under study must also be accounted for by theories of change.

By contrast with the preceding cases, ❸, the phonetic output representation, does not represent an aspect of the acquired knowledge of the speaker. Once the speaker has posited a phonemic representation and a system for phonological computation, it would be redundant to internalize the fully predictable phonetic representations. Nor does this representation serve as the basis for any learning – indeed, it cannot do so, since the acquirer has no access to it! Its status as a derivable intermediate representation (intermediate between the stored phonemic form and an actually pronounced output) to which the learner has no access means that no independent account of the history of such forms is required of the historical phonologist. Once she or he has posited a set of underlying forms and a set of phonological rules, the historical phonologist will have completed all necessary work on the grammar itself.[13] It was thus a mistake, arising because of the lack of an articulated theory as to the nature of synchronic phonology, with its underlying representations and rule systems, that the Neogrammarians considered phonological history to hold of this level directly.[14]

The systems of transduction, ❹ in figure 6.7, are assumed to be universal in nature – it does not appear for example that individual learners can influence the way acoustic waves interact in the physical world (once they've been set in motion), or learn to manipulate their short-term memory systems in a language-specific manner, or the like. Since this component of the figure is invariant across humans, it cannot change, and thus, while it is important for the historical linguist to understand the range of articulatory/perceptual outputs licensed by the transduction systems for a given phonetic output form (as we will see momentarily), the transduction systems themselves lie outside the scope of diachronic linguistic investigation.

The forms which the transduction systems give rise to, for a particular phonetic representation, represented by ❺ in figure 6.7, are every bit as epiphenomenal as the phonetic output forms. Given a sufficiently rich theory of the transduction systems, a given set of phonological rules and an underlying phonemic representation, the range of outputs of the transduction process could be fully specified.

13 One might well counter that, given the phonetic output representations and the underlying phonemic representations, the rules could also be deduced, and are thus just as redundant as I have argued the phonetic forms to be. The problem with this position is clear, however: if one stores phonetic output representations and does not store phonological rules, there is absolutely no need to store phonemic underlying representations, and no need to have a phonology at all. Since there is ample evidence that humans have phonological systems, I will not further consider the possibility that the rules themselves are epiphenomenal.

14 As we saw in chapter 4, this does *not* entail that it was necessarily a mistake to do reconstruction in the way the Neogrammarians did.

However, unlike the phonetic representation, the articulatory/perceptual form of a given string *does* serve as core data for the acquirer attempting to construct a grammar and lexicon. It is this status as primary input to acquisition, and thus as the likely locus for much of the "noise in the channel" which makes change inevitable, that elevates these representations to the status of key components to the successful construction of a constrained theory of phonological change.

However, while the material in ❺ is of relevance to the historical linguist's task in a way in which the invariant transduction systems themselves (❹) are not, because such outputs are derivable from a combination of the other elements which the acquirer must learn (the underlying phonemic representation and the rule system), together with the effects of the invariant transduction systems, these articulatory/perceptual output forms cannot change *on their own*, with the rule system and underlying representation held constant. That is, it would never be necessary, or appropriate, for the historical linguist to claim that a given articulatory/perceptual output *changed* into another articulatory/perceptual output. What can change is limited to the phonemic representation (❶) and the system of phonological computation (❸). Crucially, because all evidence for the acquirer comes from the articulatory/perceptual output representation in ❺, any change at these grammatical levels will need to find an explanation at the articulatory/perceptual level.

So let us return for the last time to the question of the coming into being in Marshallese of rounded velars. We have seen that, in terms of the articulatory behavior of speakers of Pmc, it is unlikely that the particular velars which show up as round in Marshallese, all of which once stood before Pmc round vowels, were anything other than round at the Proto-Micronesian stage. How then are we to account for their change in phonemic status, from mere physiological roundings of phonemic and phonetic velar stops not specified as either round or nonround to phonemically round velar stops? Not surprisingly, since the articulation of the segments themselves has probably not changed substantially from Proto-Micronesian times, the account for this change lies in shifts in the nature of the phonological context within which the velars were located. To fully understand these, we must turn to a relatively comprehensive account of the history of Marshallese vowels.

6.3 Phonetics, Phonology, and Sound Change II: The Marshallese Vowels

In our earlier discussion of Marshallese historical phonology, we essentially accepted the view of contemporary historical linguistics, and indeed of historical phonologists since the 1930s, that we should be concerning ourselves predominantly with the *phonemes* of a language when doing diachronic work. In the

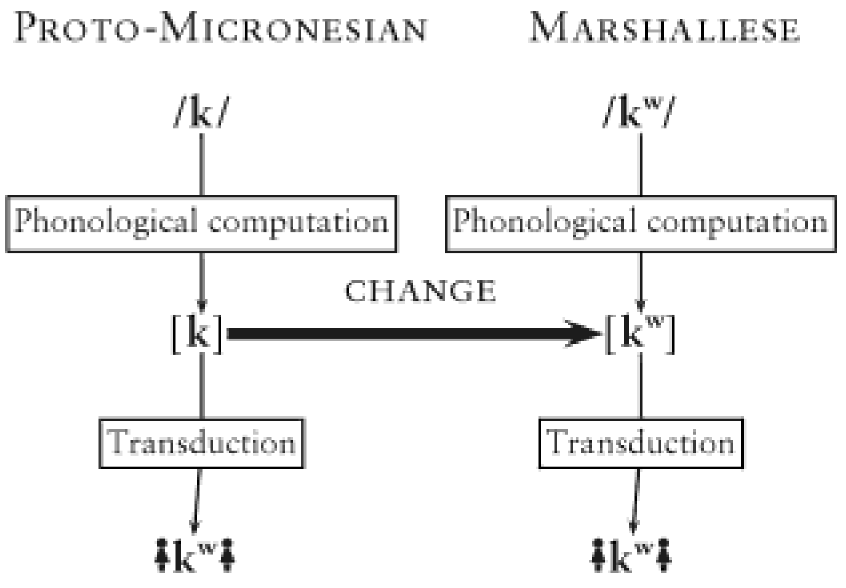


Figure 6.8: *k>kʷ: the Neogrammarian version

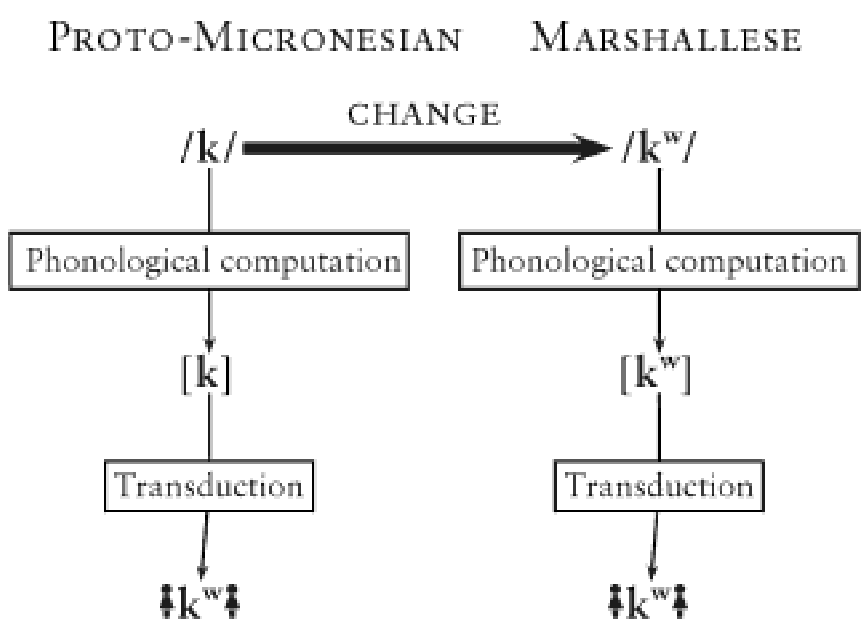


Figure 6.9: *k>kʷ: the Structuralist version

section just completed, I hope to have established that the material which should serve as the focus of our attention includes, but is not limited to, the phonemic representation. We must also consider the phonological rule system and the articulatory/perceptual properties of utterances. That is, the optimal approach to the question of the history of the Marshallese velars – and any other issue in diachronic phonology – is not the Neogrammarian one, which we could represent as in figure 6.8, nor the structuralist one in figure 6.9.

Nor, finally, is it the generative one, which I won't even attempt to graphically represent here, though a look at figure 6.5 should help the interested reader to draw his or her own version of a figure such as those above. Instead, the explanation for sound change is to be sought in an examination of the relationship between the input which the acquirer received during the acquisition process – all input relevant to the acquirer's task of constructing a set of underlying representations and a phonological computation system, not just the individual segment which has "changed" – and the underlying representations and phonological systems that acquirer constructed.

This said, we are finally in a position to treat the issue of why it is that, while by our account above the *pronunciation* of Mrs /**kʷ**/ and /**ŋʷ**/ has remained constant since Pmc times, we nevertheless see "change" in their diachronic development. As we have argued above, there is a difference at the phonemic level between Pmc and Mrs: Proto-Micronesian had proto-velars which were underspecified along the "roundness" dimension, whereas Marshallese has contrastive specification along that dimension for all of its velars. But, as the previous discussion has hopefully made clear, it is not possible for one underlying representation to simply change into another with everything else being held constant: there must have been some difference in the data set the acquirer who first constructed phonemic /**kʷ**/ was exposed to which licensed this divergent learning outcome. How do we go about uncovering what this difference might have been?

A useful approach, it seems to me, is to examine the antecedent system (its underlying representations, its phonological rule system, and its articulatory/perceptual output forms)[15] with a view toward understanding what kind of properties in the evidence which could in principle be generated by such a system might lead to the observed change.[16] In the present instance, and in many like it, two basic possibilities seem to suggest themselves.

The first possibility has been outlined in generative terms in a paper by Larry Hyman (1976). He dubs this process "phonologization" in that paper, and we can treat a concrete example which he mentioned as potentially relevant: the nasalization of vowels before nasals. It is generally assumed that in English there is a significant degree of nasalization on vowels before nasals, i.e., that speakers produce something like 🧍**sɪ̃n**🧍 for phonemic /**sɪn**/. Nasalization in this environment has been found for humans across the board, i.e., regardless of native language; however, the nasalization found in English is thought to be even greater than that which arises for humans generally. Let's assume this to be correct. It follows that some part of the English speaker's nasalization in 🧍**sɪ̃n**🧍 is to be directly

[15] I have come to anticipate that many readers balk at this point at the suggestion that one could "examine" the properties of the articulatory/perceptual output of a reconstructed language such as Proto-Micronesian. Obviously, I am talking about *hypothetical* articulatory/perceptual output, just as we must in any such case talk about *hypothetical* phonemic representations of forms in the protolanguage, and *hypothetical* processes in the protolanguage's phonological computation system. The matter is not significantly different at the other end of the temporal path being dealt with here: we are dealing with the development of Pmc forms into what is our current *hypothesis* regarding the nature of Marshallese phonemic representations, phonological computations, and articulatory/perceptual output forms. The nature of our evidence for any of these many hypothetical entities may be, in any particular case, better or worse, and we may wish to moderate the strength of our claims based on our confidence in our hypothesis, as always in science.

[16] Bearing in mind that it may be necessary to posit a string of change events – one is not typically examining the relationship between grammars separated by only a single generation of transmission.

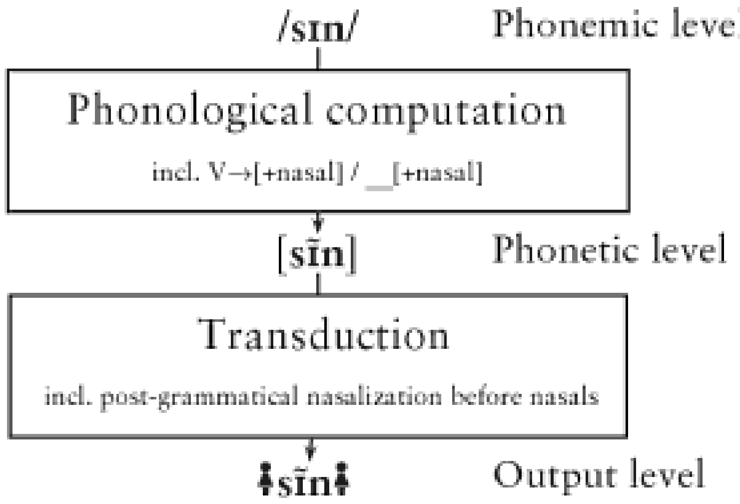


Figure 6.10: Phonetic vowel nasalization before nasals in English

attributed to their particular grammar, while some of that nasalization is post-grammatical, i.e., language-independent. We can sketch this using our familiar diagram as in figure 6.10.

In this figure the initial nasalization of the vowel of **/sɪn/** is *phonological*, and gives rise to a phonetic representation **[sĩn]**, while the second nasalization is postgrammatical, and is responsible for causing the phonetic representation to be realized as **🧍sĩn🧍**, which should have an even greater degree of nasalization on the **ĩ**.[17] If we imagine that the phonological nasalization was an innovation at some point in the history of English, then there was a time when that nasalization was not present, and the derivation of a pronunciation of **/sɪn/** would have looked as in (16):

(16) **Stage I English**: **/sɪn/** → **[sɪn]** → **🧍sĩn🧍**[18]

In (16) we see that, English speakers always having been humans, the universal aspect of vowel nasalization before nasals was of course present at this earlier stage of the language. However, it did not operate upon an *already nasalized* vowel, and thus the articulatory/perceptual output form at this time was not *as nasalized* as it is in many varieties of English today.

It does not seem particularly difficult to develop an account as to where the phonological nasalization in contemporary English might come from. Following Hyman, we may with a reasonable degree of confidence assume that this nasalization represents the *phonologization* of the postgrammatical nasalization seen in English at Stage I. That is, an acquirer has misparsed nasalization triggered by universal properties of human articulatory implementation as being due rather to an explicit command to nasalize – acquirers attribute such "explicit commands" to the phonetic representation (which is, after all, what articulatory schema normally implement during speech). Thus a low-level, universal aspect of speech planning and/or production has been elevated to the status of a phonological rule.

[17] Once again the IPA fails us.

[18] Where the nasalization on the articulatory/perceptual output in this case is *less* than that in figure 6.10.

Up to this point, this scenario will not give us the Marshallese situation with respect to rounded velars, since they are *underlyingly* round, whereas in the case of vowel nasalization before nasals in English, the modification remains phonetic, rather than phonemic. To get it to be phonemic, we need to leverage the fact that the universal human nasalization of vowels before nasals serves as a major cue to the presence of those nasals as segments. Once the nasalization comes to be inserted by rule, as it has in English, there is *more* nasalization on the vowel than could be attributed to the effects of a following nasal, and the nasalization thus ceases to be a good cue to the presence of that nasal (since it must be taken, in any event, as inherent on the vowel itself). It is always possible that in this situation, the nasals will fail to be accurately or consistently perceived by an acquirer, leading to the loss of the nasals and the phonemicization of the nasal:oral contrast on vowels. This, indeed, is what is assumed to have happened in the history of French, for example. The process whereby the triggering segment is lost and the contrast originally induced by that trigger comes to be represented underlyingly, rather than being derived, Hyman calls "phonemicization."

In the case of velar rounding in Marshallese, the "phonologization" of the rounding would work as in (17).

(17) **Stage I: /ku/ → [ku] → 🚹k^{w}u🚹**
Stage II: /ku/ → [k^{w}u] → 🚹k^{w}u🚹

At Stage I we have a purely implementational rounding, while at Stage II the round velar is an allophone, created by grammatical computation, of the underlying velar phoneme. To get to the "phonemicization" stage, we would need for something to happen to the conditioning factor – in the case, to the rounding of the following vowel. And of course we already know that something *does* happen to the rounding of the vowel – it disappears, at least phonemically, from the Marshallese reflex of ***u**. It would thus appear that the rounding of velars in Marshallese may represent an excellent case of the type of development discussed by Hyman. Exploring just how this is so allows us to deal with many of the issues we have been discussing, especially the relationship between phonetics, phonology, and the acquisition process. To understand these matters, we must consider in some detail the *phonetics* of the Marshallese vowels, as well as their realization at the articulatory/perceptual level.

In articulatory/perceptual terms, the Marshallese vowel system is perhaps even more strikingly odd than it is phonologically. The "surface" vowels are given in (18), which displays the outcome of each of the Marshallese vowel heights in every possible interconsonantal position.[19] The first row represents the realizations

[19] Because every Marshallese word begins with a consonant, and every Marshallese word ends with a consonant, and there are no vowel–vowel sequences in the language, every vowel in Marshallese is interconsonantal.

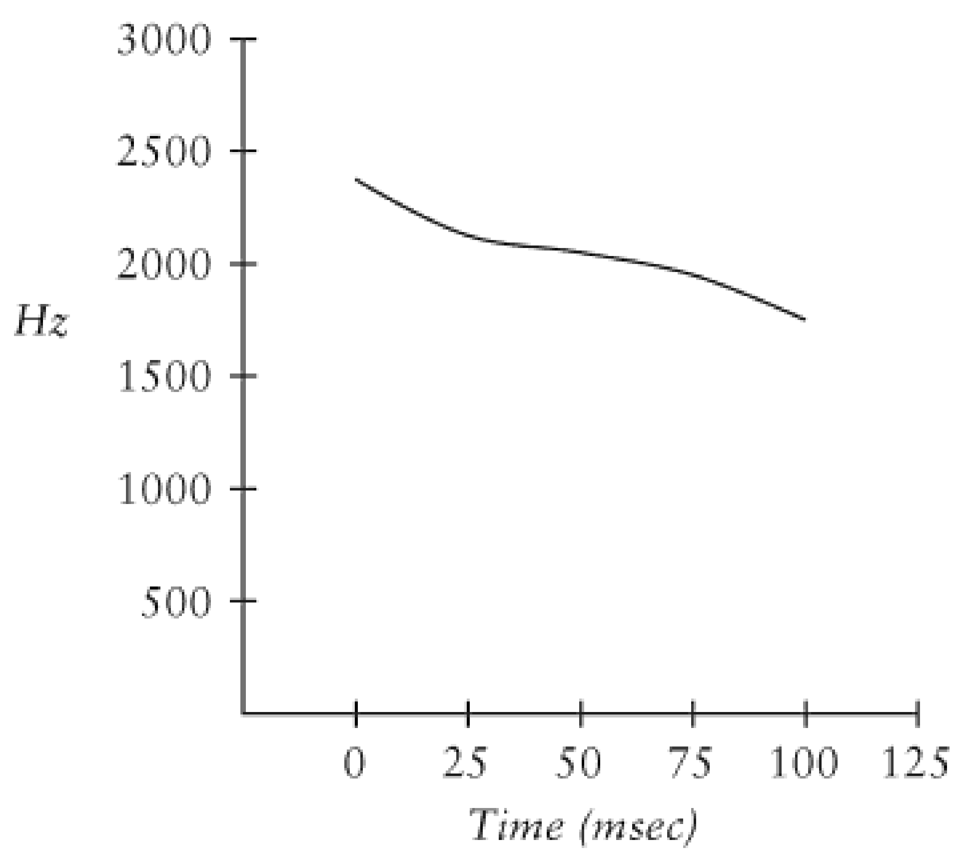
of the high vowel, which we have been writing with the symbol /V^{i}/; the second row represents the upper-mid vowel, which we have been writing /$V^{é}$/; the third row shows the realizations of the mid vowel, which we have been writing /V^{e}/; and, finally, the final row depicts the outcomes of the phonemic low vowel, which we have been writing /V^{a}/. I have allowed $\mathbf{t}^{j}$ to represent the class of "light," or "palatalized," consonants, $\mathbf{t}^{ɯ}$ to represent "heavy," or "velarized," consonants, and $\mathbf{k}^{w}$ to represent round consonants. In this table, the "tie" symbol, as in **i͜u**, represents a smooth transition from one vowel to another, e.g. here, *i* to *u*, articulated over the temporal span of a single vowel.

(18) **The realization of Mrs vowels**

t^{j}_t^{j}	$\mathbf{t}^{ɯ}$_$\mathbf{t}^{ɯ}$	$\mathbf{k}^{w}$_$\mathbf{k}^{w}$	$\mathbf{t}^{j}$_$\mathbf{t}^{ɯ}$	$\mathbf{t}^{j}$_$\mathbf{k}^{w}$	$\mathbf{t}^{ɯ}$_$\mathbf{t}^{j}$	$\mathbf{t}^{ɯ}$_$\mathbf{k}^{w}$	$\mathbf{k}^{w}$_$\mathbf{t}^{j}$	$\mathbf{k}^{w}$_$\mathbf{t}^{ɯ}$
i	ɯ	u	i͜ɯ	i͜u	ɯ͜i	ɯ͜u	u͜i	u͜ɯ
ɪ	ɤ	ʊ	ɪ͜ɤ	ɪ͜ʊ	ɤ͜ɪ	ɤ͜ʊ	ʊ͜ɪ	ʊ͜ɤ
e	ʌ	o	e͜ʌ	e͜o	ʌ͜e	ʌ͜o	o͜e	o͜ʌ
æ	a	ɒ	æ͜a	æ͜ɒ	a͜æ	a͜ɒ	ɒ͜æ	ɒ͜a

An example may make this clearer. Choi (1992: 68) presents a graph, sketched in (19) below, of an F2 trajectory for the Marshallese word /$\mathbf{t}^{j}$V^{e}$\mathbf{p}^{ɯ}$/ 'to return.' Since $\mathbf{t}^{j}$ is a "light" consonant, and $\mathbf{p}^{ɯ}$ is a "heavy" consonant, the lower-mid vowel /**e**/ should be realized as in the fourth column of the table in (18), that is, as [$\mathbf{t}^{j}$**e͜ʌ**$\mathbf{p}^{ɯ}$]. F2 trajectory reflects movement of the tongue along the front–back dimension, with high F2 correlating with frontness, low F2 with backness.

(19) **F2 (i.e., backness) tracking of the vowel of Mrs /$\mathbf{t}^{j}$V^{e}$\mathbf{p}^{ɯ}$/** (Choi 1992: 68)



The graph in (19) shows quite clearly that there is no steady-state position for the tongue along the backness dimension during the realization of this vowel. The tongue moves steadily from a front position at time 0 to a back position at 100 msecs. As Choi (1992) demonstrates, this lack of steady-state position holds for all of the "tied" vowels above. As we noted earlier, Bender (1968) used his impressionistic perception of these facts to argue that the most coherent phonological analysis of the Marshallese vowel inventory is one in which the vowels themselves bear no features along the dimensions front–back and round–unround. That is, they differ from one another *only* along the height (or, in my view, height and ATR) dimensions.[20]

Bender's analysis anticipates significantly not only Choi's (1992) acoustic analysis of Marshallese vowels, but also Pat Keating's important work on phonetic underspecification (Keating 1988). Keating argues in detail, with respect to Russian /x/, that it is necessary to distinguish between phonological underspecification – the failure to specify an underlying representation, i.e., a particular phoneme, for some feature – and phonetic underspecification, the latter being the failure of a particular *output* phonetic representation to be specified for some feature value. The idea is clear enough, conceptually. Imagine that the feature specifications are transduced into articulatory instructions. We can imagine, then, three possible behaviors for a segment which finds itself between two other segments specified with the opposite value for some feature. This is represented in figure 6.11.

In the leftmost representation in figure 6.11, the middle segment is specified positively for the feature [back], just like the segment to its left is. The segment to its right, by contrast, is specified negatively for that feature. The articulatory/perceptual transition from the positive valued correlate to the negatively valued one must be fit into the temporal domain between the second and the third segments, and thus shows a relatively steep slope. In the rightmost representation in figure 6.11, the middle segment is specified negatively for the feature [back], as is the

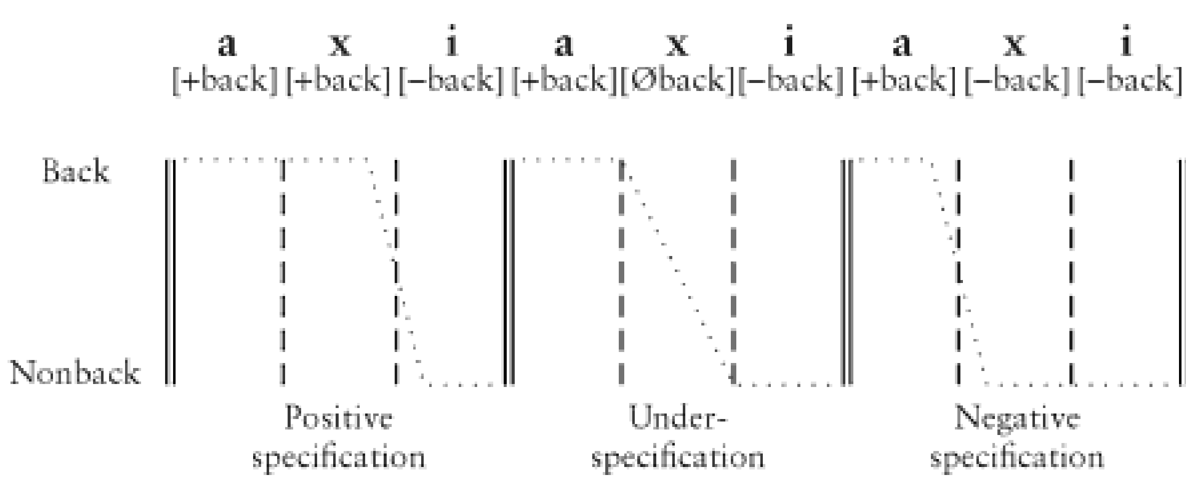


Figure 6.11: Phonetic specification and phonetic underspecification

20 Bender proposes an analysis in which one of the height contrasts might be eliminable, given sufficiently abstract underlying representations. The particular type of abstractness involved is no longer widely practiced in phonological circles.

segment to its right, while the segment to its left bears a positive specification for that feature. Now the transition must be fit into the space between the first and the second segment, and must again be relatively rapid (i.e., steep).

It is the central representation in figure 6.11 which will be our focus here. Keating showed that there were instances when a segment made it all the way to the phonetic level *without* receiving (or without underlying possession of) a value specification for some particular feature. In such a case, the transduction process simply progresses steadily from the first specified value to the next, moving with smooth transition through the underspecified segment. In the central representation above, the medial segment bears no specification for the feature [back], which we have chosen to represent as [Øback]. The segment to its left is *positively* specified for [back], and the segment to its right is *negatively* specified for [back]. The gestural score which the transduction process gives rise to in order to implement this phonetic feature set simply gives no instructions as to backness for the duration of the [x], allowing the tongue placement in the back dimension during that temporal window to be a simple function of the specifications on either side.

The implications of this for our analysis of Marshallese vowels is straightforward. We have already noted in our treatment of Marshallese above that Marshallese vowels are phonemically underspecified on the [back] and [round] dimensions. The phonetic realization facts introduced in (18) and (19) make it clear that the best analysis of the Marshallese vowels is that they remain underspecified along these dimensions at the phonetic level as well. Given the example of Marshallese **/t^{j}V^{e}p^{ɯ}/** discussed above, which is relatively simple because we need concern ourselves only with backness (rather than with both backness and roundness, which would demand a 3-dimensional figure), we can represent the effects of Marshallese phonetic underspecification as in figure 6.12.

The sloping line in the [V^{e}] portion of figure 6.12 is a schematic representation of the actual declination of F2 measured by Choi and presented in (19). Represented in the "three-tier" model – phonemic, phonetic, and articulatory/perceptual – which we have been developing in this chapter, **/t^{j}V^{e}p^{ɯ}/** would look like figure 6.13.

Let us now represent a case of velar rounding using what we know about the synchronic phonetic and articulatory/perceptual properties of PMC and of the Marshallese vowels. PMC ***tokona** 'his walking stick' develops into MRS

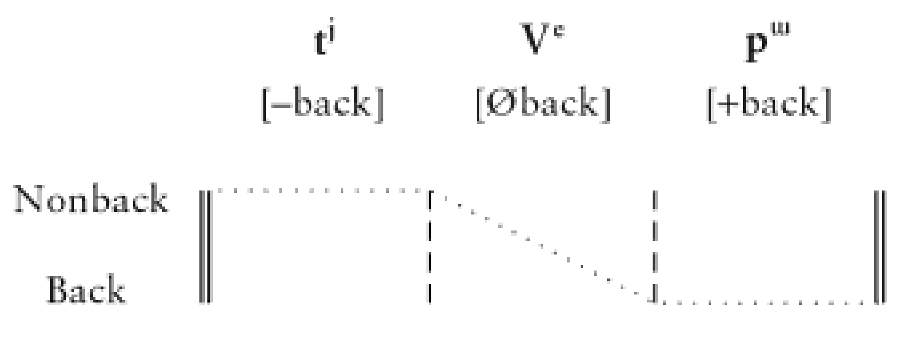


Figure 6.12: Phonetic underspecification and realization

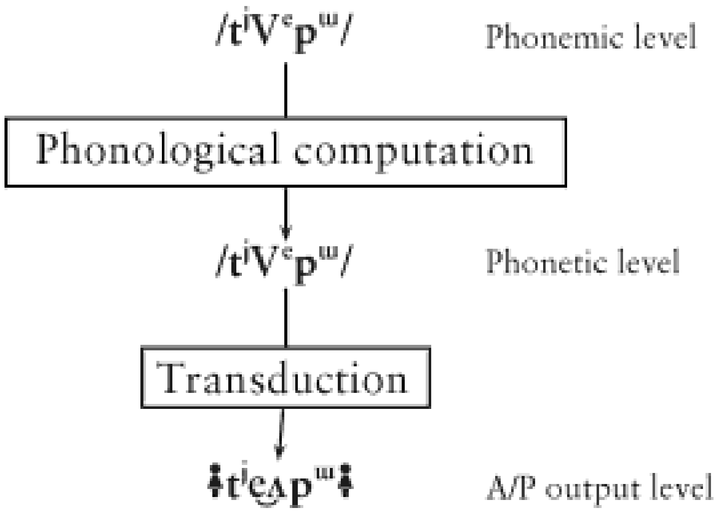


Figure 6.13: The three-tier model

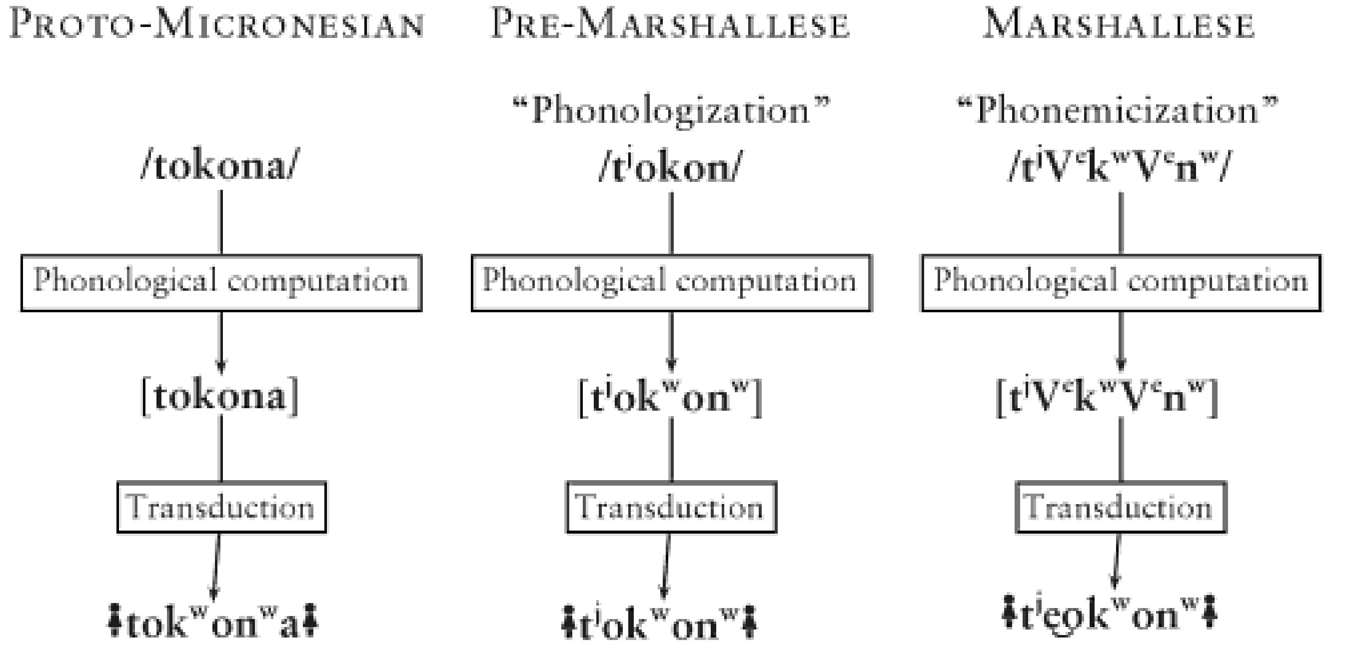


Figure 6.14: Velar rounding revisited

/$t^jV^ek^wV^en^w$/.[21] We will focus initially on the development of the second syllable of this word, which involves the velar rounding process. Figure 6.14 presents a sketch of what we know about this development now.

In this figure we see that MRS shows its regular palatalized reflex of PMC ***t** and, as expected, has lost the final (posttonic, unstressed) vowel of the PMC word. In addition, the rounding, which was found in the articulatory/perceptual output of the PMC form because of the "low-level" rounding of the relevant segments in the neighborhood of round vowels in that language, has been, at the first, pre-Marshallese, stage, "phonologized" and at the second, Marshallese stage, "phonemicized."

[21] We have not dealt in detail above with the rounding and velarization of PMC ***n** in Marshallese, and it is not necessary to treat it in detail here, either. There is considerable evidence from Micronesian languages that PMC final ***a** assimilated to preceding round vowels (thus becoming PMC ***o**) – a development which surely may be implicated in the Marshallese /**n^w**/ in this word. Given this distinct possibility, I will reconstruct the PMC form here with a surface **⫶n^w⫶**, which would probably really only be appropriate if we posited the above-mentioned vowel assimilation.

If we worry for the moment only about the developments directly relevant to the history of Marshallese velar rounding, we would posit at the "phonologization" stage a new rule in the phonology of pre-Marshallese – our Velar Rounding Rule. This will supply, phonologically, an underlying velar not specified for any value along the [round] dimension with the feature [+round] if there is a following round vowel. Since in this case there is a round vowel following the Pmc ***k** of this word, the phonology will apply this new rule to the underlying form for 'his walking stick' and a phonetic representation with a round velar will result. Since nothing happens to affect this roundness in the course of transduction, the form would be expected to be pronounced under normal circumstances with a medial round velar. The origin of the rule is to be sought in the misanalysis of what was a low-level, postgrammatical rounding of the velar in the Pmc word as being due instead to a [+round] feature at the phonetic representation level, in a manner directly parallel to the discussion of vowel nasalization above.

At the "phonemicization" stage in figure 6.14 the roundness found in pre-Marshallese at the phonetic level, introduced by a rule in the phonological component, is now present in the phonemic underlying representation. It is safe to assume that the Velar Rounding Rule, introduced at the pre-Marshallese stage, has now been eliminated from the grammar, since the rounding it brought about is already present at the phonemic level at this stage. Why and how was the rule lost? What triggered the shifting of velar rounding from the phonetic to the phonemic level? In the case of vowel nasalization which we discussed above, it was loss of the triggering segment (the postvocalic nasal) which led, in French for example, to the phonemicization of nasality on vowels. But if we examine how the second syllable of Pmc ***tokona** was pronounced at the pre-Marshallese stage, and is pronounced in Marshallese, it is hard to see how we could claim that the roundness which triggered velar rounding has been lost: the segment was pronounced as an **[o]** in Pmc, it was pronounced as an **[o]** in pre-Marshallese, and it is pronounced as an **[o]** in contemporary Marshallese. But if the roundness on vowels which was responsible for low-level round realization of underlying velars at the Pmc stage is still present in contemporary Marshallese, what could have triggered the change in phonemic status?

Interestingly, due to the nature of the realization process for underspecified vowels, any Pmc vowel could, depending on its consonantal environment, come to be realized in Mrs just as it was in Pmc, even though it differs from its Pmc correlate at both the phonemic and the phonetic level. The second syllable of ***tokona** reveals this for ***o**. I cite examples below for the remaining Pmc vowels.

(20) **'Preservation' of vowel quality at the articulatory/perceptual output level**
Pmc ***wuwu** 'wicker fishtrap' > Mrs **/wV^{i}w/** (realized as **[wuw]**)
Pmc ***lima** 'five' > Mrs **/l^{j}V^{i}m^{j}V^{a}-/** (realized as **[l^{j}im^{j}ɛ-]**)
Pmc ***meña** 'thing' > Mrs **/m^{j}V^{e}n^{j}/** (realized as **[m^{j}en^{j}]**)
Pmc ***m^{w}arem^{w}are** 'necklace' > Mrs **/m^{ɯ}V^{a}r^{ɯ}m^{ɯ}V^{a}r^{ɯ}/** (realized as **[m^{ɯ}ar^{ɯ}m^{ɯ}ar^{ɯ}]**)

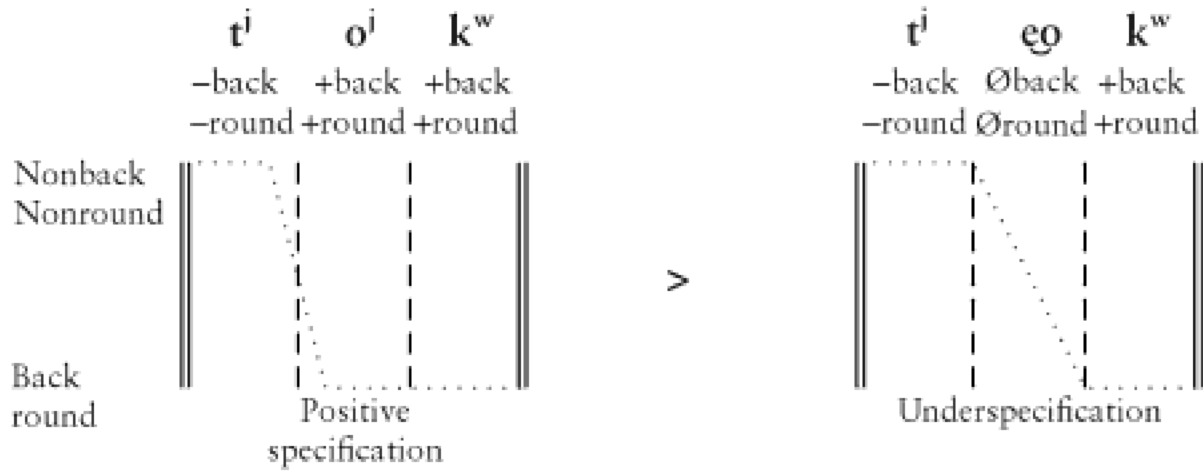


Figure 6.15: Phonetic specification and phonetic underspecification

Given this fact, it would appear that changes in the physical properties of vowels, those properties which serve as the basis for acquisition, are not required in every instance in order to trigger a reanalysis of the phonological, or even the phonetic, properties of that vowel. The vowel in the MRS word for 'five' has apparently always been pronounced 🚹**i**🚹, but the reasons why it has been so realized have shifted over time.

If we seek to understand what kind of evidence is responsible for that shift, we can direct our attention to the *first* vowel of the MRS word for 'walking stick,' depicted in figure 6.14. In this case a shift in realization is observable: PMC had 🚺**tok**^{w}🚺 and contemporary Marshallese has 🚺$\mathbf{t}^{j}$**e̜o̜k**^{w}🚺. Part of this change is of course the development of a palatalized dental stop from an earlier plain dental stop. Given the fronting of the first part of the original ***o**, it would appear that this shift in the articulation of ***t** is at least part of the story. For the reanalysis of the vowel nucleus itself, once the change in ***t** has taken place, it might be useful to look again at the nature of phonetic underspecification in such cases. The change involved would appear to be representable as in figure 6.15.

In this figure, note that before the change of vowel type the left edge of the **o** was fronted and unrounded, as part of the natural transition from the $\mathbf{t}^{j}$ to the **o**. The right side of the **o** was round and back, of course, because no change along either of these dimensions was required in order to make a transition between **o** and $\mathbf{k}^{w}$. These statements also hold of the "after" stage in the figure – the left edge of **e̜o̜** is front and nonround, the right edge back and round. This is thus yet another case in which low-level transition effects have been reanalyzed as representationally-driven, which of course necessitates a reassessment of what representations are involved in the generation of the forms in question. Once the acquirer misparsed the left-hand side of figure 6.15, taking it to be the right-hand side, what analytical possibilities were available to him/her?

Note first that although we write the vowel realization in this case as **e̜o̜**, we must recognize that this does not represent some diphthongal pronunciation of the sequence.[22] In fact, the articulation of the vocalic nucleus in the right-hand

[22] Diphthongs have an on or off glide and a nucleus; in our case we are dealing with a steady transition.

side of figure 6.15 is *not a representationally possible fully-specified vowel.* There are no human languages with fully specified vowels whose representation includes continuously changing scalar values along the back and round dimensions. Thus the phonetic realization, post-reanalysis, of the vowel of the first syllable of Mrs 'walking stick' tells the acquirer that (at least some) Marshallese vowels are underspecified. Since an underspecified analysis works just as well for the second syllable of 'walking stick' – even though this vowel is *consistent* with a fully specified representation, it is *also* consistent with an underspecified one – as well as for the seemingly fully-specified vowels cited in (20), none of these forms stands in the way of an underspecification analysis of Marshallese vowels. It is thus by leveraging their analysis of vowels such as those found in the first syllable of /**tʲVᵉkʷVᵉnʷ**/ that the acquirer develops a new analysis of what would appear to us to be diachronically invariant vowels such as that of the second syllable of that word. This "phonological change" in the absence of "phonetic change" is not limited to relatively obscure cases such as that of Marshallese vowels, as I will now try to show.

6.4 Phonological Change without Phonetic Change

An interesting set of changes involving differences in phonemic and/or phonetic representation in spite of articulatory/perceptual identity can be seen from certain aspects of the well-known the Great English Vowel Shift. The basic "facts" of this case, as found in any standard handbook, I sketch in figure 6.16.

Only the long vowels were affected; the highest long vowels were diphthongized, the other long vowels rose a notch or (eventually) two. Much ink has been spilled concerning the question of whether we are dealing in this case with a "push-chain" mechanism, whereby the low vowels slowly crowded the higher ones out of the way, or a "drag-chain" or "pull" mechanism, whereby the highest long vowels diphthongized and the lower ones were pulled up to fill the vacuum. This type of argument from "phonological space," like arguments from "pattern congruity" and the like, were a hallmark of structuralist reasoning in the area of sound change.

Wells (1982: 185) states that "It is a matter of some dispute among scholars which was the precipitating factor in the Great Vowel Shift, the raising of the mid vowels or the diphthongization of the close vowels. Did the raising of the more open vowels trigger the raising of /**eː**/ and /**oː**/, which by their raising in turn pushed /iː/ and /uː/ aside into diphthongization (the 'push-chain' theory)? Or did the close vowels diphthongize first and thus facilitate or entail the raising of the mid vowels (the 'drag-chain' theory)?" Similarly, Strang (1970: 173) notes: "The principle of keeping distance, which dominates when such a [vowel] movement takes place, might be set in action in two ways. As we have seen, /**aː**/, for lack of close neighbours, was free to start drifting, and its movement would set

iː → **əj** → **aj** 'time'
eː → **iː** → **i** 'sweet'
ɛː → **eː** → **i** 'clean'
aː → **ɛː** → **e** → **e** [**ej**] 'name'
aj → **ɛj** → **ej** → **e** [**ej**] 'day'

uː → **aw** → **aw** 'house'
oː → **uː** → **u** 'moon'
ɔː → **oː** → **o** → **o** [**ow**] 'stone'
aw → **ɔw** → **ow** → **o** [**ow**] 'bow'

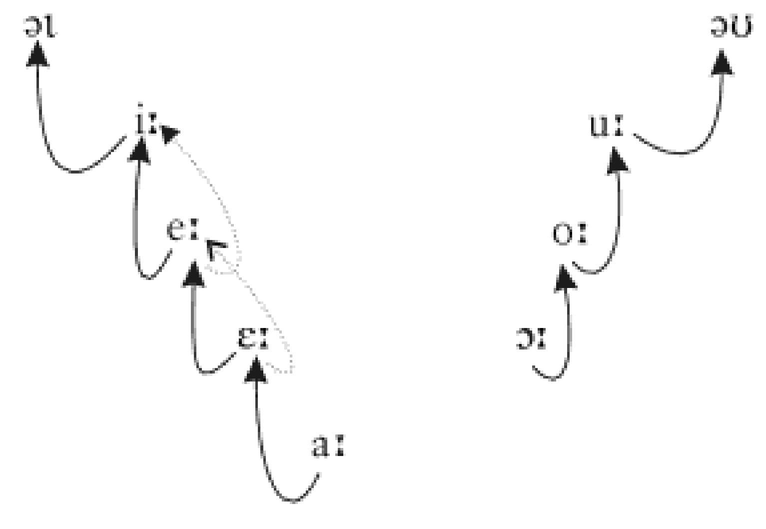


Figure 6.16: The Great English Vowel Shift

off a chain-reaction, which, since the impetus comes from behind, is called a push-mechanism. Alternatively, we have seen that /**iː**/ was liable at any time to start diphthongising, and whenever it did would create a gap in its old position, in which, by the principle of distance-keeping, /**eː**/ might be drawn; this kind of reaction, whose impetus comes from attraction into a gap, is called a pull-mechanism." There is, in any event, a general consensus that vowel shifts of this type are triggered by some functional mechanism, as Kiparsky (1988: 373) put it: "It would not be surprising if phonological systems tended to be organized in such a way as to permit maximum use of the available perceptual space, and if vowel and consonant shifts were motivated by that end." Note that any kind of strict interpretation of the push-chain or drag-chain mechanisms as being in some sense necessary would block the later /**iː**/-/**eː**/ ('sweet':'clean') merger, as well as countless other such changes in other languages. It is not clear to me how speakers during the Great English Vowel Shift era are supposed to have deduced that the raising of /**eː**/ necessitated the raising of /**iː**/, for example, while their descendants at the time of the later merger were unable to see this necessity.

It is my opinion that, in fact, neither the push-chain nor the drag-chain account is likely to be of any value whatsoever in explaining the events of the Great English Vowel Shift. Figure 6.17 is taken from Disner (1986), who attempts to develop a method to study cross-linguistic variation in vowel realization.

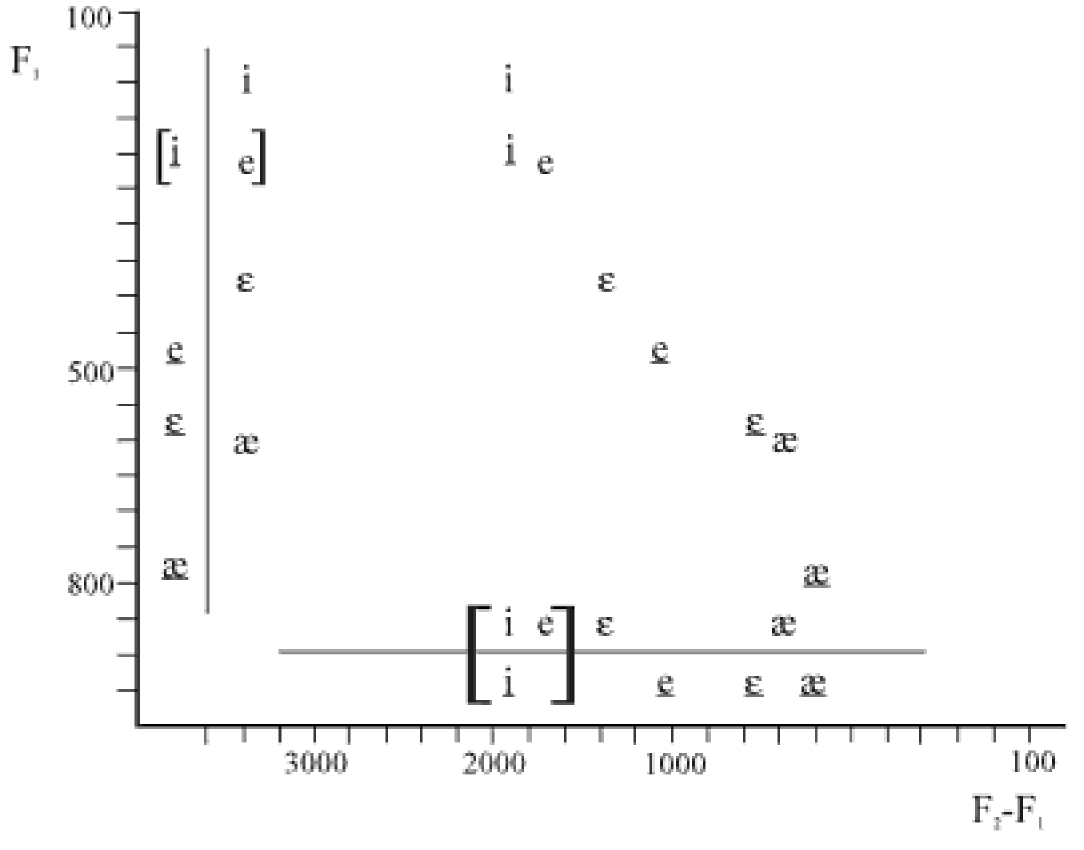


Figure 6.17: Mean values of four selected front vowels of English (underscored) and Danish. The means are plotted in a formant space and projected onto each of the axes, with brackets grouping the vowels which are not significantly different along a particular axis. (From Disner 1986.)

This figure shows comparable pairs of English (underlined) and Danish vowels, mapped onto the vowel space by charting the first formant value against the difference between the second and first formant values as is standard in such studies. Vowels which are nondistinct, statistically speaking, in a given dimension, are bracketed along that dimension. As can be seen, English **i** and Danish **e** are nondistinct both with regard to height (the *x*-axis) and backness (the *y*-axis) – that is, they have the same place of articulation. This presents a possible approach to the Great English Vowel Shift: prevowel-shift English may well have had vowel realizations not unlike Danish. The reality would be, under such an assumption, that the Great English Vowel Shift involved the assignment of a phonetically unshifted **e** to a different underlying representation: the **e** which used to be the phonetic realization of an underlying **e** became instead an **e** which was the phonetic realization of an underlying /**i**/, apparently because it was (after the diphthongization of /**iː**/), the highest and most front vowel in the language.

Prevowel-shift English: /**eː**/ realized as [**eː**]
Postvowel-shift English: /**iː**/ realized as [**eː**]

Since the /**eː**/ never "went" anywhere under this analysis, it cannot have "pushed" the /**iː**/ into a diphthongal realization, nor can it, racing to fill the phonetic vacuum left by the departing /**iː**/, have pulled the lower vowels up (and indeed, as the chart indicates, these lower vowels did not move appreciably either).

Note that, given this account, we can explain at least to some degree the phonetic realization data displayed by Disner and known at least anecdotally for some time. Perhaps more importantly, it is incumbent upon us as historical linguists to give an account of such differences in phonetic realization. A learner of English must acquire whatever mechanism gives rise to this relatively low realization for /**i**/, which means that it must be transmitted from speaker to speaker, from generation to generation, which means it can, in principle, change, and it is our job to understand its diachrony, including its origins. Whatever the representational, or computational, properties of the grammar of "English"-type speakers it might be that ensures this realization, those properties must have a history, and understanding that history is crucial to giving an account of the diachronic phonology of a given language.

Before moving on, I would like to point out another intriguing aspect of "phonetic realization rules" given by the data concerning the Great English Vowel Shift. As is well known, the mid vowels **e** and **o** of Modern English are characterized by a strong diphthongal off glide. As seen in the sketch of the vowel shift given in figure 6.17, the modern segments are the result of a merger between earlier diphthongs and earlier monophthongs. The products of this merger phonologically are (at least in most analyses of the English vowel system) the mid vowels **e** and **o**, but the normal phonetic realizations of these segments are, in fact, the original diphthongs. In other words, when the merger took place, the "marked" member of the merged pair survived phonetically, under the standard analysis, the phonological system assigned that realization to an underlying form which matched the less-marked member.

Such events appear to be quite common. To take another example from the history of English, the normal phonetic realization of /**r**/ in American English in word-initial position involves lip rounding and velarization, whereas in coda position these features are absent from /**r**/ (Lass & Higgs 1984). Thus 'red' is pronounced with a retroflexed approximant (which I will symbolize as [ɽ^{ɯ}]) which is both velarized and round. So, too, is 'wrist.' However, as indicated by the orthography, the initial /**r**/ of 'wrist' is the result of the merger in onset position in the history of English of *r*- and *wr*-. The roundedness and velarization of onset /**r**/ in English is simply the result of features of the original /**w**/ of 'wrist,' 'write,' etc., surviving as part of the representational and computational apparatus which gives rise to the realization of modern /**r**/. Moreover, this realization not only survived – it was generalized to all onset /**r**/'s, at least in most American English dialects. Note that the traditional account of this change, which treats it as an instance of *w*-loss before *r*, misses both what actually happened phonetically to *wr*- lexemes like 'wrist' and, perhaps more crucially, gives no consideration whatsoever to the fact that words like 'red,' which never had a *w*, changed as well (by adopting the "round and velarized" articulation of onset *wr* from 'wrist'-type examples).

The point of these examples is clear: on the one hand we must recognize the existence of changes which affect the realization of a segment but not its underlying form (Middle English underlying /**e**/ > Modern English underlying /**e**/ – now realized as [**ej**]), and on the other, the existence of changes which affect an element's shape in the lexicon, but not its realization (prevowel shift /eː/ continuing to be realized as [e], but now as the phonetic realization of underlying /i/).[23] It is not hard to see that the failure to attend to the true complexities of phonological systems and the processes which regulate the realization of their outputs represents a major barrier to further progress in the study of diachronic phonology.

6.5 Discussion Questions and Issues

A. Consider the three-way distinction emphasized in this chapter between phonological, phonetic, and (bodily or impressionistic) output representations. Discuss the techniques which you would use to determine at which level a particular observed phonemonon finds its origins.

B. Discuss the Proto-Indo-European *ŋ issue. Data is provided for the word for 'five' in Latin, Armenian, Lithuanian, Sanskrit, and Greek. Using the three-tier system advocated in this chapter, which distinguishes between the levels of representation addressed in the previous question, develop an account which explains which of the Indo-European daughter languages mentioned appears to have undergone a change from the Proto-Indo-European situation.

C. Discuss the full set of developments sketched in figure 6.16 with regard to the issues of alleged "phonological space" (e.g., the "drag-chain" and "push-chain" theories alluded to in the chapter).

D. Find a sound-change problem you were exposed to in an introductory (or introduction to historical linguistics) course and examine it in light of the three-tier system presented in this chapter. Which of the changes in your problem appear to be readily attributable to the kind of "phonemicization" account sketched here?

[23] Both cases can be easily overlooked in the types of material normally used by historical linguists doing, e.g., comparative reconstruction, at great cost to explanatory power.

7 The Regularity of Sound Change

7.1 The Neogrammarian Hypothesis

The claim that sound change is a regular process has long been sufficiently well-linked to the "Neogrammarians" that it generally goes by the name "The Neogrammarian Hypothesis." For the Neogrammarians themselves, the central claim was simply that "sound change" was *systematic* (in German, *konsequent*). Hermann Paul provides some explication of what this label meant to him and his fellow Neogrammarians (Paul 1880: 69):

> When we speak of systematic effects of sound laws we can only mean that given the same sound within the same dialect every individual case in which the same phonetic conditions are present will be handled the same. Therefore, either wherever earlier the same sound stood, also in the later stages the same sound is found or, where a split into different sounds has taken place, then a specific cause – a cause of a purely phonetic nature like the effects of surrounding sounds, accent, syllabic position, etc. – should be provided to account for why in the one case this sound, in the other, that one, has come into being.[1]

There are two basic claims regarding sound change in this passage which have come to characterize Neogrammarianism: sound change is *regular* and it is *purely phonetically conditioned*. Hoenigswald (1978) has argued, I think convincingly, that these claims are true by definition within the context of the work of the Neogrammarians. Ancillary hypotheses (e.g., "analogy," the phrase "within

[1] "Wenn wir daher von konsequenter Wirkung der Lautgesetze reden, so kann das nur heissen, dass bei dem Lautwandel innerhalb desselben Dialektes alle einzelnen Fälle, in denen die gleichen lautlichen Bedingungen vorliegen, gleichmässig behandelt werden. Entweder muss also, wo früher einmal der gleiche Laut bestand, auch auf den späteren Entwicklungsstufen immer der gleiche Laut bleiben, oder, wo eine Spaltung in verschiedene Laute eingetreten ist, da muss eine bestimmte Ursache und zwar eine Ursache rein lautlicher Natur wie Akzent, Silbenstellung, u. dgl. anzugeben sein, warum in dem einen Falle dieser, in dem anderen jener Laut entstanden ist."

the same dialect," and the concept of "sporadic sound change"[2]) allow the Neogrammarians to restrict the use of the term "sound change" to precisely those events which are regular and phonetically conditioned. As is always the case in definitional matters, the Neogrammarians cannot then be "wrong" in their assertions about the regularity of sound change. The way their definitions divide up the phenomena could be such that they enhance, or such that they impede, our understanding of the world, but there is no fact of the matter in such a case.

For a modern linguist, who has a very different understanding of the nature of the object of linguistic study ("the grammar" as an aspect of an individual mind/brain, for example), the question naturally arises of whether or not the terminological categories established by the Neogrammarians can be given a more substantive foundation – that is, whether the distinction between "sound change" and "sporadic sound change," for example, can be made to follow in some sensible manner from our current conception of the human linguistic endowment and the nature of language change itself. I believe that the Neogrammarians were correct in distinguishing between two fundamentally different types of event which can occur in the course of the transmission of linguistic knowledge. As to the question of whether or not sound change is "purely phonetically conditioned," we will see that developing an answer to this question involves a number of additional definitional matters which I will also try to address in what follows.

In general, modern literature on the Neogrammarian doctrine assumes that its two central propositions – that sound change is *regular* and that it is *purely phonetically conditioned* – are independent. The propositions are thus usually evaluated in a manner consistent with that assumption. It has been claimed by numerous modern authors that *both* propositions are false (see, e.g., Kiparsky (1995) with literature).

It is not without interest to attempt to understand *why* the two proposals are linked by the Neogrammarians themselves. I believe that the Neogrammarian Hypothesis represents not two independent conjoined claims about the nature of sound change, but rather two necessarily related components of a single conception of the phenomenon. I will demonstrate this first by showing that a standard interpretation of the meaning of "regular" appears to be, on its own, relatively uninteresting. However, when put together with the issues surrounding the proper characterization of the *environment* in which a change takes place – i.e., its conditioning – the issues become much more intriguing. While

2 Defined by Paul (1880: 64) in the following terms: "It is not in this case a question of the changing of the elements out of which speech is constructed by shifting, but rather of a substitution of these elements in certain individual cases." ["Es handelt sich hierbei nicht um eine Veränderung der Elemente, aus denen sich die Rede zusammensetzt, durch Unterschiebung, sondern nur um eine Vertauschung dieser Elemente in bestimmten einzelnen Fällen."]

the Neogrammarians cannot, given the state of their understanding of the nature of grammars, have had precisely the system I propose in mind, it seems that the fundamental success of methods which are directly dependent upon Neogrammarian notions, such as the comparative method, indicate that their pretheoretical phenomenological insight in this domain was quite advanced.[3]

Turning first to the "regularity" issue, it would appear that the standard interpretation of this term in historical linguistics is relatively straightforward. Given a change of the type $X \rightarrow Y / Z$, the change is regular iff for every X in environment Z in G_1, we find Y in G_2.[4] It seems clear that "sound change" (and indeed, any change) will be "regular" under this conception of what precisely "regularity" is. A change will be a maximally general statement of a difference between G_1 and G_2 (refer back to figure 3.1, p. 28 above). If a claimed change ($X \rightarrow Y / Z$) is a true assertion about the relationship between G_1 and G_2, then the conditions for the application of the term "regular" will be met. While this appears at first to make "regularity" a resoundingly uninteresting issue, I will attempt to show in what follows that it nevertheless allows one to focus the discussion on a set of precise issues which give these concepts valuable content.

Let us examine a typical case of what has traditionally been called "sporadic" sound change (generally felt to be nonregular – i.e., outside the domain of Neogrammarian "sound laws"): Proto-Polynesian **lango* shows up as *ngaro* in Maori.[5] The expected outcome, given the regular change of PPN *l* to Maori *r*, is *rango* – the attested form shows an irregular metathesis.[6] This "change" took place on at least one occasion in the speech of someone from whom, for sociolinguistic reasons, it diffused.[7] An accurate statement of that change at the moment of innovation will require that the environment, Z, be *lexical*, rather than phonological – i.e., this was a change in the phonological representation of an individual lexical item, not in the phonological system of Maori.[8] Since, if the statement of the change is to be accurate, the environment must fully spell-out

[3] The methodological problem of accounting for the stunning success of the comparative method – which depends crucially on Neogrammarian assumptions – is generally neglected by those who reject the Neogrammarian Hypothesis of regularity.

[4] I leave to one side here the possible interaction of this change with other change events. In general, in the conception of change given here (a relationship between an input source and the grammar resulting from an acquirer being exposed to that source) such complications will be minimized.

[5] I follow standard Oceanic practice in representing the velar nasal with the orthographic sequence <ng>.

[6] Compare PPN **langi* 'sky' > Maori *rangi*, with unmetathesized **l* and **ng*.

[7] Note that the change may have taken place any arbitrary number of times – it is the chance coming together of an innovation in the grammar of a particular individual who happens to occupy the right kind of sociolinguistic nexus which leads to its presence in the historical record of Maori.

[8] Compare Paul's characterization of "sporadic sound change" cited above.

the lexical item in which the change took place, the change will be regular within its domain (in this case, a single lexical item).

We see, therefore, that the *environment* is crucially involved in any discussion of "regularity." If the term "regular sound change" is to have any useful meaning, we cannot use it to refer to any change which is regular in its stated environment (for, if the environment is stated correctly, this will always be the case). On the other hand, we cannot require of a change that it have *no* conditioning environment if it is to be counted as "regular" – this would exclude many cases which are clearly regular in the required sense (e.g., intervocalic lenition, final consonant loss, etc.). One coherent way to limit the term "sound change" is thus by requiring that the environment in which the change takes place be specified in *phonological*, rather than lexical terms. This was, in some ways, the tack taken by the Neogrammarians, and it seems a useful one.

Neogrammarian theory was thus never intended to account for changes in the phonological representations associated with individual lexical items. Such "lexical" changes are rather numerous – e.g., my grandmother's word for what I call a 'couch' was 'davenport.' The coming into being of the difference between her lexical item and mine is not a change anyone would want to call a "sound change," clearly, even though the phonological representation associated with a given semantic entity has changed. If we restrict "sound change," as we, in my view, must if we are to exclude */dævnport/* > */kawč/*, to instances in which the environment is to be stated in phonological, rather than (e.g.) lexical terms, it is clear that sound change will be regular in the required sense.[9]

It thus appears that fundamentally distinct types of misanalysis are involved in the two cases. It would not be helpful to the enterprise of historical linguistics if this difference were to be ignored. To call the contrast between "(regular)

[9] It is worth pointing out that even under a rather different, perhaps more sophisticated, view of what should count as "sound change" it may still be most useful, methodologically, to keep sharply distinct "regular" and "sporadic" events. One could argue that rather than using the *environment* to classify a change as "lexical" or "phonological," one might want to ask just which aspects of the output of G_1 formed the basis for the misanalysis by the acquirer. Under this conception, the *ngaro* case will have a very different status from the 'couch' case. In the former, it seems likely that the misparse which gave rise to the metathesis was phonetic in nature, while in the latter, this seems most unlikely. But even under this conception, there is clearly a difference between the misanalysis of PPN **lango* and that involved in a "regular" sound change such as *p^h > ϕ. In the **lango* case the misparse was idiosyncratic – it did not lead the acquirer to treat all subsequent instances in his/her input data of *l . . . ng* as *ng . . . l*. By contrast, in the case of the misparse of *p^h as ϕ, an acoustic "chunk" involving labiality and continuancy was treated as representing a single target segment with simultaneous realization of these features (whereas in the acquirer's source it had been generated as a single "contour" segment with sequential realization of these features). This particular parse of that acoustic "chunk" was then applied to all subsequent input data of the relevant shape – hence the "regularity" of the change.

sound change" and lexical changes of the type we have discussed above "merely definitional" entails that there is no crucial distinction in the underlying dynamic which gives rise to the two types of change event. To the extent that there is a fundamental difference in these mechanisms the "regularity of sound change" ceases to be a purely terminological matter.

Interesting questions arise at this point. On superficial consideration, the Neogrammarian Hypothesis appears to be true by definition. On a more complex reading, it appears to be a valid way of approaching issues in sound change. Why, then, is the hypothesis so widely opposed by so many historical linguists, especially theoretically oriented ones? What are their concerns and how do those concerns relate to the issues raised in this book? To explore these questions, we turn to a consideration of the extensive critical discussion of the Neogrammarian Hypothesis by Paul Kiparsky, especially Kiparsky (1995).

The first part of Kiparsky's paper proposes a type of phonological change not previously described in the literature. The basic idea is relatively straightforward. Taking English nasals as our example, it is clear that they are all voiced ([+voice]). This means of course that given the information that a segment is [+nasal], voicing is predictable (much as the plural /kæts/ is predictable from the existence of a nominal stem /kæt/ with no override of the default plural). The general principle that redundant (i.e., predictable) information should not be stored in the lexicon (since it is derivable) can thus be invoked to deduce that nasals do not have a [+voice] specification in underlying representation. On the other hand, they clearly should not be marked [–voice], either. It thus follows that they carry no value for the feature [voice], i.e., they are *underspecified* with respect to voicing, much as Marshallese vowels are underspecified along the dimensions of [back] and [round].

A problem arises, however, in the course of a derivation in which, e.g., stops assimilate in voicing to following consonants. In general, nasals will trigger voicing of voiceless stops in such an instance, but how can they, if the nasals need not bear the relevant [+voice] feature? Such redundant, predictable values must be "filled in" before the relevant rule applies, it seems. If this analysis is correct, there must be a process within the phonological component which fills in default values for underspecified segments.[10] That is, there are rules in phonology which provide default structure at some point in the derivation for segments which lack (but require by the time of phonetic realization) values for the features in question.

One of the central ways in which underspecification can be exploited is as follows. Imagine a situation in which the bulk of the lexicon of some language shows regular penultimate stress. Suppose further that there are a handful of

[10] I oversimplify somewhat for exposition purposes here. There may, in fact, be default-value fill-in rules applying at different levels of the phonological representation – i.e., there may be a series of such rules within the phonological component, rather than a single battery of them.

lexical exceptions to this generalization. Underspecification would license an analysis in which the stresses of the lexical exceptions are specified in the lexicon, whereas the (predictable) penultimate stresses are assigned by default (i.e., when there is no lexical override).[11]

As Kiparsky eloquently points out, this gives rise to a potential mechanism of change familiar from many cases of "regularizing" morphological change. Failure to acquire "exceptional" underlying specification will license the application of the default rules to the lexeme in question, giving rise to "regularization." The difference in this case is, however, that we are not speaking of "morphological" regularization, but rather of *phonological* regularization. The distinction will be critical, as the discussion below will show.

Kiparsky cites two examples of what we might call "phonological regularization." The first concerns the shortening of English /u:/. As Kiparsky points out, this shortening was regular (in the Neogrammarian sense) in the environment [−anterior] __ [−anterior, −coronal].[12] The environment for the change was "extended" (in Kiparsky's terms) "by relaxing its context both on the left and on the right" (1995: 643). In lexical items which show the "extended environment," the shortening took place in a lexically idiosyncratic manner. Thus when the environment was only __ [−anterior, −coronal], we find shortening in cases such as *took, book, nook*, etc. We find length in *bazooka.* And the outcome is "variable" in the case of *snook, snooker, boogie, Sook, gadzooks, spook.*[13] When the environment was "extended" to [−anterior] __, we find shortening in *good, could, should, hood* 'covering,' *hoodwink*, length in *brood, shoot, boot, behoove, scoop, coon, coot, roost, groove, . . .* , and "variation" in *roof, rooster, hoodlum, cooper, hoof, room, root, hood* 'ruffian,' *coop, proof.*

[11] The assignment of stress normally demands that one also determine syllabification and potentially moraic structure. These also need to be prespecified in the "exceptional" cases, but determined by rule in the default cases. As pointed out by Inkelas (1994), this is somewhat awkward for underspecification theory, since syllabic and moraic structure may be perfectly well-formed according to the default rules for building such structure, but nevertheless would have to be prespecified in such cases.

[12] Examples cited by Kiparsky include *cook, hook, shook, rook, brook, crook, hookah.*

[13] Note that from this and other claims of his paper, Kiparsky is working with traditional notions of "language" (E-language) and "change." The outcome is variable only from the point of view of "the English language" (my dialect, e.g., has short *u* in *snook* and *snooker*, *u:* in *gadzooks* and *spook*, and lacks the other example lexemes altogether – it shows no "variation"). In a chart (21.1, 1995: 643), he claims that Neogrammarian sound change is "rapid" while lexical analogy and lexical diffusion are "slow" – positing extended temporal dimension for change events better conceived of, in my view, as a sequence of discrete and independently motivated events. This, once again, is only consistent with an "E-language" notion of the object of diachronic linguistic study. It is difficult to determine whether many of Kiparsky's data analyses and claims about Neogrammarian sound change can be made sensible under more standard generativist assumptions about the object of study in linguistics.

The second example discussed by Kiparsky is the well-known instance of *æ* tensing, which in the "core" case took place before tautosyllabic *f, s, θ, n*, and *m*. We simplify Kiparsky's presentation, which is quite detailed, and discuss only the Philadelphia case, in which the rule shows, in addition to the "core" environments above, an extension to the environment before *d* and *l* (as well as occurring before the segments given in the "core" environment) and a relaxation of the tautosyllabicity requirement. Since the [+tense] feature of tense *æ* (which Kiparsky sometimes writes /A/) is "predictable" in core environments, one can assume that *æ* is underlyingly unspecified for tense and is assigned the feature [+tense] by a "structure-building" process such as the one outlined above which assigns default values to underspecified segments. The rare exceptions in core environments (Kiparsky lists *alas* and *wrath*) will have an *æ* which is exceptionally marked [–tense] in the lexicon, thus preventing the structure-building rule from assigning it the default (for this environment) [+tense] specification. Labov has shown that *æ* tensing in Philadelphia is being extended beyond its core environment in the two ways mentioned above. First, some *æ*'s which meet the conditioning environment as far as the following segment are concerned, but in which the consonant in question is *not* tautosyllabic with the *æ* in question, show the tensing anyway (*planet, damage, manage, flannel*). Second, the consonantal environment for the tensing is being extended to include cases of *æ* before *l* and *d* (e.g., *mad, pal*). Kiparsky makes three important points about the "extension" of the tensing rule: (1) "the environments into which tense *A* is being extended are not arbitrary phonologically"; (2) "there are no reported caes of lax *æ* being extended into words which have regular tense *A*"; and (3) "[æ] changes not into any old vowel, but precisely to [A]."

Kiparsky's analysis of the "extension" of *æ* tensing runs as follows (1995: 651)

> The old tensing rule, applicable before a class of tautosyllabic consonants, is generalized by some speakers to apply before certain additional consonants and the tautosyllabicity condition is dropped . . . But being structure-building (feature-filling), the rule applies only to vowels underspecified for the feature of tenseness, and speakers with the generalized rule can still get lax *æ* in the new contexts by specifying the vowels in question as la[x] in their lexical representations.

Why might the rule be extended in this way? In Kiparsky's view (1995: 644), analogical change, of which this is an example, is "an optimization process which eliminates idiosyncratic complexity from the system" – it is "grammar simplification."

I have some difficulty seeing either of the cases discussed above as involving "optimization" or "simplification" in any meaningful sense. In the case of the shortening of /u:/, is it really (computationally) more optimal or simpler to change a system which requires an exceptionless "structure-building" rule which "shortens" /u:/ in a well-defined environment ([–anterior] __ [–anterior, coronal]) to one which requires a rule of /u:/ shortening, for example in the environment [–anterior] __ but requires memorization of a list of lexical exceptions?

this latter sense, which seems to me virtually inconceivable, would the claims of Kiparsky (and others before him) that any model which licensed sound change without regard for the resulting phonological system could produce, through normal diachronic processes, phonological systems which are not within the computational capabilities of the human organism – i.e., phonological systems which violate fundamental principles of UG – be true. Since this cannot happen, by definition, sound change must be constrained in its effects such that the resultant system is a possible human phonological system – it cannot therefore proceed "blindly" in the latter sense – a sense which, as far as I can see, has never been given to the term in Neogrammarian work.

It would seem to follow from this (and such reasoning is not uncommon, though usually less explicitly formulated) that for a given change A > B, the set of grammars which contain feature A (and thus could conceivably show the A > B change), G_{A1}, G_{A2}, G_{A3}, . . . will fall into two classes. Labeling the grammars which would result from the occurrence of A > B in G_{A1}, G_{A2}, G_{A3}, . . . by means of G_{B1}, G_{B2}, G_{B3}, . . . , we can assume that some G_B will be possible human languages, and some will not (given principles of UG). Thus for some G_A, A > B is a possible change, while for other G_A, it is not. The G_A for which A > B is a possible change must share some set of structural features (such that substitution of B for A results in a possible human language), these will of course be the structural preconditions for the change A > B.

It is important to be clear, given the claims of the relevant section of Kiparsky's paper (to be discussed below), that we are talking of "possible" and "impossible" change events for a given G_A, not "likely" or "unlikely" change events. It is possible that there are structural features which *favor* a particular change (making it more trivial) or disfavor a particular change (making it less trivial), but these have nothing to do with the principles of UG, which only require of a given change that the result of its taking place in a given grammar will give rise to a *possible* human linguistic system.[19]

There is, however, a serious problem with the line of reasoning concerning structure dependence outlined above. It assumes, crucially, that the grammar changes "one rule at a time." There is, however, nothing in the model of grammar transmission which requires, or even favors, such a conception of change. The possibility of multiple simultaneous changes between grammars during the transmission process makes the argument considerably more complex, since for any given G_{A_x} for which a simple change of A > B would result in an "impossible" grammar G_{B_x}, a simultaneous change of the type C > D could render the resulting grammar (G_{A_C}, changes A > B, C > D, resulting in G_{B_D}) fully licit, in UG terms. Therefore the conclusion reached above, that for some G_A a change of the type A > B is an "impossible" diachronic event will hold only if A > B is the *only*

[19] The structural preconditions which "favor" a given change are those features responsible for the ambiguity in the output along the change dimension which licenses reanalysis by the acquirer.

change under discussion. But since grammar transmission is never constrained in this way ("one change at a time"), no change, even one for which the result of applying A > B (as the only change) to the set of G_A would invariably result in an impossible human linguistic system (i.e., for which all G_B are ruled out by UG), can be considered an "impossible" change on structural grounds alone (although it may, of course, represent an impossible acoustic misparsing).

The result is that there is in fact no structure-dependence of the type argued for above: no structural feature (or set of features) of the input sources directly precludes a given change event. The only constraints of this type would have to be much more complex than the literature normally assumes. For example, if it were the case in the scenario sketched above involving the changes A > B and C > D that all changes of the type C > D (i.e., all changes which would make the result of the application of A > B to G_A lead to an acceptable result for UG) were excluded in their own right, which itself involves proving that all changes which could make the C > D-type changes possible are also excluded (and all changes which would make the changes which would make changes of the C > D-type possible are excluded, etc., leading to a potentially infinite line of argumentation), then and only then could we exclude A > B as a possible diachronic event. The prospects for constructing actual arguments which would support a claim that a given change event can be excluded as impossible for a given input grammar (or set of input grammars which share some structural feature) are thus rather bleak, in my view.[20]

This result is, in fact, hardly surprising. If the core context for change is reanalysis during the acquisition period, there can hardly be structure-dependence of the type usually advocated. The structure is not given, it does not exist for the acquirer, it is, in fact, what is being constructed. Only the output of the acquirer's source(s) is given. Any analysis which is consistent with this output (and of course with the principles of UG) is possible. The constraints on change will therefore be a combination of the set of possible (mis)analyses of the input data (much of which is presumably ambiguous – i.e., consistent with more than one grammar) and the global constraint that holds that the result of opting for various choices made possible by these ambiguities be consistent with UG. That is, the set of possible analyses of the data will generate a set of "possible" changes: A > B, A > C, C > D, E > F, etc., and the principles of UG will demand that the grammar constructed show a subset (potentially null) of those changes which result in a grammar which is consistent with the principles of UG. The constraints provided by UG are univeral, of course, and thus can show no dependence on the structure of the input sources. The candidate set (before the constraints of UG) of "possible" changes is constrained only by possible misanalyses of the input strings provided to the acquirer: some of these misanalyses probably follow from nonlinguistic

[20] The place to seek constraints on possible change events is not, therefore, in the underlying structure of the input sources, but rather in the set of possible misparsings of the output generated by the grammar being acquired.

aspects of the human perceptual system, others from more directly linguistic concerns, but they are not of the type that the possible B's posited for a given A in the input sources is constrained by the structural features of the grammar containing A – they cannot be, the child does not know what the structural features of the grammar containing A are.[21] It is precisely those features which the grammar, once constructed, is a formulated hypothesis about.

What are Kiparsky's arguments in favor of the structure-dependent nature of sound change? There is not much data-oriented argumentation in this section of Kiparsky's paper – the arguments are more conceptual. The first offered can be seen in the following quotation (1995: 654):

> Jakobson was in fact able to cite fairly convincing long-term tendencies in the phonological evolution of Slavic, involving the establishment of proto-Slavic CV syllable structure by a variety of processes (degemination, cluster simplification, metathesis, prothesis of consonants, coalescence of C + *y*, coalescence of V + nasal) . . . Since it is human to read patterns into random events, it would be prudent to look at such arguments with a measure of suspicion. But the number and diversity of phonological processes collaborating to one end do make Jakobson's case persuasive.

Such "long-term" conspiracies, spanning hundreds of years in the case of Slavic, are frequently cited in the literature. They clearly argue against the "blind" operation of sound change, a thesis which we have no desire to defend in any event, if they exist. The question of course is how can they exist? How can a language which does not have a restriction against closed syllables (as pre-proto-Slavic did not) acquire a compulsion to develop one, a compulsion which achieves its desired goals only hundreds of years later? Kiparsky acknowledges that this "mysterious mechanism of orthogenesis" itself has no explanation (1995: 655). Indeed, "has no explanation" is rather weak in its criticism of such a hypothesis. Where would such a conspiracy reside and how would it exercise its influence on grammar construction over such a long span of time? Why would a "language" (if we even wanted to admit the relevance of such a concept into our considerations) conspire for generations to attain the point where it has only open syllables, only to surrender this feature shortly thereafter?

Kiparsky's own attempts to resolve this difficulty cannot, I think, be deemed successful. His proposal can be seen in the following quote (1995: 655):

[21] One could acknowledge an indirect connection, in as much as the structural features (including of course the input representations) of the source grammar partially determine the output of that grammar. But it hardly seems worthwhile to pursue this indirect connection, when an explicit theory of the connection between misanalysis and all aspects of the acoustic output (not just those aspects of it conditioned by structural features of the grammar) will be required in any event. Surely the constraints should build around this more direct relationship.

> Traditionally, the acquisition of phonology was thought of simply as a process of organizing the primary data of the ambient language according to some general set of principles (for example, in the case of the structuralists, by segmenting it and grouping the segments into classes by contrast and complementation, and in the case of generative grammar, by projecting the optimal grammar consistent with it on the basis of Universal Grammar). On our view, the learner in addition selectively intervenes in the data, favoring those variants which best conform to the language's system. Variants which contravene language-specific structural principles will be hard to learn, and so will have less of a chance of being incorporated into the system.

Note first that this is an inherently conservative principle – it favors minimal change. It can hardly explain, and indeed directly counterpredicts, the "long-term tendencies" posited by Jakobson for Slavic. Since Slavic did not have a constraint against closed syllables when Jakobson's "conspiracy" began (indeed, it did not have such a constraint until Jakobson's conspiracy was completed), Kiparsky's proposal would predict that changes which favored a restriction to CV-syllable types (i.e., that disfavored coda-consonants) would be selected *against* by the acquirer, rather than favored (since a restriction against coda-consonants would "contravene language-specific structural principles").

Moreover, the proposal demands that the acquirer, during the acquisition process, have access to "language-specific structural principles," though these are presumably available only *after* the specific language in question has been acquired. This conceptual difficulty also undermines, in our view, Kiparsky's "priming effect" proposal (1995: 656):

> Redundant features are likely to be phonologized if the language's phonological representations have a class node to host them.

Once again, one of the key challenges to the acquirer is precisely to determine which class nodes need to be present in the language's phonological representations. Changes such as "phonologization" are not dependent upon existing representations (which the child cannot directly access), but rather represent solutions to that challenge which differ from those opted for by previous generations.

The data cited in support of this principle is replete with empirical difficulties. The first argument provided by Kiparsky concerns tonogenesis (1995: 656): "The merger of voiced and voiceless consonants normally leaves a tone/register distinction *only in languages which already possess a tone system*"[22] [Italics in original – MRH]. Though I do not know of a large number of instances of tonogenesis in nontonal languages which are not in contact with tonal languages, such cases clearly exist. The Huon Gulf and New Caledonian cases come to mind, as does, arguably, Scandinavian – see Ross (1993) and Rivierre (1993).

[22] Kiparsky goes on to acknowledge that areal effects can trigger tonogenesis in nontonal languages.

The next case mentioned concerns compensatory lengthening: "DeChene and Anderson (1979) find that loss of a consonant only causes compensatory vowel lengthening when there is a preexisting length contrast in the language." Kiparsky himself notes the exception provided by Occitan to this claim (in his footnote 16).[23]

Finally, the third piece of empirical support offered by Kiparsky concerns the genesis of geminates: "total assimilation of consonant clusters resulting in geminates seems to happen primarily (perhaps only?) in languages that already have geminates (Finnish, Ancient Greek, Latin, Italian). Languages with no pre-existing geminates prefer to simplify clusters by just dropping one of the consonants (English, German, French, Modern Greek)." Ancient Greek and Latin, in any event, frequently "simplify clusters by just dropping one of the consonants" (rather than all clusters giving rise to geminates).

Of course none of these empirical difficulties is of much significance, given how the "priming effect" is stated: it is not a claim about the possibility of certain changes (and thus can play no role in the development of a theory of constraints on diachronic phonological events), but merely about the "likelihood" of certain changes (and thus could be useful in deriving a triviality index for a given change in a given language). Since all of the changes involved are optional (i.e., they need not take place) and since the same changes may take place in languages which lack the necessary "priming effect" (they are just, if Kiparsky is right, "less likely"), one would not want to label such changes "structure-dependent" (which implies that they have structural preconditions to their occurrence and will be triggered under these structural conditions).

The brief section of Kiparsky's paper on "naturalness" is, in my view, marred by a lack of clear distinction between constraints on synchronic phonological processes and constraints on diachronic events. There is no *a priori* reason to believe that synchronic phonological systems and diachronic events are constrained by principles which are at all the same. Indeed, there is a very real danger that many constraints proposed on synchronic phonological systems (proposed because there are no known exceptions in the languages we have studied so far) are in fact not synchronic constraints at all. Consideration of how each of these types of constraints – synchronic and diachronic – should be deduced reveals little connection between the two: synchronic constraints should ultimately reflect the

[23] Observe that many of the languages which have a preexisting length contrast and show compensatory lengthening for the simplification of some clusters, do not show compensatory lengthening for the simplification of others, thus lessening the force of Kiparsky's use of "are likely to be phonologized" in the statement of the priming effect. Indeed, as DeChene and Anderson (1979) already argued, the nature and syllabic structure position of the lost segment are very relevant to whether compensatory lengthening occurs, yet these are matters quite unrelated to the question of whether the language's phonological representations have a class node to host certain "redundant" features.

real-time computational capabilities in the area of phonology of the human organism. They follow from the "phonological" portion of UG. Diachronic constraints, on the other hand, should result from a theory of possible misanalyses of input data. The relationship between the two is hierarchical: the phonological part of UG constrains possible diachronic events in that no acquirer can subject his or her input data to an analysis which results in an impossible (given the constraints of UG) phonological system, because the human organism is not capable of constructing such systems. Diachronic events on the other hand have no effect on UG, given the uniformitarianism hypothesis (i.e., assuming that at the time depths within which historical linguists normally operate there have been no "evolutionary" changes in UG). However – and here's the rub – diachronic events provide us with the bulk of our evidence for "possible" phonological rules: the morphophonemic alternations which form the backbone of phonological rule systems are the result of diachronic events. It is entirely possible, in my view, that the set of possible phonological processes is a superset of the set of possible diachronic misanalyses, in which case no cross-linguistic survey of phonological processes – which is necessarily restricted to those processes which have resulted from diachronic misanalyses – will reveal the actual computational capabilities (in the phonological domain) of the species.

This impacts Kiparsky's argument in the following way: if some of the proposed constraints on phonological systems are in fact not constraints on the organism (i.e., deducible from UG), but rather constraints on diachronic events incorrectly analyzed as constraints on phonological systems, then the "structure-dependence" of diachronic events which Kiparsky attributes to "natural" phonological processes is a mirage. The structures upon which the diachronic events appear to depend are mere synchronic statements of constraints upon possible diachronic events. Indeed, it appears that the diachronic filter, which, as simple laboratory experimentation on the confusion matrices generated by perceptual testing reveals, favors some misanalyses over others (rather than absolutely precluding disfavored misparsings), is the reason why most claims about "naturalness" and "markedness" are statistical, rather than absolute claims.[24]

The stunning success of methods in historical linguistics which depend upon there being a phenomenon of "regular sound change" reveals that the proposals of the Neogrammarians must contain some essentially valid content, in my view. We have seen that many of the criticisms of the Neogrammarian Hypothesis appear to miss their mark – but how exactly *do* we get there to be "regular" sound change? Why aren't all misanalyses in the course of grammar transmission restricted to the misanalysis of individual lexical items?

I have proposed that misanalysis of the target articulation can be of two basic types: it can be morpheme specific (in which case it may lead to a direct restructuring of the underlying representation) – such changes will be of necessity

[24] The work of John Ohala is particularly instructive in this regard. See Ohala (1981), (1982), and the references therein for further discussion.

"sporadic," applying as they do just to the misparsed morpheme(s); or, it may be a general, across-the-board misanalysis (in which case, of course, it will affect all realizations of the segment in question). It is easy enough to see how the former type of change could take place: one need only misparse a realization of a given morpheme (because of failure to properly undo transition effects, failure to realize that a production error has been made, or a variety of other factors). That such misparsing is not always "corrected" in the light of subsequent input is clear enough from the existence of "sporadic" changes of this type – though it should be equally clear that the vast majority of such misparsings are in fact later corrected by the acquirer his/herself based on further tokens of the morpheme in question. But how is "across-the-board" misparsing possible? That is, having established that there is very compelling reason to believe that the Neogrammarians were correct in positing such a thing as "regular" sound change, we must still confront the question of how it could be possible under normal acquisition circumstances.

It is common to attribute such changes to the acquirer's "mishearing" the target segment of his or her input source, an idea which has been developed (in more sophisticated ways than this summary indicates) by John Ohala in numerous works (e.g., 1981).[25] For example, Ohala (1981) treats a case of the fronting of back vowels in the environment of coronals (using Tibetan data like that in table 4.1, p. 63 above) in the following manner. All back vowels are somewhat fronted phonetically in the environment of (let's say a following) coronal because the front part of the tongue must move, during the articulation of the vowel, towards the alveolar (or dental) region – i.e., front – in order to make the relevant stop (or fricative) closure. Ohala argues that listeners "know" about this (and other) "transition effects" and readily "undo" the fronting in this context, thus correctly hearing a back vowel in spite of the fronting. In a process which is familiar from many structurally similar cases (e.g., umlaut), if the following coronal is "lost" (as a result of its being "weakly articulated" or otherwise obscured in some way which is left a little vague), the acquirer who is to lose the coronal will still hear the fronting (because, after all, the coronal is there in the speech of his or her sources, by definition), but will no longer be able to attribute it to the following coronal (which went unperceived), and will thus posit a front (rounded) vowel. This accounts for changes (in Tibetan, for example) of the type /lot/ > /lö/. This analysis appears reasonable. Changes of this type, where the "triggering" segment for a change is lost at the same moment as

[25] Ohala pointed out quite nicely that this is not the only mechanism. He argues that "dissimilatory" sound change is frequently due to the hearer's correctly noting some acoustic feature present in a given segment, but mistakenly attributing the cause of the presence of that feature to "spread" from an adjacent segment. As a result, the acquirer posits in his or her own grammar a target which lacks that feature – i.e., which makes the target less like its adjacent segments.

the change itself takes place, are widely known. The problem is simply this: such changes are among the most regular known to us (umlaut, palatalization, and various other types of contact assimilation), which means that we need them to be due to a very general misparsing. Yet it is unclear, given that, e.g., in the case under discussion the final coronal must be present in the input source, how we get the acquirer to *invariably* fail to "hear" it – it cannot be, as is implied in Ohala's discussion, due to ambient noise or occasional "weak" articulation (this would give rise only to "sporadic" misanalysis). Crucially, to get a regular change, the acquirer must hear accurately the acoustic pattern emerging from his or her source, but assign that pattern a different "target" in the grammar being constructed (this will ensure that the next time that pattern is encountered, that manifestation, too, will be assigned the new target articulation). It cannot depend on failing to hear something in the signal, but must instead depend on correctly hearing the signal but attributing its features to some other articulatory mechanism than that being used by the source.

In the case of a change of the type /lot/ > /lö/ what we must recognize is that the primary cue as to the place features of the final stop were in fact the transitions to the coronal place of articulation, realized on the preceding vowel. Once the acquirer posited an analysis whereby the particular acoustic pattern that the /o/ in this context showed was to be accounted for by positing the target segment /ö/ (by misparsing transitional features as part of the target articulation), the evidence for a following coronal ceased to exist – what had been transition features to a following coronal were now inherent features on the segment itself. Moreover, when the acquirer heard similar transition patterns on other back round vowels, s/he also posited a front rounded vowel to account for the acoustics of these segments, thus losing, in these cases as well, the evidence for a following coronal. In the absence of such evidence, no coronal was posited by the acquirer, and the coincidence of vowel fronting and coronal loss is accounted for.[26]

Thus assimilation cases such as this one are much more similar to Ohala's dissimilation cases than his own discussion indicates.[27] In the dissimilation cases, it isn't a failure to hear a given feature on some segment which gives rise to the change. It is rather an accurate perception of the feature but a misattribution of the segmental source of that feature. The acoustic string XY is misparsed (but not misheard) such that the feature's presence on X is attributed to spread from

[26] It seems likely to me, though I have no evidence in the relevant case, that this loss would take place through an intermediate stage in which the /t/ was replaced by a glottal stop. This would account for the perception of a "checked" syllable on the part of the acquirer, without necessitating the positing on his/her part of any oral place features associated with the stop in question.

[27] Indeed, in some sense, it is identical to Ohala's dissimilation case. Once the "coronal" (i.e., front) features were assigned as inherent to the vocalic nuclei, the coronal stop underwent place dissimilation of the Ohala type.

Y, whereas in the input source it was an inherent feature on X. The primary cause of the *assimilatory* change discussed above is not the failure to hear the following coronal, but rather the misattribution of the source for fronting on the /o/: in the source grammar the fronting was due to spread, in the acquirer's system it is inherent. Being inherent, it fails to provide evidence for the presence of a following coronal, for which it, in the earlier grammar, served as the primary acoustic cue. Therefore, in general, assimilation will result when features on a segment which were, in the source grammar, due to "spread" are misparsed as being inherent; dissimilation will result when features which were inherent in the source grammar are misparsed as being due to "spread." It follows, of course, that assimilation may give rise to simultaneous "loss" of the "triggering" segment (whose cues will have been incorporated into the assimilated segment as inherent properties – thus ceasing to be cues), whereas dissimilation will not (since the perceived features have to come from somewhere!).

7.2 Conclusion

There can be little question that the type of "phonetic" argumentation I have just briefly summarized is going to play a key role in the development of the theory of the regularity of sound change, but many mysteries remain (what about changes that are neither dissimilatory nor assimilatory? do such changes exist? what are their properties? etc.). I do not myself feel that we should be particularly discouraged by this fact. After all, without purging the domain which we are seeking to provide a theoretical account for of the wealth of interfering data, i.e., without adopting the "Galilean" approach so lucidly articulated by Sklar and Chomsky, progress in the explanatory domain will be impossible. We find scholars seeking explanatory principles for sound change treating multigenerational change events as a single "change" (although of course the *same* explanation will not hold for these – if indeed, they need any explanation beyond accounting for their constituent single-generation events), failing to distinguish between the "three levels" we have outlined here, ignoring fine phonetic detail, or ignoring the computational component of the phonology, or ignoring the phonemic properties of linguistic representations, reifying mystical long-term forces on "languages" (not "grammars"), invoking ever-present, constant factors as explanations for change events (e.g., the drive to simplicity, to communicative clarity, to functional ease, to aesthetically pleasing patterns in phoneme inventories, etc.), etc. I am hopeful that if we can all just be more explicit about what we are trying to explain, and why we are trying to explain *that*, rather than something else – in short, if we can all try to approach these tasks using well-established principles of scientific investigation, we could be standing at the threshold of a series of exciting breakthroughs in our understanding of "sound change."

7.3 Discussion Questions and Issues

A. Discuss the theoretical issues which arise from claims that change is "optimization" or "simplification." Are the two notions the same? If not, how do they differ? Can you envision a mechanism which would trigger "simplifying" or "optimizing" changes? Why does a particular "optimization" or "simplification" take place at a particular point in time, and not earlier, for example? Is this the same problem, or is it distinct from, the question of why a particular change (under the assumption that changes are not "simplifying" or "optimizing") takes place at a particular point in time?

B. If, as I have argued in this chapter, phonological processes are constrained, as to the entities and processes which they consist of, by the principles of UG, but sound changes, since they do not reside in a mind, are not so constrained, why do the two sets of phenomena seem so similar in the kinds of processes they license and the kinds of entities they operate over? Are they "similar," as I just said, or are they in fact "identical"?

C. In discussing the New Caledonian case in this chapter the concept of "imperfect diffusion" (e.g., via hypercorrection) was introduced. Does this create problems for the earlier claims that "change" and "diffusion" are distinct processes? Can you envision a resolution of the apparent conflict?

D. Give an overall assessment, for yourself, of Neogrammarian doctrine. Were they "right" in positing the "Exceptionlessness Hypothesis," or were they "wrong," or are those terms perhaps not particularly useful in discussing the complex theories of our forebears?

E. Consider Kiparsky's claim that "the learner in addition selectively intervenes in the data, favoring those variants which best conform to the language's system." Discuss the arguments offered up against that position, but also try to present a coherent sketch of what precisely it might mean for a variant within a language to "best conform" to that language's system. If the long-term history of language was regulated by a learning mechanism of this type, what would the gross form of languages move towards? What would languages eventually look like? You might consider these same issues with respect to "simplification" and "optimization" as mechanism of language change as well.

F. How do critics of the Neogrammarian Hypothesis deal with the fact that linguistic reconstruction, at least in the phonological domain, appears to be so successful? How would you, if you were to reject the regularity of sound change, deal with this problem? Does the success of the comparative method, if real, undermine the claim that the Neogrammarian Hypothesis is false?

Part III Syntactic Change

8 What is Syntactic Change?

> Clearly there are phenomena of a syntactic nature ("syntactic" in the broadest sense) which are attested in many languages of the world without there being any relationship between the speakers in question, and which, through their very generality, become clear in as much as one realizes that forms of expression that we find in Greek, Latin, and German are deeply grounded in the nature of human speech . . .
>
> *Jakob Wackernagel,* Vorlesungen über Syntax, *1920: 5 (author trans.)*

8.1 "Regular" Syntactic Change

One of the results of the extended survey of phonological change in the last few chapters is that approaches to change which focus almost exclusively upon (behavioral) output, as the Neogrammarians did, or almost exclusively upon underlying representations (phonemes), as the structuralists did, or almost exclusively upon the computational component of the phonology (rule addition, rule loss, rule reordering) as the generativists have done, are all inadequate as a foundation for the complex and demanding task of providing an insightful analysis of change events. Instead, a more comprehensive view which considers all of these elements and, perhaps most crucially, the way in which these components interact in the acquisition process, must be developed. A very similar story can be told about diachronic syntax, although that story lacks the straightforward (if at times somewhat caricature-ish) connection to well-established "stages" in the history of linguistic thought.

It is worth pointing out that although it is difficult to link the history of diachronic syntactic investigation to specific developments in linguistic theory since the mid-nineteenth century (and earlier), the frequent attribution of this difficulty to the absence of meaningful diachronic syntactic research in the past reflects a relatively superficial and egocentric reading of the history of the field.[1] It is true

[1] Campbell and Harris (1995) discuss this matter insightfully.

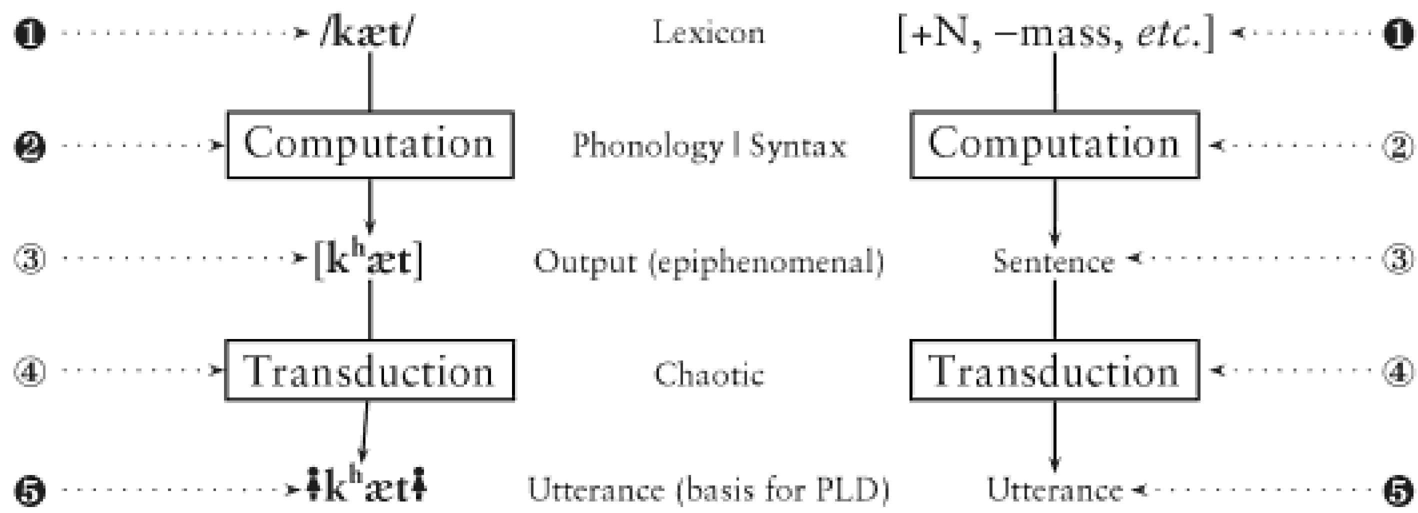


Figure 8.1: Phonology and syntax

that many of the issues which most concerned scholars working in the area of historical syntax in the pregenerative past – matters such as the syntactic conditions on the use of the subjunctive, or of the optative, or the instrumental, ablative or locative case (note the small "c") – do not fall within the primary areas of interest of contemporary syntacticians. As far as I can see, there is no disputing that the phenomena in question are syntactic in nature,[2] and thus the fact that contemporary syntactic theory has little or nothing to say about such things is more a reflection on the shortcomings of contemporary theory than it is an indictment of the diachronic syntactic research of the past. In the areas where those working in the past treated matters of contemporary interest – e.g., Bopp's work on the grammaticalization of dative abstract nouns into verbal infinitives (see Campell and Harris 1995 for a brief discussion), or Wackernagel's work on clitics (discussed in the next chapter of this book) – such work has been leveraged by modern diachronic syntacticians, much to their benefit. The serious disconnect in topics analyzed makes meaningful exploration of the relationship between modern and earlier conceptions of the nature of the diachronic syntactic enterprise a demanding exercise. In any event, the more comprehensive view of the nature of phonological change stands in direct homology to the situation in contemporary diachronic syntax. This can be seen in part in figure 8.1.

The left-hand side of figure 8.1 represents schematically the generation of a particular pronunciation of the word 'cat.' The right-hand side, even more schematically, represents the generation of some sentence.[3] Both phonological and syntactic computations are defined over features and the structures which bundle and hierarchically arrange those features. Phonological computation

[2] By which I merely mean every bit as "syntactic" as the topics which do get treated within the modern tradition.

[3] The right-hand side involves a seriously distorting simplification, in that the syntactic output representation is subjected to phonological processing before actually becoming an "utterance," which is not explicitly indicated in the figure. This matter will be discussed in some detail when we consider clitic diachrony in the next chapter.

operates only over the so-called "phonological" features brought in on lexemes (i.e., the phonological component cannot compute over features such as [−mass] or [+N]), and the syntactic computation operates only over the so-called morpho-syntactic features brought in on the lexeme (and thus not, e.g., on the [−voice] feature of the /k/ of 'cat'). The relevant features in both cases reside in long-term memory, associated with specific lexical items, drawn from the pool of features (both phonological and morpho-syntactic) provided by UG. Computation over these features (steps ❷ and ② in the figure) is present in both the phonological and syntactic modules. It is not clear whether the set of computational operations (which, recall, are defined over distinct representational systems in the two modules), including deletion and insertion of features, restructuring of bundling and/or hierarchical relationships, etc., are the same or different across the two modules, but it does not appear to be the case at this point that the two domains can be treated as computationally the same.

Both computational systems produce an output (③), obviously. This output itself is a mental representation, in short-term working memory, in the same representational alphabet as that used in the relevant underlying input forms. Again, in the case of both components, these outputs are run through a set of transducers (④), which change the form of an output from a representation to a physical implementation plan. The result of running that plan through a human body is an actual utterance (❺).

I have gone through this parallelism in some detail not so much to formulate an argument about the essential identity of the phonological and the syntactic systems, but rather to tease out just those aspects of the two systems which are different. Even a moderately observant reader will have noticed that some of the "steps" identified above have been labeled with numbers which are white against a black background (e.g., steps ❶ and ❺ for both the phonology and the syntax sides of the diagram, and step ❷ for the phonology only), while others are black against a white background (e.g., steps ③ and ④ for both phonology and syntax, and step ② for syntax). The distinction being encoded here involves what might be termed the "diachronic relevance" of the elements or computational systems under discussion at the stage in question. For example, we have argued in some detail in our discussion of phonological change that underlying phonological representations can undergo change over time and the same certainly needs to be true of the underlying representations accessed by syntactic computation. Thus the first stage for both domains is designated by a white number with a black backdrop. By contrast, the output of phonological computation, the phonetic representation in ③, is neither learned (since it results from computation over what is learned), nor an input to the learning process (since it is not accessible to the learner), and thus the establishment of this form cannot be a causal factor in change. Such elements are represented by black numbers against a white background.

Both the aspects of stored lexical representations which are accessed by the phonological computation and the ones which are accessed by the syntactic computation (❶ above) are subject to diachronic reanalysis. Neither the output of the phonological computation (the so-called "phonetic" representation) nor that of the syntactic computation can change independently of changes in the underlying representation relevant to that computational system, nor can either be accessed by an acquirer, and they are thus irrelevant for the development of a model of change in both domains.

The transduction systems involve a constantly varying set of (bodily and environmental) physical factors; however, the essentially chaotic nature of the combined effects of the numerous systems involved introduces what is basically a constant random "noise" factor in the grammar transmission process. Such a factor thus does not direct the flow of diachronic developments in any systematic manner. Aside from providing a general account of the inevitibility of mistransmission, these factors play no recoverable role in diachrony.

The actual utterance provides the basis for the acquisition process – the so-called Primary Linguistic Data (PLD). It is the only data the acquirer has reasonably direct access to, though it is seriously impoverished relative to the structures which the acquirer must posit in constructing an underlying system of the appropriate type. In the case of phonology, the utterance lacks the formal features which the underlying representation is stored in and over which phonological computation is defined. It contains no overt encoding of moraic structure, syllable structure, or higher-order prosodic representational apparatus. In the case of syntax, the utterance lacks any encoding of hierarchical structure; indeed, it is devoid of "words," of "clitics," etc. It is, after all, merely an acoustic wave.

The process of getting from this utterance, or from a set of such utterances (the PLD), to a set of underlying representations and an appropriate computational system provides the central task of language acquisition, and the core mechanism of diachronic change. Both of these phenomena result from the interaction between the innate structure of the learner (so-called UG) and the data provided in the PLD.[4]

I have withheld discussion of the second level above, the phonological and syntactic computational systems, until this juncture because it is at this point that theories of syntax come to differ rather widely in their assumptions. In order to make progress, of course, it is necessary to take a position on the nature of syntactic computation, since the properties of such a system will play a key role in the types of underlying representations we will need to posit. I will follow in my discussion in this book the quite radical, but in the end, in my view, compelling position sketched in Chomsky (1992) as part of the development of the so-called "Minimalist Program." Under this conception of things, the acquisition task in syntax is taken to be limited to what we know *a priori* must be learned

[4] Note that there are two distinct phenomena here: that is, the study of diachronic linguistics does not reduce to the study of acquisition. Acquisition, in particular, concerns itself in large part with the development, via exposure to the PLD, of knowledge states intermediate between that provided by UG (S_0) and the ultimate acquired grammar – developments which lie well outside the scope of the concerns of the historical linguist.

– the morphosyntactic (and phonological) features of individual lexical items. That is, the computational system of the syntactic module is held to be universal and invariant in its properties, all apparent cross-linguistic syntactic variation reducing to differences in features on the elements over which syntactic computation takes place, rather than variation in the computational system itself. Chomsky himself put it as follows:

> The standard idealized model of language acquisition takes the initial state S^0 to be a function mapping experience (primary linguistic data, PLD) to a language. UG is concerned with the invariant principles of S^0 and the range of permissible variation. Variation must be determined by what is "visible" to the child acquiring language, that is, by the PLD. It is not surprising then, to find a degree of variation in the PF component, and in aspects of the lexicon: Saussurean arbitrariness (association of concepts with phonological matrices), properties of grammatical formatives (inflection, etc.), and readily detectable properties that hold of lexical items generally (e.g., the head parameter). Variation in the overt syntax or LF component would be more problematic, since evidence could only be quite indirect. A narrow conjecture is that there is no such variation: beyond PF options and lexical arbitrariness (which I henceforth ignore), variation is limited to nonsubstantive parts of the lexicon and general properties of lexical items. If so, there is only one computational system and one lexicon, apart from this limited kind of variety. Let us tentatively adopt that assumption – extreme, perhaps, but it seems not implausible – as another element of the minimalist program. (Chomsky 1992: 4f.)

Obviously, what holds of cross-linguistic variation under this scenario holds of change as well – apparent change in the syntactic system will always be the result of some modification of the feature specification on lexical items (since the computational system itself is cross-linguistically, and diachronically, invariant). The computational system itself consists of the (arguably unifiable) processes Merge and Move, which concatenate elements. Merge takes elements from the so-called *numeration* – a list of elements drawn from the lexicon which will be used in constructing the tree (with an index for how many instances of the element should be used, since some elements may occur more than once in a given derivation) – and concatenates them either with one another, or with an already built-up partial tree. Move performs the same concatenation operation over a portion of the built-up tree and that tree's own root (or highest) node.

As an element is taken from the numeration, its index is reduced by one. The derivation must exhaust the numeration (i.e., reduce all indices to 0) to "converge" (a nonconvergent derivation is said to "crash"). The derivation represents a mapping from the numeration, on the one hand, to the conceptual-intentional interface (formerly known as LF, a label I will continue to use here) and, on the other, to the articulatory-perceptual interface (formerly known as PF, a label I will also continue to use here). Each of these interfaces imposes "bare output conditions" that must be satisfied by the representations they receive if the derivation is to converge. It is widely assumed that the conceptual-intentional interface is universal (since it is hard to imagine how an acquirer could learn anything about it

V features on T should not engender a crash, since it does not do so in the "French" case, and the bare output conditions at PF are assumed to be universal. Late *Spell-Out*, however, violates a condition on economy of derivation that requires that SPELL-OUT take place at the earliest licit point in the derivation (i.e., movement must be covert, if it can be) if the derivation is to converge. Under minimalist assumptions all cross-linguistic syntactic variation is to be accounted for by differences in the point at which SPELL-OUT occurs in a derivation – the differences themselves being a function of the "strong" or "weak" nature of the features on functional heads.

What is the relevance of the sketch above for issues in diachronic syntax? Clark and Roberts (Clark & Roberts 1993, Roberts 1997) have posited a "learning strategy" superficially similar to "economy of derivation" to account for certain syntactic changes in the history of English and of some varieties of French.[6] In providing an account for the loss of verb-second phenomena (such as exist in Modern German) in Middle French they argue (1993: 335) that, when faced with a highly ambiguous PLD, an acquirer will opt for a grammar with "covert" verb movement over one with "overt" verb movement because "learners follow a least effort strategy in that they try to assign the simplest possible parse to the input string." Note, however, that "parsing" a string presumably entails establishing that the representation that one has assigned to that string is the SPELL-OUT of a convergent derivation. That is, the parse requires that the acquirer (1) posit a numeration, (2) establish that the numeration can be "run through" the universal computational syntax system so as to converge at LF, and (3) posit the appropriate features on the functional heads so as to allow convergence at PF. It is difficult to see how a parse of a derivation, thus defined, that undergoes SPELL-OUT before verb movement, for example, could be any "simpler" than a parse of a derivation that undergoes SPELL-OUT after verb movement. En route to LF, precisely the same number and type of "nodes, traces or chain positions" (each of which the authors claim may be relevant to the question) will be created by the derivation. The derivation requires the same number of steps – the number required to get from the numeration to LF plus the step of SPELL-OUT itself. It is difficult therefore to see how one parse could involve any "less effort" than the other.

Roberts (1997: 421) states that the simplicity metric favors "covert" movement "arguably because overt movement always creates adjunction structures, while the lack of movement may not, and adjunction structures are more complex than non-adjunction structures." But given the model sketched above, which appears to be the one assumed by Roberts, "adjunction" structures (like all syntactic structures) are generated for a given derivation *regardless* of when SPELL-OUT occurs. The "covert" vs. "overt" movement contrast is orthogonal to the issue of the "simplicity" of the derivation, in this sense of "simplicity."

[6] The authors explicitly contrast (1993: 335 n. 16) their proposed "learning strategy" with "derivational economy," stating that the former is "not a principle of grammar."

Nothing, of course, precludes stipulative definitions of "simplicity," such as Roberts's (1997: 421) statement that "[t]he simplest representation compatible with the input is chosen, where representations lacking overt movement *are defined* as simpler than those featuring movement dependencies" [emphasis mine – MRH]. Equally stipulative is Roberts's assertion (1997: 421) that "weak features represent the default (or unmarked) value" in UG. No empirical support for such a claim is offered. The claim itself appears to run counter to at least one of the many definitions of "unmarked" – statistical predominance. Note that in any event only one of the stipulations is necessary. If the UG default is "weak" V features and the PLD presents no compelling evidence to override that default, the result will be "covert" movement (without regard for "simplicity"). On the other hand, if the proposed "simplicity" metric is relevant to acquisition, there is no motivation – in fact, no evidence – for positing a UG-default "weak" feature setting.

The economy of derivation itself is not relevant to the loss of verb movement surveyed by Clark and Roberts (1993) and Roberts (1997). For a given string of the PLD to play a role in grammar construction the acquirer must be able to parse the string by positing a convergent derivation that will generate it. The competing parses for a string that is ambiguous with respect to verb movement will therefore necessarily involve assuming, for the French-type case, that the numeration must have contained a C with strong V features (under the "verb-second" hypothesis) or must have contained a C with weak V-features (under the non-verb-second hypothesis). Establishing the strength of the features on the lexical item C is *the same task* as determining the point at which SPELL-OUT has applied. As Chomsky has pointed out (1995: 227), economy of derivation is relevant only to the evaluation of derivations involving the same numeration. It cannot, therefore, be invoked to choose between these two competing hypotheses, since they involve different numerations.

If the computational component of the syntactic module of human grammar is universal and invariant then it does not change. To the extent we consider the workings of this module to be what "syntax" is, there is, then, no "syntactic change" at all. Variation is limited to the lexicon and what has traditionally been considered "syntactic change" is to be taken instead as representing a change in lexical features (in particular, those lexical features that are of syntactic relevance). This fact presents an interesting dilemma. In the domain of phonology, as I have argued in some detail earlier in this book, direct change in the lexicon usually implies that the change is "sporadic," i.e., not "regular" in the Neogrammarian sense. Such changes, being lexical, affect only a single lexical item and thus do not receive the attention that systematic changes do. If direct change in lexical items affects only one lexeme at a time, which seems likely *a priori*, one might assume that all "syntactic change" should be "sporadic" in this manner.

In spite of the fact that, as we have seen from the preceding discussion, it is unlikely that, in the technical sense, syntactic change *per se* exists, I would like to argue that there is a class of diachronic phenomena for which it might be useful to continue to the use the term "syntactic change." The phenomena in question represent what I call "regular syntactic change," with an intentional implied parallel to the process of regular, "Neogrammarian," phonological change.

We have discussed in the phonological change chapters the fact that historical linguists do not typically include just *any* instance of the change in phonological specification for a given lexeme as a case of phonological change. To repeat an earlier example, in the three instances of change given in (8.5–8.7), we find a change in the phonological information in the lexicon for a given lexeme.

(8.5) Early Modern English *brought* > Modern Ypsilanti[7] English *brung*

(8.6) Middle English *lutter* > Modern English *pure*

(8.7) My grandmother's English *davenport* > my English *couch*

In the first instance, the form *brung* is not due to the normal phonological development of final *-ɔt* in Ypsilanti (e.g., *thought* and *caught* have not become ***thung* and ***cung*), and thus does not represent a regular sound change. Indeed, it seems unproductive to label the change "phonological" in nature at all – clearly no *coherent* restrictive theory of possible phonological change will result if we license such events. Historical linguists, both traditional and theoretical, thus treat such changes as instances of "morphological change." In the second instance we are confronted by one of many words borrowed from "French" into "English." Again, "borrowing" has never been counted as an instance of "phonological change," and again, given the unconstrained nature of the process, no coherent theory of phonological change could be developed if it were. Finally, the third case represents a straightforward instance of "lexical replacement" or "lexical death" – I did not, as an acquirer, misparse *davenport* as *couch*.

Such cases contrast sharply with, e.g., the vocalization of coda *l* in Cockney English, as seen in [fiʊ̯] 'feel' (vs. [filɪn] 'feeling'). The vocalization of *l* in this environment is completely regular (there are no exceptions). It is not due to a morphological change, it is not borrowed from elsewhere, and there is clearly a direct connection between the ancestral form of [fiʊ̯], which was [fil], and the Cockney form (unlike 'davenport' and 'couch'). This regularity is clearly related to the existence in Cockney of a phonological rule or constraint which is responsible for the change of underlying /l/ to [ʊ̯]. It represents regular sound change because it is imbedded in the computational system of the phonology of the relevant grammars.

The contrast between what might be termed a "lexical" change, the direct modification of the phonological specification of a given lexeme (as in 8.5–8.7 above), and a phonological change, a change in the computational system of the phonology of the acquirer, cannot, however, be directly transferred into the syntactic domain if, as has been argued above, the computational system of the syntactic component is invariant and universal. Nevertheless, it does appear that

[7] The author's hometown, in Michigan.

included in the phenomena discussed under the heading "syntactic change" are both lexeme-specific, idiosyncratic developments (much like the phonological examples in 8.5–8.7) and very general, across-the-board changes.

For example, in its earliest attestations the verb 'behoove' was a transitive verb taking a genitive object as can be seen from the genitive *s*'s on *mycles læcedomes* in the sentence below:

(8.8) *Mycel wund behófaþ mycles læcedomes.*
great injury requires great cure

"a great injury requires a great cure" (K. ÆLFRED, *Bæda* IV.v, *c.*980)

It is not a general feature of Old English transitive verbs (even those governing the genitive) that they are now impersonal, as 'behoove' is. It appears that the development of 'behoove' is simply a lexical fact about the feature specifications of a single lexical item, much like the change of the past tense of 'bring' from 'brought' to 'brung.'

This differs quite clearly with, e.g., the loss of verb second (or V movement to C in simple noninversion context main clauses) in a number of languages. There is no evidence that, for many languages, the loss of V2 had anything whatsoever to do with individual verbs, affecting one verb before another, for example. Instead, it appears that for these languages they either have V2 or do not.[8] If they have V2, all relevant (by syntactic environment) verbs are affected. If they do not, none are. The same appears to be true of the coming into being of verb movement. For example, the Indo-European ancestor of Germanic did not have verb movement in interrogatives.[9] Yet in no Germanic language is it a lexical matter which verbs or auxiliaries undergo I-to-C movement in the formation of interrogatives: if a verb is in I at the relevant point in the derivation, it will move to C. The change whereby such structures come into being is thus "regular" in the relevant sense.

Changes, such as those involved in the 'behoove' case, are often, though not invariably, to be linked up with changes in the "lexical semantics" of the verb, while changes such as the coming into being or loss of V2 seem quite unrelated to lexical semantics. I would therefore propose treating the 'behoove' cases as "lexical change," rather than "syntactic change," much as the instances of change in (8.5–8.7) are excluded from the domain of "phonological change." It appears that what distinguishes the two types of change is what type of lexical item is involved in the change. When the change involves the features on a major lexical class, but leaves the features of the "functional" heads unaffected, we have a case of "lexical change." However, since the functional heads – the Agr's, T, C – are used in the construction of *every* sentence, when these elements change their

[8] This contrasts interestingly with the facts surrounding the rise of *do* support within the history of English, as we shall see below.

[9] It is absent from all of the archaic, early-attested Indo-European languages: Sanskrit, Avestan, Hittite, Greek, Latin, etc.

features, the surface structure of every clause will be affected, regardless of what major category lexemes are found in it.

While we must bear in mind that, strictly speaking, "syntactic change" as such does not exist – all apparent changes in syntax are linked up with features on lexical items – it seems to me useful to still retain the term "syntactic change" for just these cases of changes in the features of functional heads. The mechanism which gives rise to such changes is going to be fundamentally distinct from that behind "lexical (syntactic) changes," being normally *semantically inert*. If a language goes from having *in situ* interrogatives to having WH movement, the semantics of interrogation have not changed, merely its syntactic expression.

This distinction is critical because it does not appear likely that we will at any point in the near future be able to develop an even minimally constrained theory of semantic change (and thus of the lexical changes of the 'behoove' type). A simple examination of a few cases of changes in lexical semantics reveals the problem: French *crétin* is from Latin *christianus* 'Christian,' English 'bead' used to mean 'prayer' and Rennellese *tou ta'e* is used as the first person singular humilitive pronoun – it is etymologically 'your excrement' (Elbert 1988: 63 *et passim*). It is difficult to conceive of a theory of change in lexical semantics which would permit these attested changes but still be sufficiently restrictive to be of real empirical value. If the change in 'behoove' (and other such lexeme-specific changes) is the ultimate result of this kind of semantic misanalysis, it seems unlikely that it will be amenable to coherent study at this time.

By contrast, changes in the feature specification of functional heads do not involve, at least in the normal case, semantics. They must therefore represent instead structural misparsing on the part of the acquirer – a mechanism which, given a sufficiently restrictive theory of syntax and syntactic acquisition, should allow for coherent investigation. At any rate, it must be recognized that there is a fundamental difference in the underlying dynamic in the case of the two types of change. It will therefore be critical to distinguish between them, if we are to develop a coherent theory of either.

In fact, it would appear that changes of the "lexical" type also may in many cases involve structural misparsing as well as semantic reanalysis. We need not delve into the chicken–egg issue arising in these cases as to which change is driving the reanalysis and which is just coming along for the ride, as it were. It is unlikely we would be able to answer that question without a coherent theory of semantic change in any event. It should be clear, however, that uncovering the mechanism whereby the acquirer comes to a determination that virtually *all* of the clauses in his/her PLD have structure Y (when in fact they have, for the acquirer's source speaker, structure X) – as is required in the case of changes in the featural specification of functional heads – is likely to provide us with valuable evidence about the nature of structural misparsing which the more local, lexeme-specific, possibly semantics-dependent cases cannot. In my opinion, we should focus on the purely structural type, bringing knowledge gained in this domain to bear on the more subtle problem of the "lexical" change cases.

> one may wonder how children determine the syntactic properties of infinitives: infinitives themselves occur characteristically in embedded clauses, generally in their D-structure position and not in INFL. An infinitive in that position is therefore not accessible to a degree-0 learner. *Presumably children can learn that infinitives exist as an uninflected morphological category from "citation forms" and unembedded instances* . . . But how do children learn the distibution of infinitives . . . ? [emphasis added – MRH]

The highlighted sentence in this quotation invites the inference that being "accessible" for morphological and lexical analysis (rather than simply for purposes of parameter-setting) is what is at issue in the degree-0+ proposal.[16] However, as we saw in the "nursery rhyme" discussion, cited briefly above, children can and do parse material more deeply embedded than the deepest level to which the degree-0+ proposal gives them parameter-setting access.[17] Lightfoot hints that we may be dealing in such cases with "some auxiliary system whereby people make sense of unfamiliar phenomena, perhaps through 'marked' or 'peripheral' operations" (1991: 93).

It was, I believe, a fairly standard position in GB theory that there exists, in addition to the "core grammar" provided by UG, a set of language-specific "peripheral" structures or elements which in part constitute the grammatical competence of a given speaker. See, for example, Chomsky and Lasnik (1993: 8). The constraints on the powers of the "periphery," as well as studies of the principles which govern its construction, remain to the best of my knowledge uninvestigated. It seems clear, however, that the "core" grammar consists of those aspects of UG which are not parametrized (and thus are "given") together with those structures which follow from the setting of the parameter values for those aspects of UG which are parameterized. The most constrained theory of the

[16] No one, to the best of my knowledge, has ever proposed that having uninflected infinitives is a parametrized aspect of UG, nor can I conceive of such a proposal gaining wide acceptance.

[17] I am assuming that parsing is a necessary prerequisite for what Lightfoot labels "understanding" in the nursery-rhyme quote. Alternatively, one could believe that children invoke ancillary, nonlinguistic cognitive strategies to interpret what is, for the purposes of their grammatical competence, merely a string of lexemes with no internal syntactic organization (rather like what one uncovers upon asking adult speakers of Modern English varieties to interpret *the the dog man bit*). Lightfoot invokes the presence of "partially analyzed structures" as a step towards acquisition, considering it in fact (1991: x) "one of the claims of the book" that "the triggering experience consists not of raw data but of partially analyzed structures" (what would it mean for the learner to use "raw data" as a triggering experience?); but what such partial analysis consists of, how it is constrained, how, in particular, it could be used to generate a meaningful analysis and understanding of *this is the cow that kicked the dog that bit the rat that ate the cheese that lay in the house that Jack built* WITHOUT positing hierarchical syntactic structure, is not addressed.

"periphery" would seem to me to be one which took "peripheral" grammar structures to be due to lexical "overrides" of parametrized UG values.[18] Thus, for example, one could imagine that in a language such as Sanskrit, in which we find both prepositions and postpositions, the head parameter (or a "direction of government" parameter, or whatever) is set for some value, let us say in this case for head finality, and that therefore postpositional phrases can be freely projected from the lexicon with no special operations. Prepositions, however, would contain subcategorization information indicating that they precede their complements, projecting onto D structure as apparent head-initial structures. As Sam Epstein (p.c.) points out to me, such a system, although more highly constrained than any yet proposed, may nevertheless still pose learnability problems of such magnitude that they cannot be transcended.

In any event, the acquisition of these "peripheral" grammatical operations, should they exist, is independent of the parameter-setting operation, and, although completely uninvestigated, it is apparently (to judge from Lightfoot's statement above) not restricted by the degree-0+ constraint. Invocation of the notion of the "periphery" is somewhat unsatisfying in the present context, since it challenges our ability to distinguish structural changes which are to be attributed to changes in parameter setting from those which could rather be due to modification of the "peripheral" grammar. This makes empirical evaluation of the degree-0+ learnability hypothesis that much more difficult.

In addition to the complications for the hypothesis of degree-0+ learnability that are presented by the potential lack of a typologically valid corpus of diachronic reparametrizations and the empirical difficulties arising from the relatively unconstrained option of invoking "peripheral" grammar to explain counter-evidence, there are several other proposals in the book which serve to isolate the hypothesis of degree-0+ learnability from empirical evaluation. For example, in his efforts to find "robust" evidence for underlying verb finality from unembedded domains in Old English, Lightfoot invokes (1991: 62) sentences such as:[19]

(8.9)	*Swa*	*sceal*	*geong*	*guma*	*gode*	*gewyrcean.*
	thus	shall	young	man	good things	perform

"Thus shall a young man perform good things" (Beowulf 20)

noting that "even if adult grammars in the Old English period did not have a monoclausal analysis of sentences like (23) [the Beowulf sentences in 8.9 – MRH], it is likely that children's grammars did, and that two-year-olds did not perceive or analyze the modal verb." Lightfoot cites Klein (1974) in this regard, stating (1991: 53) that he

[18] Under minimalist assumptions, of course, this is quite clear.

[19] I have corrected Lightfoot's *gewyrecean*, a typo, as well has his translation – he mistakenly translates the sentence as if the subject and finite verb were plural.

> gives good reasons to believe that Dutch children pay much attention to such structures from an early age. Questions then arise about the nature of the structures assigned to such expressions by young children; for example, Klein suggests that an unstressed "auxiliary" is in some sense not perceived and thus plays no role in the analysis of sentences.

There is much laxness in the formulation of this argument: it is not clear what the intended force of "pay much attenton to"[20] and "in some sense not perceived" is. More importantly, however, Lightfoot is proposing in this discussion that it is not degree-0+ domains as defined over the relevant structures in the adult grammar's output (i.e., the output of the grammar the child is acquiring), but rather such domains defined in terms of the structures posited by the child at the time the parameter is being set. In this case, in particular, Lightfoot is claiming that it was crucial to the regular acquisition of the grammar of Old English that children misparse structure involving "premodals" such as *sceal* (only to eventually parse them correctly, one assumes) and set their parameters according to this misanalysis. Since the nature of the historical record precludes direct access to children's grammars of Old English during acquisition, Lightfoot is right to look for typological parallels from contemporary acquisition studies. However, studies such as Klein (1974) tell us little about the crucial issue for degree-0+ learnability – the role of structures involving "auxiliary"-like elements in *parameter-setting*. Given that fact, invoking the grammar of the child serves only to remove the hypothesis of degree-0+ learnability from straightforward empirical evaluation.

Turning from these general methodological issues to the specific issue of parameter-setting and diachronic syntax, Lightfoot (1991) conveniently summarizes in his final chapter the major changes treated in the book. He labels these changes "six new parameter settings in the history of English." The six cases he has in mind are (1991: 166–7):

(1) "the new verb-complement order at D structure"
(2) "the ability of the infinitival *to* marker to transmit case-marking and head-government properties of the governing verb"
(3) "the loss of the inherent D structure oblique case"
(4) "the emergence of a reanalysis operation"
(5) "the recategorization of the premodal verbs"
(6) "the loss of the ability of verbs to move to a governing INFL position."

There is a lot of technical terminology here and it would go well beyond the scope of this book to explain in detail what the changes Lightfoot is referring to actually are. It is sufficient, for my purposes here, to point out that none of these

[20] Given that Lightfoot asserts elsewhere, as we saw above, that children understand deeply embedded material without using that material to set parameters, would we expect them, with this understanding, not to "pay attention" to such material? Here too Lightfoot seems somewhat unclear about the nature of the hypothesis under evaluation.

is a "parameter" as such. Indeed, in none of the six cases cited above is Lightfoot explicit about what the parameter is that changed. This is not a trivial omission; parameters are taken by Lightfoot, as by others working within this framework, to be quite abstract, formal, and general. A precise statement of the parameter involved in each case would allow the reader to deduce what further implications follow from a change in the setting for that parameter. The changes listed in (1)–(6) above are far too specific to be UG "parameters" themselves.

Moreover, it seems clear that in at least one of these cases a change in parameter-setting is in fact not involved. The "recategorization" of the premodal verbs, (5) above, is discussed in some detail by Lightfoot, building upon his own 1979 treatment, amended in the light of criticisms by authors such as Warner (1983) about the chronology of the relevant events. He now sees the emergence of modal verbs in English as being due to two distinct changes: the "recategorization" of the premodal verbs as INFL (i.e., auxiliary) elements and the "loss of the ability of verbs to move to . . . INFL" (his change (6) above), as they do in French, for example. The first of these is described as follows (1991: 147):

> The first change, whereby the premodal verbs came to be classified as instances of INFL and to be generated under INFL, was a change in lexical specifications and therefore may have affected some items earlier than others.

Lightfoot observes that morphological changes are likely to have been the driving force behind the recategorization. In discussing the change in more detail, he notes that:

> these changes had the effect, in many ways accidental, of making the premodals into a small and distinctive class. In fact, the class was so small that it must have looked to language learners like a closed class, consisting of items which were . . . not special kinds of verbs.

It is difficult to see the change described in these quotes, whereby modals went from being a class of verbs (and thus heading VP projections) to being auxiliary elements (and thus base-generated in I) as a change in the setting of some binary parameter. The change involves a reanalysis through which a class of verbs comes to be categorized grammatically as a different syntactic class. Before the change, when these "premodals" were still verbs, they exhibited syntactic behavior appropriate to verbs. After the change, when these elements had become "modals," they ceased to head VPs (since they were no longer verbs) and were instead base-generated in a position in the tree structure appropriate to their new lexical status. Since there were no "modals" in the Old English *lexicon*, it seems unnecessary to assume that some parameter setting is responsible for their absence in Old English *syntax*. Is their absence in the lexicon to be attributed to a parameter setting? I do not see any reason to constrain via parameter setting the inventory of lexical classes permitted in any particular language (though, of course, UG limits the inventory of such items to those permitted by human linguistic systems). The set of actually occurring classes in a given language can be readily determined on the basis of the morphosyntactic features provided by UG and the limited positive evidence provided by the PLD, without recourse to any "hints" offered by a parameter of UG. Unfortunately, these and other related issues are never directly addressed by Lightfoot.

Lightfoot goes on to note that "each of these new parameter settings has some distinctive characteristics," which he proceeds to enumerate (1991: 167–9):

(1) "each new parameter setting is manifested by a cluster of simultaneous surface changes"
(2) "new parameter settings . . . also sometimes set off chain reactions"
(3) "changes involving new parameter settings tend to take place more rapidly than other changes"
(4) "obsolescence manifests new parameter settings"
(5) "any significant change in meaning is generally a by-product of a new parameter setting"
(6) "a further defining property of new parameter settings . . . is that they occur in response to shifts in unembedded data only."

The first thing to note about these "defining properties" of parametric changes is that several of them do not do a particularly good job of "defining." For example, (2) states that new parameter settings "sometimes" set off chain reactions, implying that they sometimes do not. In addition, since Lightfoot permits some chronological flexibility in the interpretation of the "simultaneous" of (1), the contrast between (1) and (2) is, in practice, difficult to maintain.[21]

The third "defining property" is also seriously weakened in empirical content by a hedge – there is no real way to test in any given case for the presence of a "tendency" to take place more rapidly than other changes, even if we had a meaningful metric for how rapid "more rapid" might actually be.

The fourth item in the list above seems rather to define "obsolescence" than to be a distinctive characteristic of new parameter settings.[22] Similarly, the fifth

[21] Note that the actual example of a "chain reaction" cited by Lightfoot (1991: 167) – the relationship between the new verb-complement order at D structure and the "transmitter" status of *to* – in fact must involve descriptive simultaneity: "a child with the new verb-complement setting is forced by the constraints of Universal Grammar to analyze expressions like *I ordered the grass cut+infin* differently from the way they were analyzed in earlier generations." The author's use of "forced" entails that there will be no grammar with verb-complement order at D structure and the earlier analysis of these infinitivals, thus no "middle link" in the chain.

[22] It is not possible that all instances of obsolescence manifest new parameter settings. To see this one need only think of simple cases of lexical obsolescence (the loss of the words *davenport*, *groovy*, and *felly* in the relevant senses in American English, for example).

"defining property" is a statement more about "significant change in meaning" than a distinctive aspect of new parameter settings. It is also hedged (with "generally") and it is not at all clear how to interpret "significant" in a noncircular way in this context. As a bald assertion it is pretty clearly false: *bead* has changed its meaning from 'prayer' to 'bead' during the course of the history of English – this seems significant, but presumably has nothing to do with new parameter settings.

The sixth defining property represents one of the major tenets of Lightfoot's approach. As noted above, most historical linguists are likely to desire a larger corpus of clear instances of changes in parameter-setting from a typologically diverse set of languages, and some clarification of the "degree-0+ learnability" issues raised above, before accepting this as a demonstrated property of parametric change.

To what extent do the six changes which the author attributes to changes in parameter-setting actually show the six proposed defining properties? Given the difficulties involved in determining precisely which parameters, if any, changed in each instance, and the conceptual difficulties with some of the "defining properties," it is not easy to answer this question. The recategorization of the premodals seems not to have been as rapid or as simultaneous as "defining property" (1) or (3) might demand, for example. However, I have hinted above that it is unlikely that this was a change in parameter-setting at all. Note that, if one accepts that the recategorization was not due to a parameter-setting change, then the fact that it may well show "defining properties" (2), (4), (5), and perhaps even (6), indicates that the listed properties are also found in other, nonparametric, changes, which clearly further weakens their status as "defining" properties of parametric change.

There is one final issue which runs through Lightfoot's discussion of parametric change – an issue which I have tried to clarify in the early chapters of this book. The issue will be discussed in detail when we turn to a consideration of "variationist" approaches to syntactic change in the next section, but it is worthwhile making it clear that it arises in work in a wide variety of theoretical frameworks. I cited some of the following passage from Lightfoot (1991: 111) in the discussion in chapter 1, but we can now see it within the context of Lightfoot's work somewhat more clearly, I think:

> In studying syntactic change, one is bound to one's texts for data about early stages of some language; one should never discount data, but one must interpret the texts with some philological skill. Taking "a single example from the thirteenth century" as evidence that such sentences were grammatical for all speakers of the language may allow one to suppose that there was no change in grammars and therefore nothing to explain, but this does not strike me as sensible. If, in fifteenth-century texts, there is a significant increase in certain sentence types, rising from near zero . . . , it is reasonable to suppose that there was a change in many individual grammars, i.e., that many individuals began to set some parameter of Universal Grammar differently from many of their forebears. One would therefore want to explain why that parameter was set differently and precisely what the parameter was. That is the approach I take.

This passage reveals quite clearly a confusion of the contrast which I attempted to draw earlier between change, to be attributed to reanalysis on the part of the grammar constructor, and diffusion, driven by sociolinguistic factors of the well-known type. The "single occurrence from the thirteenth century" must, like all linguistic data, be evaluated for authenticity as valid linguistic data using established philological methods; however, once its status as linguistically real is no longer in doubt, it reveals that the reanalysis, i.e., the resetting of the parameter in question, had taken place for some speaker(s) by that time. It is clear that subsequently this change diffused, presumably via the known sociolinguistic factors which determine diffusion, from the locus of innovation to the area responsible for the bulk of our Middle English prose texts; but that diffusion is not a case of *change*. Change takes place when the learner constructs a grammar that differs in some way from the input grammar(s) to which the learner is exposed. Sociolinguistic diffusion is the result of a selection process through which one or more of the variety of input grammars to which the learner is exposed comes to be acquired and put to primary use. Only *change* is of any interest for the study of the questions which Lightfoot is investigating. Diffusion represents transmission of the innovating grammar with its parameter settings intact. Contrary to the implications of the above quotation from Lightfoot, the number of speakers who manifest a particular change in parameter setting is of no significance whatsoever. Even if only one speaker ever showed the change, i.e., even in the absence of any diffusion beyond the innovator, that change could provide us with crucial evidence regarding reanalysis and parameter setting.[23] Indeed, all instances of the relevant change demonstrably subsequent to its innovation are suspect – they may be more properly attributed to diffusion than to direct parametric reanalysis of some input grammar's output.

Perhaps it is not surprising that the essential problems with parameter-setting approaches to syntactic change are virtually the same as those which plagued the Principles and Parameters approach generally: no plausible parameters appeared to be proposed, although the concept was regularly invoked to discuss particular sets of data.[24] When we mix a lack of clarity about what the parameters involved

[23] For trivial changes, it is always possible that there were multiple points of (very roughly speaking) simultaneous innovation. Diffusion of a trivial change from several loci of innovation, as opposed to diffusion of a nontrivial change from a single source, is readily revealed on the pages of any dialect atlas.

[24] This is particularly clear if we remember that parameters, like other aspects of UG, were to be posited on the basis of the fact that certain aspects of the mature linguistic knowledge of speakers could not be explained by positing learning over the type of nonnegative, degenerate data presented in the PLD. If we think about some parameters that were widely discussed – the null subject parameter, e.g., or the "headedness" parameter – it seems clear that they were posited on the basis of observed linguistic variation, not on the basis of learnability challenges arising from that variation. How difficult is it, compared to the many tasks acquirers succeed at, to determine that your language has phonologically null subjects, or, once you have acquired some lexical items, that heads precede or follow their complements?

in specific syntactic changes actually were with confusion about matters such as "change" vs. "diffusion," the resulting research program, not surprisingly, falters.

8.3 "Lies, Damn Lies, and Statistics": Some Models of Variation and Change

Lightfoot is hardly alone in treating syntactic change and syntactic diffusion as undifferentiated. In several recent approaches, explicit models have been constructed which either explicitly assert or unambiguously assume that the two are in fact most productively unified. While a comprehensive analysis of these approaches lies outside the scope of our investigations here, the work has become so influential, particularly in the diachronic syntax camp, that it is necessary that we address it now. We will survey two approaches – the so-called "variationist" approach of Tony Kroch and his students and colleagues and the "Stochastic Optimality Theory" approach of Joan Bresnan and her students and colleagues, beginning with the somewhat more mature, and considerably more influential, models of the variationists.

8.3.1 Variationist approaches to syntactic change

An influential and quite extensive body of work on syntactic change in what has been termed the "variationist" framework has been developed, principally by Tony Kroch and his students and colleagues. I have taken my section title from Susan Pintzuk's summary of this work in the *Handbook of Historical Linguistics* (Pintzuk 2003). Unfortunately, I find the terminology used within this framework somewhat difficult to follow, particularly on matters such as the definition of key concepts, so my survey will not be as extensive as I would like. In any event, the basic idea seems to be that the path whereby some syntactic construction A is replaced by some innovative isofunctional syntactic construction B involves a period of variation between A and B, that variation occurring at a different rate in a variety of syntactic contexts, but progressing at the same rate over time in all contexts (the so-called "Constant Rate Effect"). Leveraging the concept of "morphological blocking," which precludes isofunctional morphological doublets in a grammar, and expanding that notion into the syntactic domain, Kroch and his colleagues have argued that it must be the case, when we find instances of, e.g., *do* support next to the failure of *do* support (in the same syntactic context) in a single time period (often in a single author), that we are dealing with an instance of "grammar competition." This would seem to follow clearly enough: "Did John go to London?" and "Went John to London?" do not appear to differ semantically, so if we take the ban on "absolute synonymy" in syntax (as in morphology) seriously, these two strings must be being generated by two different grammars.

There are a number of statements which make this relatively lucid position difficult to interpret in detail. I will consider first some issues surrounding the use of "the blocking principle," then, in somewhat greater detail, some aspects of the "Constant Rate Effect" which I find troubling.

Pintzuk (2003: 535) writes the following on the question of where precisely syntactic doublets come from, given the blocking principle:

> [Kroch] suggests that in syntax, as in morphology, doublets that are semantically and functionally non-distinct are disallowed; and that doublets of this type, which may arise through language contact . . . , compete in usage until one of the forms wins out. Sociolinguistic, psycholinguistic, and stylistic factors may have an effect on the favoring of one variant over the other.

If they are disallowed, it doesn't seem that they should be able to "arise through language contact" (or, indeed, through any other mechanism). Kroch himself says the same thing, though, as near as I can tell:

> The blocking effect, as we have seen, does not prevent doublets from arising in a language by sociolinguistic means; that is, by dialect and language contact and perhaps other processes. (Kroch 1994: 17)

One could of course say that what is being claimed is that doublets can come into being as long as one is always generated by one grammar, the other always generated by another grammar. It's somewhat odd to express this by saying that the blocking effect doesn't preclude such a thing, since the blocking effect is a grammar internal process. Forms in my grammar don't "block" forms in yours, e.g., nor does the existence of a word 'dog' in my grammar of English "block" the existence of a word 'Hund' in my grammar of German, even though both grammars are in my very same head. The other problem with this interpretation of the claims cited above is that it is very difficult to see what triggers the "competition" between the two forms, if one is the product of one grammar, the other the product of another. Anyone can, I think, imagine that two grammatical systems could "compete" with one another in the sense that acquirers may adopt one in preference to the other, or speakers may use one more than the other, but of course this has nothing to do with the blocking effect, and, perhaps most crucially, it *need not* happen. Multiple grammars are in use in many speech communities around the world without one of them inevitably losing out via some necessary "competition."

But competition appears to be a cornerstone of the model. Kroch (1994) writes:

> the blocking effect will also exclude variability in the feature content of syntactic heads, as the resultant variant heads would have the status of doublets. This

which, when you are about to say a negative interrogative, induce you to use G_{do} about 12 percent of the time, whereas when you are going to say an affirmative interrogative with an intransitive verb in it you are induced by these preferences to use G_{VM} almost exclusively. Precisely parallel to this would be a modern situation, let's say in Montréal, in a social context replete with bilingual speakers with "French"-type grammars and "English"-type grammars, such that a speaker could use either one. One could then posit some algorithm which assesses "psycholinguistic and information processing preferences" over the outputs of the two grammars and which would regulate whether I would use my "French"-type grammar or my "English"-type grammar, when asking a particular type of question, for instance. This strikes me as completely implausible.

Interestingly, by 1575 people producing Kroch's data, with presumably the same kinds of minds, and thus the same kinds of "psycholinguistic and information processing preferences" as their ancestors in 1425, now about to say a negative interrogative, are induced to use G_{do} almost all the time (85.4 percent), and in the originally disfavored context, that of the affirmative intransitive interrogatives, where in 1425 their linguistic ancestors avoided almost completely the use of G_{do}, they use that grammar 42 percent of the time. What happened to the differential influence of the "psycholinguistic and information processing preferences"? How do these preferences work so strongly to impede the use of *do* support early in its development, but fail so seriously to do so later? Wouldn't the expected development be simply that in contexts which favored *do* support, speakers would come to use G_{do} *all the time*, and in contexts which disfavored it, to use G_{VM} *all the time*? That would optimize the apparently important psycholinguistic and information processing preferences.

To understand these issues in more detail it would be useful to look at the categories used to formulate the Constant Rate Hypothesis and this particular interpretation of the nature of "Grammar Competition." They turn out, however, under scrutiny to be a little peculiar. Before we consider the data in detail, however, let me just remind the reader that the data are complex even in modern "English"-type grammars. *Do* support is triggered in interrogative and negative contexts for all main verbs except for main-verb uses of "to be" (and, in many varieties, "to have"). In negative imperatives, *do* support is found for main-verb uses of "to be" and "to have" as well. A truly satisfying account of this rather peculiar distribution, which is clearly related to verb movement to I (as it was in the time period being discussed by Kroch), remains beyond our grasp, but it is worth noting the *lexical* nature of the distribution with nonauxiliaries. Apparently nonauxiliary "to be" (and, in the relevant dialects, nonauxiliary "to have") bear some idiosyncratic feature which allows them to continue to undergo V movement, and thus avoid *do* support.

There is evidence that the "lexical" nature of the development was also true for English in the period of time under study by Kroch in his discussion of the rise of *do* support. For example, for all of the negative contexts, Ellegård excludes from his statistics (as does Kroch, without, however, noting the fact) all

Table 8.1: Use of *do* support with some individual verbs in negative contexts

Verb	do *support*	*no* do *support*	*% do*
come	1	59	2
speak	3	39	7
go	6	26	19
see	14	36	28
hear	8	17	32
understand	14	27	34
mean	10	19	35
say	6	11	35
like	12	11	52
think	36	27	57

occurrences of the verbs *know, boot, trow, care, doubt, mistake, fear, skill*, and *list*.[29] This is because they simply resist usage with periphrastic *do* far more strongly, and for far longer, than do other verbs. In terms envisioned in the Kroch scenario, this would entail that speakers, about to utter a negative sentence containing the verb "fear," avoid using G_{do} quite assiduously, throughout the period studied by Kroch. It's hard to see how that could be attributed to a psycholinguistic or information processing preference, though the phrase is sufficiently vague that I of course can't exclude it. Again, the parallel with cases in which we know there are two grammars "in competition" (i.e., present) in the mind of a single speaker is instructive. Do we really believe that our French–English bilingual in Montréal would always use his or her "French"-type grammar when s/he wants to use a predicate which means "know"?

Indeed, as Ellegård notes (1953: 199 n. 1) the classification of verbs into his *know* group (the set I detailed above) and his *main* group (all other nonauxiliary verbs) is "somewhat arbitrary." He goes on to note that "It would be possible (theoretically at least) to plot a curve showing the relative frequency of the do-form with all the different verbs . . . The extreme portion of such a curve would include the know-group. It is desirable to eliminate this extreme portion: exactly where the cut is to be made is arbitrary." He then goes on to give a table of some *main*-class verbs and their different frequencies of co-occurrence with *do* support, which I repeat here as table 8.1 (it covers the period 1550–1700, within which *do* support has become quite common in negative sentences).

[29] It is not without interest that he does *not* exclude uses of these verbs in affirmative *do* support contexts, such as the various interrogative categories surveyed.

Indeed, Ellegård draws the following conclusion from his survey of this (and related) data: "[i]t is in fact not unlikely that each verb has its own history." This would seem to offer strong support for a conception of syntactic change as a change in the features of individual lexical items. But in many ways it wreaks havoc with Kroch's statistical approach. If Ellegård is right, a text which discusses 'knowing,' 'fearing,' and 'coming' more than 'meaning,' 'liking' and 'thinking' will show much less *do* support by virtue of that single, *lexically* driven (rather than "grammar competition"-driven) factor. The story of the rise of *do*-support is, indeed, a "very complex affair," as Ellegård notes. It is not clear to me that the "variationist" approach to the problem has moved us forward.

To conclude, perhaps even more strikingly, I feel compelled to point out that there *is* no theory of "syntactic change" in the literature which could be considered the "variationist approach to syntactic change." The statistical work done within this framework charts the diffusion of innovations once they have come into being, and thus can "compete," but has nothing to say about where the new forms come from or how they might arise in the first instance. I personally would be shocked if the spread of syntactic innovations, once they come into existence, shows a profile which differs in any fundamental way from the spread of other grammatical features once they come into existence as innovations: they move from prestige speaker to wanna-be prestige speaker through the same sociolinguistic mechanism as has been uncovered in hundreds of studies in that field since Labov's New York Department Store research. Linguistic variables when studied in this way in their sociological context can provide a fascinating glimpse into the structure of the society under study, about the hidden motives and values of speakers, and about a wealth of other engaging social issues, but they cannot help us develop a constrained theory of syntactic *change* (as opposed to diffusion), about which they have simply nothing to say.[30]

8.3.2 Stochastic Optimality Theory and morphosyntactic variation

Athough formalized somewhat more explicitly than Kroch et al.'s "variationist" approach to syntactic change, Joan Bresnan and her colleagues working in the domain of Stochastic Optimality Theory ("Stochastic OT") have developed a model of morphosyntactic variation (trivially extendable into a model of change) which shares many of the underlying assumptions we have treated in the last

[30] And I note with sadness that the fascinating work undertaken in the early days of sociolinguistics, which provided us with a glimpse into the hidden social structures around us, has been replaced, in the "variationist approach to syntactic change" literature, by statistics ripped from their social context, without regard for any of the subtleties we find in the early sociolinguistic literature, and none of the compelling sociological tales which could be developed under earlier approaches.

	Standard		Kent		Kent variable	
	SG	PL	SG	PL	SG	PL
1	am	are	are	are	am, are	are
2	are	are	are	(are)	are	(are)
3	is	are	is	are	is	are

Figure 8.7: Some English 'be' conjugations (present, noninverted)

section. It is worth our while to consider the issues arising from this very new approach now.

I will not predominantly concern myself with the specific analyses presented in the Stochastic OT literature on morphosyntactic variation, but rather on the theoretical underpinnings of the framework itself. Particularly interesting in this regard is the analysis of some forms of English dialectal 'be' presented in Bresnan and Deo (2001). The general framework developed by these authors can be explored without fully considering the range of English dialect data they treat in their paper – we will focus here for expository purposes on their treatment of Standard English, the dialect they call "Kent," and the one they call "Kent Variable." I will sketch only most perfunctorily the structure of the OT model itself, assuming that by this time most linguists have enjoyed some exposure to the basics of the model. In any event, it is not the machinery of the model that will concern us here, but rather the interpretation of that machinery – in particular, we will focus on the questions of what the framework is modeling and whether that is, in fact, what we should be trying to model.

Let us first examine the data, extracted by the authors from the Survey of English Dialects (SED) materials. The forms in question are those of the present tense of the verb 'to be' in noninversion contexts. The relevant data can be seen in figure 8.7.

The forms labeled "Standard" are presumably familiar to the reader of this book. The dialect called by the authors "Kent" differs from the Standard only in the first-person singular, where we find the form *are* where the Standard has *am*. The variety of English which the authors have called "Kent variable" shows both the "Standard" and the "Kent" forms of the 1SG form of 'to be.' Although one might want to subject the precise characterization provided by Bresnan and Deo (2001) to some scrutiny, as well as their use of these particular sources of evidence, for the time being we will accept that the generalizations which form the foundation for the table above are valid and require some type of linguistic explanation. In synchronic terms, we will need to account for the existence of these various linguistic systems *qua* systems. In diachronic terms, we would like to try to understand the precise way in which these dialects differ from one another such that we can state clearly just what has, in fact, changed and, ultimately, how the relevant changes may have come about.

[1sg]	*PL	ID(N)	*SOC	ID(P)	*2	MAX(P)	*1	*3	*SG	MAX(N)
☞ am[1sg]							*		*	
art[2sg]				*!	*				*	
is[3sg]				*!				*	*	
is[sg]						*!			*	
are[pl]	*!	*				*				
are[]						*!				*
are[1pl]	*!	*					*			
are[2pl]	*!	*		*	*					
are[3pl]	*!	*		*				*		

Figure 8.8: Tableau for "Standard" variety 1sg 'am'

Bresnan and Deo (2001) present their analysis in terms of Optimality Theory, whose basic structure is quite straightforward. Following Bresnan and Deo we will ignore issues like "noninversion context" and the like and focus on the fact that we are seeking to find a mechanism to generate the ISG form of the verb 'to be.' In the framework developed and used by the authors this will involve an input form (let's say [1sg], assuming the universe of discourse to be noninverted forms of 'to be'), a set of candidate output forms (again, for simplicity, we'll limit the forms we consider to potential expressions of 'to be' actually found in English dialects), and a set of ranked constraints. The constraints perform two important tasks in the model: they can penalize output forms which ignore specified aspects of the input (or, as an alternative way of saying the same thing, favor those output forms which are faithful to the input in the relevant respect) or they may penalize output forms which violate universal markedness requirements. The ranking of these so-called Faithfulness Constraints for individual features of the input against the so-called Markedness Constraints for those features determines to what extent language-specific markedness is allowed to surface and to what extent universal markedness considerations are able to block the surfacing of particular forms. The evaluation metric is simple: as candidate output forms violate highly ranked constraints which other candidate output forms respect, they fall out of the running for "winning candidate" status. In the end, there will be a candidate which has run least afoul of the highly ranked constraints and that candidate will be the actual output form. A concrete example should make this quite clear. In figure 8.8 is given Bresnan and Deo's "Tableau" of candidates and ranked (left to right) constraints for the "Standard" variety of English. We can then walk through the evaluation process.

Let us first consider the Constraint Set, which runs across the top of the tableau.[31] The first constraint says *PL which means merely that it is marked to

[31] The form in the upper left corner, to the left of the double bar, is the relevant portion of the input representation, and not a constraint.

have contrastively plural output forms. Forms such as *are[pl]* violate this constraint in that they are specified as distinctly plural. This violation is indicated by a "*" symbol in the column under the constraint *PL. In addition, since this constraint is not violated by at least one other output candidate (in fact, it is not violated by several other candidates), the violation incurred by forms such as *are[pl]* is fatal for these output candidates, which can now never win. This fact is indicated informally by placing an "!" in the column as well. The next constraint, ID(N) is a Faithfulness constraint, favoring output forms which are faithful (stand in Identity with) the Number (N) specification of the input candidate. Since the input candidate represented in this tableau is 1sg, forms which have a different specification for Number (such as, again, *are[pl]*) incur a violation mark in this column. It is worth noting that the form *are[]*, which is a morphological form not specified for number at all (an "unmarked" or "default" output form, if you will) does not violate the constraint ID(N) because it does not contain a conflicting number specification. In this particular tableau, that for the first person singular, the second constraint does not actually get rid of any candidate output forms not already eliminated by the *PL constraint.

The constraint which Bresnan and Deo call *SOC is defined by them as follows: "SOC: Avoid singular expressions for second person inputs. That is, mark a candidate if the input PERS value is 2 and the candidate NUM value is SG" (Bresnan and Deo 2001: 11). This constraint is designed to capture the fact that it is common, cross-linguistically, for speakers to "avoid too direct reference" to the second person, a tendancy which the authors note "may become crystalized in grammars." This constraint plays no direct role in the tableaux we will be considering here.

The constraint ID(P) favors output candidates which respect the input form's specification for Person. Obviously forms such as *art[2sg]* violate this constraint. In fact, at this point in the evaluation process the only surviving candidates are *am[1sg]*, *is[sg]* (which is underspecified for Person, and thus does not violate ID(P)), and the massively underspecified *are[]*.[32] The next constraint, which is a markedness constraint against having distinct specified second-person forms, does not get rid of any of the surviving candidates in this particular tableau. The MAX(P) constraint is a Faithfulness constraint, like the Ident constraints we saw earlier; however, in order not to violate it, an output candidate must be as fully ("maximally") specified along the Person dimension as the input form. Underspecified forms such as *are[]* which do not violate ID constraints, do violate MAX constraints, since they do not maximally encode a Person (or whatever) specification like the input form does. Since this holds of both *are[]* and *is[sg]*, but not of

[32] It is important for the general assessment of work in Optimality Theoretic syntax that one try to understand just how the output candidate set is arrived at, but that matter lies outside the scope of our critique here. We will assume, though it is far from obvious that this assumption is valid, that an appropriate mechanism exists.

[1sg]	*PL	ID(N)	*SOC	ID(P)	*2	*1	MAX(P)	*3	*SG	MAX(N)
am[1sg]						*!			*	
art[2sg]				*!	*				*	
is[3sg]				*!				*	*	
is[sg]							*		*!	
are[pl]	*!	*					*			
☞ are[]							*			*
are[1pl]	*!	*				*				
are[2pl]	*!	*		*	*					
are[3pl]	*!	*		*				*		

Figure 8.9: Tableau for "Kent" variety 1sg 'are'

am[1sg], at this point in the tableau *am[1sg]* becomes the winning candidate. Every other candidate has violated some higher ranked constraint than has *am[1sg]*. For the standard language this is, of course, the correct result. Note that the fact that the winning candidate, *am[1sg]*, violates lower-ranked constraints against specified first person forms (*1) and specified singular forms (*SG) is irrelevant – all of its competitors have already violated constraints which were more highly ranked and thus have been eliminated; the violation of the lower-ranked constraints is completely irrelevant.

How does the Optimality Theoretic grammar of the "Kent" variety differ from that of the "Standard" variety? If we were just worried about these two varieties of English (recall that Bresnan and Deo attempt to include a broader range of data in their model), we could readily represent the constraint rankings for "Kent" as in figure 8.9.[33] The only difference between the constraint ranking in this tableau and that in the "Standard" tableau above has to do with the relative ranking of the constraints *1 and MAX(P). In the "Standard" variety tableau, MAX(P) outranks *1, whereas in the "Kent" variety tableau, the reverse holds. Let us now turn to a consideration of how the reranking changes the winning output candidate for the first-person singular form.

Since the first 5 constraints have maintained their ranking status in Kent, we find that when we get to the sixth constraint we have the same three surviving candidates as we had at that point in the "Standard" tableau: *am[1sg]*, *is[sg]*, and *are[]*. In the "Standard" tableau, the next constraint to be considered was MAX(P), which served to eliminate *is[sg]* and *are[]*, leaving *am[1sg]* as the winning output form. In the Kent tableau, by contrast, the next constraint to be considered is

[33] In Bresnan and Deo's analysis, there are many differences between the "Standard" constraint ranking and that seen in the "Kent" tableau – these have to do with getting the entire paradigm to come out in numerous dialects. The issues arising from that approach need not detain us here.

*1, which of course eliminates *am[1sg]* from the output candidate pool, since it favors forms which are not specified for first person. This leaves only *is[sg]* and *are[]* as potential output candidates.

The next constraint, MAX(P), which served to eliminate these candidates from the "Standard" tableau, can now no longer perform that function. Since *both* surviving candidates violate MAX(P) it fails to select an optimal output form from between them, and the evaluation process continues by checking the candidates against the next most highly-ranked constraint. The constraint *3, which favors forms which are not specified for third person, also fails to distinguish between our two surviving output candidates, since neither is so specified. It is only when we get to the next constraint, *SG, which favors forms which are not specified as singular, that the grammar allows us to eliminate *is[sg]* as a potential output candidate, leaving only the massively underspecified "default" form *are[]* as our winning candidate, and thus the optimal output form. As you will recall, this is correct for the Kent variety.

The third variety treated by Bresnan and Deo (2001) – and the variety which allows us to explore their model of morphosyntactic variation in detail, is that which they call "Kent variable." To account for this output, which shows, as you will recall, variation between *am* and *are* in the first-person singular, Bresnan and Deo expand the computational power of the Optimality Theoretic grammar by adding one more component to it: "stochastic perturbation" of real number based rankings. In traditional OT, as described above, the constraints are simply rank ordered – the distance between any two adjacently-ranked constraints is always the same (one "step," if you will). Under the constraint ranking system envisioned by Stochastic Optimality Theory, each constraint is assigned a numerical value, so that the distance between any two adjacently-ranked constraints could be a very large one or a very small one.

Imagine, for example, in the Kent tableau given in figure 8.9 that the numerical ranking value of the constraint *2 is 0.75, that that of *1 is 0.60, and finally that the numerical ranking value of MAX(P) is 0.58. The relative ranking of these constraints will be just as in the nonstochastic version sketched above: *2 will outrank, at 0.75, *1 (whose value is 0.60), which will in turn outrank MAX(P) (at 0.58). However, crucially, *2 outranks *1 by a much greater amount than that by which *1 outranks MAX(P). To this more explicitly detailed constraint ranking model, Stochastic Optimality Theory then adds the notion of "stochastic perturbation," whereby the rankings are, on any given computational "run" through the grammar, each multiplied by a random, i.e., "stochastic," factor. Those constraints which are ranked one above the other, but with a considerable distance between them, will not be affected in their ranking by the relatively slight changes in numerical ranking value induced by the stochastic factor (so in our tableau the constraints *2 and *1 will not be likely to change their ordering under stochastic perturbation). However, those constraints whose separation is relatively small, such as the constraints *1 and MAX(P) under the assumptions sketched in this paragraph, can have their values "perturbed" to such an extent of the world")! Note in particular the striking contrast between the earlier claim of these authors that "variation is part of the internalized knowledge of language – the linguistic 'competence' – of speakers," with the claim above that "[t]he specific choice of variant outputs is *not* determined solely by the grammar, and stochastic evaluation provides an explicit model of this fact." Stochastic perturbation must be either, as the first quote would indicate, part of the internalized linguistic competence of the speaker, or a way to tweak our models because of gaps in our knowledge of the world, but it cannot be both.

The following passage is somewhat more explicit about the details of the model, building on Boersma and Hayes (2001). Note, however, the frequent use of some crucially undefined concepts.

> It is well known from sociolinguistics that macro-level factors – such as the social meaning of an expression in a certain context – affect variation. While some aspects of social meaning could be grammaticalized into the contents of expressions and constraints (morphological markers of politeness levels, for example), other social aspects could be independent of the grammar fragment/partial theory in question, constituting "noise" to the syntactician, perhaps. A third way that sociolinguistic factors could affect variation is by systematically boosting or depressing selected constraints. A model of this effect is given in (16) (adapted from Boersma and Hayes, 2001: 82–3):
>
> (16) effective ranking = constraint ranking$_i$ + *styleSensitivity*$_i$ · *Style* + noise
>
> Here *styleSensitivity* is a constraint-specific value added to the constraint ranking: when positive, a constraint's ranking is boosted; when negative, the ranking is depressed; and when zero, the ranking is unaffected, or stylistically neutral. *Style* is a continuous variable ranging from 0 (for most casual style) to 1 (for most formal). According to this model, the rankings of various "style sensitive" constraints may covary (directly and inversely) with the speech style. These covarying subgrammars could be viewed as representing **sociolinguistic competence.** (Bresnan & Deo 2001: 37–8)

The passage defines some terms mentioned in the quotation from a somewhat later paper by Aissen and Bresnan (2002), but, as mentioned above, gives rise to a number of unclarities. What is a "grammar fragment/partial theory" and who cares what is independent of it?[35] What are "covarying subgrammars"? Are these the same, or different from, Kroch's "dual base" systems? What is "sociolinguistic competence" and how do "covarying subgrammars" represent it? Is it different from the authors's earlier mention of "linguistic" competence (which "variable output" was also taken to be a hallmark of)?

A rather clear sketch – though one that is not consistent with many of the quotations given above – of how these issues come together for some proponents

[35] After all, "phonology" could be absent from a "grammar fragment/partial theory" of English, but does that tell us anything?

of Stochastic OT can be seen in Slide 47 from Aissen and Bresnan (2002). In response to the self-posed question, "Does it make sense to derive frequencies of usage from grammar?," they note the following:

> Knowledge of the grammatical structure of a particular language is represented by the (mean) ranking values of the constraints. Extra-grammatical factors affecting language use are represented by the variables that perturb the rankings. So each "competence" grammar (= set of ranking values) is embedded in a "usage" grammar (the style and noise variables). This embedding enables a much richer array of evidence to be used in studies of grammar than with classical approaches. (Aissen & Bresnan 2002: Slide 47)

The first sentence of this passage stands in stark contrast to the claim by Bresnan and Deo (2001) that a Stochastic OT grammar represents the knowledge of language ("linguistic competence") of a speaker. In this discussion, the OT grammar *without* any stochastic perturbation is taken to represent grammatical knowledge, and the stochastic perturbation is an "extragrammatical" factor.

As I have had occasion to argue extensively at several points in this book, no one to my knowledge has ever maintained that all aspects of human behavioral output in the language domain are the result of grammatical computation. Grammars do not tell one what to say, how loud to say it, how fast to talk, which language to use on a particular occasion, nor do they control many other fascinating dimensions of human behavior. That the output of the grammar is subjected to postgrammatical transduction and computational processes is quite clear. What is unclear is what advantage might arise by labeling some or all of these processes elements of a "usage" grammar.[36] Expanding the term "grammar" in this way only makes seemingly intriguing statements, such as "[t]his embedding enables a much richer array of evidence to be used in studies of *grammar* than with classical approaches," statements which are repeated like political slogans, incredibly mundane. It is, after all, fairly obvious that if I link some number of additional formal systems to that of the "competence" grammar (i.e., the "grammar" as that term has been used in late twentieth-century linguistics), e.g., that of planetary motion (call it a "planetary grammar"), that a "much richer array" of evidence will be used to study "grammar" (which term now has been expanded to include the systems which underlie planetary motion). But have we learned anything by taking distinct systems, each of which could (and, as we have seen, almost certainly must) be studied independently, and treating the diverse evidentiary foundations for their study as a unitary body? I fail to see how we do.

Some amount of the "rich array of evidence" must be used to construct the "competence grammar," which does not generate variant output. This is, of

[36] Note that the passage from Aissen and Bresnan (2002) starts out calling the factors which are now being said to be part of the "usage grammar" *extragrammatical*. So why call it a "usage *grammar*"?

course, the type of evidence exploited by "classical approaches" to the study of linguistic competence. The rest of this "much richer array of evidence" is to be "accounted for" by positing a stochastic perturbation of the "competence" grammar's constraint ranking – but if the "stochastic perturbation" is simply an admission that, for the relevant phenomena, we have a "gap in our knowledge of the world," then are we actually *using* this new kind of evidence in any meaningful sense?

If we ask ourselves what the Stochastic Optimality Theory approach has gained us over an approach which posits only categorical grammars for the case of the "Kent variable" variety of English whose analysis by Bresnan and Deo I have sketched in some detail above, it is not difficult to see, in my opinion, that we have learned very little indeed. After all, a perfectly plausible explanation of the "Kent variable" data, one that – unlike the Stochastic OT approach – doesn't separate such variation from the real sociolinguistic context in which the data was gathered, can be readily constructed. The "Kent" variety has, as noted above, a first-person singular present tense noninverted form of the verb to be *are*, where the "Standard" variety of English has *am*. It seems not at all implausible that many if not all "Kent" speakers are aware of this difference between the English they experience in the media, in books, and from non-Kentish anglophones they meet on a day-to-day basis, and their own dialect. It would not be at all surprising if the choice of which form to use in a particular context carries social significance. Some Kentish speakers, plausibly, have experienced more exposure to the standard than others; some, doubtless, feel less comfortable using Kent dialect forms with non-Kentish individuals than others, etc. If a speaker in Kent occasionally used the Standard form instead of the Kentish one, or if a speaker who generally, in her/his day-to-day life, speaks the Standard a great deal were to provide the linguistic fieldworker with Kentish forms with less than perfect consistency in the course of the Survey of English Dialects interview, who would be astonished? The mixture of forms from different dialects for social effects is a well-known attribute of polydialectal speakers; that it occasionally worked its way into the SED materials is to be expected. "Kent variable" is variation *between* grammars, not "covarying subgrammars," nor a "single Stochastic OT grammar" producing variable output. There is simply no compelling reason to complicate our understanding of the pursuit in which we are engaged with such notions, nor with "usage grammars" next to "(competence) grammars." Ripping the subtle and complex sociolinguistic processes which give rise to variable linguistic *behavior* by individuals out of their social context is emphatically *not* the way to develop a richer understanding of this interesting phenomenon.

Finally, let me make a general point about positing grammars which give rise to multiple outputs. There is no denying (and to my knowledge, it has never been denied) that the output of humans is variable. This variability is certainly not limited to what one might term "subgrammatical" variation (e.g., in the precise height of an [æ] within the æ space), but includes variation along dimensions normally regulated by grammatical knowledge. There are several possible

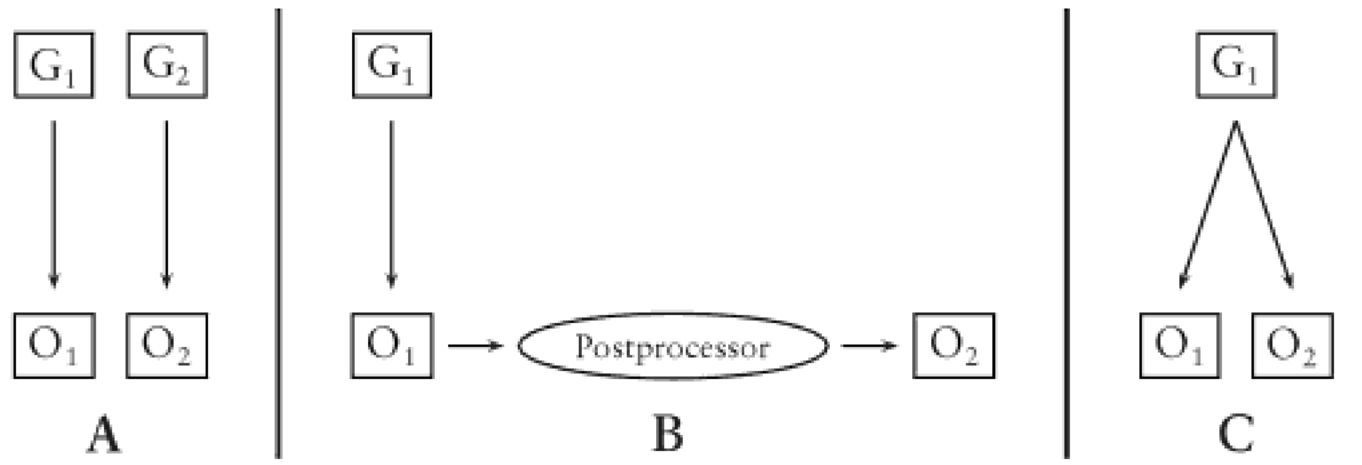


Figure 8.10: The natures of sociolinguistic variation

explanations for this phenomenon, the most plausible of which I have sketched in figure 8.10.

At the leftmost portion of the figure we find Explanation A. As in all the scenarios to be discussed, the speaker generates two distinct forms of output for "the same" communicative intent, let's say.[37] The outputs are labeled O_1 and O_2. Under scenario A, each output is generated by a different grammar. The speaker in question is bidialectal or bilingual, so, of course, produces two types of *behavioral* output. Type A phenomena must exist, barring quite bizarre assumptions about the mental states of, e.g., Chinese–English bilinguals.

Under the model in B, by contrast, the speaker is getting variable behavioral output out of a single mental grammar. This is done by direct modification of the grammar's output by what I have termed earlier a grammatical postprocessor which "translates" the speaker's output forms into a new shape, usually for prestige reasons. Basically, the speaker is feigning bidialectal or bilingual competence. Type B can be experimentally distinguished from Type A because the postprocessor requires higher order cognitive engagement than does grammatical computation, and thus falters under distraction or other performance impediments in ways in which the grammar itself does not. Again, Type B phenomena must exist, since I can fake an unfamiliar English accent after an hour's training. Of course, I cannot fake it *well* after so little exposure, but, after all, that's kind of the point.

In the Type C scenario, there is again a single (relevant) grammar in the mind of the speaker, and that speaker is not using a postprocessor to modify the output of that grammar. Instead, the grammar itself directly generates doublets, much like the Stochastic OT model we considered earlier in this chapter. The question naturally arises as to whether we should supplement our theory of the nature of grammars by expanding them such that they have this capacity – i.e., do Type C phenomena necessarily exist? We already have two ways of explaining the relevant type of variant behavioral output, and each of these two, it

[37] It's a matter of some difficulty to know what to label the relationship between what people call "variants," so I'll leave it in this rough form.

seems, must exist. I can't imagine what evidence could not be accounted for by one of the already necessary types of explanation (Types A and B) such that we should make our theories more powerful in order to have a *third* mechanism for getting variable data. Doing so would appear to be a rather blatant violation of Occam's Razor.

8.3.3 *Relevance of the foregoing for the study of diachronic syntax*

What, the reader may be wondering at this point, does this digression into the world of variation have to do with the study of diachronic syntax, or, indeed, diachronic linguistics generally? The most significant issue which arises from variationist and probabilistic approaches to *synchronic* linguistic systems, which is the real essence of the models discussed above, is simply this: if grammars were to directly generate multiple outputs for the same input, as such models hold, our understanding of the nature of "change" (and thus of what it is we need to explain when positing a particular theory of historical linguistics) would need to be significantly revised. In particular, we would need theories of *change* to account for the shifting frequencies we see in a Kroch-type model (since those shifting frequencies are properties of the grammar itself, and change is about differences in grammars), or changes in the numerical ranking values (and "stochastic perturbation," unless that is a constant) in a Stochastic OT grammar (since, again, these numerical values are part of the grammar itself, and change is about differences in grammars). We would still need a comprehensive theory of diffusion (as opposed to change) events as well, a sociolinguistically sensitive one, of course, since such factors clearly play a central role in diffusion events. The distinction between an innovation and the spread of that innovation would be difficult to discern in such a complex model, it seems to me, and, as I have argued in some detail above, it is hard to see what the field might gain by making the waters, already difficult to see through, even murkier in this manner.

8.4 Conclusion

I have not regaled the reader with long, detailed accounts of complex data sets in the diachronic syntax domain in this chapter, opting instead for a general account of those avenues in this new field which seem likely to be profitable to pursue, and those which seem, to me, less so. We turn in the next chapter to a consideration of how a "lexical features" approach to syntactic change, such as that envisioned by the Minimalist Program, might inform our pursuits in light of a specific, data-rich example: the development of clitic systems. We will also consider, in that chapter, the interaction between (diachronic and synchronic) phonology and syntax in the evolution of such systems.

8.5 Discussion Questions and Issues

A. Discuss the seemingly confused argument, presented in this chapter, that whereas syntactic change proper does not exist, nevertheless it may be useful to use the term "syntactic change" to label a set of diachronic phenomena. Why might it be useful?

B. Included in Lightfoot's list of "distinctive characteristics" for parametric changes is the assertion that "new parameter settings . . . also sometimes set off chain reactions." Discuss the concept of "chain reaction," which is frequently encountered in both the phonological and syntactic diachronic literature. What criteria can be used to decide when, given a chronological sequence of changes A, then B, then C, the researcher is entitled to consider A→B→C a "chain reaction," and when not? Do changes B and C need to be "inevitable" for this to be a "chain reaction" (and if they are inevitable, how can they fail to happen immediately upon the change A's taking place)?

C. Lightfoot also notes that "changes involving new parameter settings tend to take place more rapidly than other changes." Again, the idea that some changes are "rapid" and others "gradual" is widespread in the literature on both historical phonology and historical syntax. Discuss what these terms might mean and how they might be applied to diachronic research.

D. How does Kroch use the concept of the "Blocking Effect" to justify the assumption of "competing grammars" as a model of syntactic variation?

E. Discuss the details of the so-called "Constant Rate Effect." Why is it thought to be significant, and what problems arise regarding it?

F. Imagine that Ellegård is correct, and each verb in the history of English has its own path of diachronic development as far as *do* support goes. What do the statistics used by Ellegård and Kroch and his students tell us? What are they measuring?

G. Discuss the role of stochastic processes in modeling human grammars, considering the various claims cited in the discussion of Stochastic Optimality Theory. Are there truly stochastic processes in other observed computational systems with which you might be familiar?

9 The Diachrony of Clitics: Phonology *and* Syntax

9.1 Wackernagel's Law: Traditional Diachronic Syntax

Although there has been considerably less traditional work on diachronic syntax, when compared to the relative wealth of studies available in the phonological domain, there is at least one quite famous accomplishment in this area: the generalization usually known as "Wackernagel's Law." As a leading Indo-Europeanist notes (Watkins 1964: 1036): "One of the few generally accepted syntactic statements about Indo-European is Wackernagel's Law, that enclitics originally occupied second position in the sentence." In this section we will review the principal claims of Wackernagel (and subsequent researchers) regarding this "law," pointing out some of its major accomplishments and, at the same time, some of its shortcomings. We are particularly interested in exploring how research on Wackernagel's Law is related to then-current general conceptions of the nature of syntactic systems, as reflected in synchronic (and, of course, diachronic) research on syntax. Finally, as an example of a domain within which diachronic syntactic research has shown considerable promise, we will turn to the more general consideration of the diachrony of clitic systems, both with respect to their origins and to specific diachronic developments within existing systems.

We will focus our discussion of Wackernagel's Law on evidence from a single language in an effort to keep the complexities offered up by each individual archaic Indo-European language to a minimum. The data we will use will come primarily from that of the earliest attested Sanskrit – the Sanskrit of the *Rigveda*.[1] This language offers a wealth of evidence regarding clitic behavior (much of it cited by Wackernagel in his original article), and is a language that we have made some progress in understanding the syntax of in recent years.

To begin with some general observations on Rigvedic syntax, we should point out that the word order of that language is what one would generally have referred

[1] This section builds on Hale (1987).

to as "free." The question of whether or not this freedom results from systematic manipulation (via movement processes such as topicalization and focalization, for example) of a more fixed base order, whatever such a term might mean, has not been fully clarified at this time. It does however seem to be the case that we can fruitfully investigate the structure of the Sanskrit clause by examining the distribution of elements which appear to have a more or less fixed position in the clause. At least two types of elements fit the bill: interrogative and relative pronouns (the so-called "wh-words" of contemporary syntactic theory) and the Wackernagel's Law clitics themselves. Since the placements of these two sets of elements show interesting interactions, it is worth our while to survey the distribution of wh-words, even though our real interest will be in the clitics.

That interrogatives show a strong tendency to occupy initial position in their clause has been known at least since Delbrück (1888: 24). As we have said above, the generalization known as Wackernagel's Law asserts that clitics occur in "second position" in their clause. In the archaic Indo-European context, "second position" was defined as "after the first stressed word" (rather than after the first constitutent, e.g., as in many other languages).[2] That both the statement regarding interrogative pronouns and that involving Wackernagel's Law clitics do not tell the whole story can be quite readily seen from examples such as (9.1).[3]

(9.1)	*brahmā́*	*kó*	***vaḥ***	*saparyati*
	priest-NSg	which-NSg	you-APl	honors

"which priest honors you (pl)?" (RV 8.7.20c)

The interrogative pronoun is not in "first" position in its clause, nor is the pronominal clitic *vaḥ*, whose behavior should be being regulated by Wackernagel's Law, in second. Understanding why clauses such as that in (9.1) are found in the language of the Rigveda will bring us a considerable distance in developing an understanding of clitic syntax in that language.

Of course, Delbrück would not have made his generalization about interrogatives, nor Wackernagel his about clitics, if the bulk of the data offered by the language of the Rigveda did not support their assertions. Indeed, interrogatives do normally occur in clause-initial position, and Wackernagel's Law clitics in second. Trivial examples of this type, which could be easily multiplied, are given in (9.2) and (9.3).

[2] It seems clear that this property of clitic distribution is related – at least in some contexts – to the freedom of word order in archaic Indo-European languages, especially to a seeming lack of respect for constituency generally. The matter is explored in greater detail in Hale (1996).

[3] In the examples in this section, clitics will be bold faced in their sentential context to aid the reader in their indentification.

In addition, the exceptional behavior of the Wackernagel's Law clitic in (9.1) is, as it turns out, a consistent pattern of structures which involve both wh-movement and "topicalization," as we can see in (9.8) and (9.9).[6]

(9.8) *idhmáṃ* *yás* ***te*** *jabhárac* *chaśramāṇáḥ*
kindling-ASg who-NSg you-DSg would-bear exerting-oneself

"who would bear the kindling to you, exerting himself" (RV 4.12.2a)

(9.9) *ā́* *yā́ṃ* ***te*** *śyená* *uśaté* *jabhā́ra*
hither which-ASg you-DSg eagle-NSg desirous-DSg brought

"which the eagle brought hither for desirous you" (RV 3.43.7b)

Since Wackernagel's Law would require that the pronominal clitic *te* in these examples follow the first word (which, in these clauses, is the same as the first constituent), we are dealing with a consistent pattern of exceptions. In fact, there are no instances of pronominal clitics of the Wackernagel type in a position to the left of a fronted wh-word in the language of the *Rigveda*.[7] But it would not be correct to say that Wackernagel's Law clitics in general do not appear in their expected positions in wh-clauses which also show topicalization. The examples in (9.10) and (9.11) show *nonpronominal* Wackernagel's Law clitics in such structures, and they occupy their expected Wackernagel's Law position.

(9.10) *áśmānaṃ* ***cid*** *yé* *bibhidúr* *vácobhiḥ*
rock-ASg Emph who-NPl smashed words-IPl

"who smashed *even rock* with (mere) words" (RV 4.16.6c)

(9.11) *utá* ***vā*** *yó* ***no*** *marcáyād* *ánāgasaḥ*
also or who-NSg us-APl would-harm innocent-APl

"or also who would harm us, though innocent" (RV 2.23.7a)

In (9.10) we see the so-called "emphatic" particle – a Wackernagel's Law clitic – *cid* in a clause which contains both a wh-word (*yé*) and a "topicalized" element (*áśmānaṃ*), but this clitic takes its position after the first word of the clause, as Wackernagel's Law requires. The matter is even clearer in (9.11), where the nonpronominal Wackernagel's Law clitic *vā* 'or' occupies the expected Wackernagel's Law position after the first word, but the pronominal Wackernagel's Law clitic *no* sits in the "exceptional," post-wh-word position we saw for pronominal clitics above. There can be no question of a "suspension"

[6] Note the minimal contrast between (9.6) and (9.8).

[7] Nor, for that matter, in Homeric Greek. The constraint appears to be an old one, not surprisingly, as we shall see.

of Wackernagel's Law in this clause, since we need to account for the placement of *vā*.[8]

The placement of the emphatic Wackernagel's Law elements, like *cid* in (9.10), can be dealt with readily, but a discussion of the *vā* and pronominal types must await a more detailed consideration of our modern understanding of clitic behavior, to be explored later in this chapter. In the *cid* case, it would appear that emphatic clitics of this type attach to the emphasized element (rather than, e.g., to the clause as a whole).[9] Since an emphasized element such as *áśmānaṃ* 'rock' in (9.10) is subsequently subjected to "topicalization" (because of the syntax and semantics of "emphasis" in the language, one assumes), any clitic which is attached to the emphasized element will find itself in apparent Wackernagel's Law position, i.e., after the first word, the first word being precisely the element that the clitic rode in on, in some sense.

What this discussion reveals quite clearly, especially when coupled with the distinct placement of *vā*-type clitics and pronominal clitics in examples such as (9.11), and many others of that type, is that there is no unitary phenomenon of "Wackernagel's Law" in archaic Indo-European languages nor, presumably, in other languages. For a number of distinct reasons, some of which we have not yet explored, a variety of "unstressed" enclitic elements happen to come to occupy positions near, but not at, the beginning of their clause. In many clauses (e.g., those lacking wh-elements, and thus providing no indication as to the precise location of the specifier of C position in the observable string) it appears these elements are piling up in the "same" position, but this is a mirage. For example, in (9.3), repeated here as (9.12) for your convenience, under the traditional analysis the clitics *vā* and *te* are "piling up" in "second position."[10]

(9.12)	*kéna*	***vā***	***te***	*mánasā*	*dāśema*
	by what-ISg	or	you-DSg	intent-ISg	we-worship

"or by what intent would we worship you?" (RV 1.76.1d)

However, since we now know that if one were to emphasize *mánasā* in such a clause, the resulting string would be *mánasā* ***vā*** *kéna* ***te*** *dāśema*, rather than the

8 In any event, it's completely unclear what a "suspension" of Wackernagel's Law might mean. It is of some interest that in living languages which display Wackernagel's Law phenomena, we don't find apparent "optionality" in its application, as is so often assumed for archaic Indo-European languages.

9 There are, in fact, interesting complications in this domain, but they need not detain us here. See Hale (1996) for further discussion.

10 Note that their ordering in this Wackernagel's Law procedure must be stipulated, and it would appear to be completely due to chance that when the two clitics are teased apart, e.g., by the effects of "topicalization," it is *vā* that ends up after the first word and *te* which ends up after the fronted wh-word. If the clitics, on the other hand, are analyzed as having been placed by distinct processes, their ordering can be made to fall out from the nature of those processes, rather than by stipulation.

ungrammatical **mánasā vā te kéna dāśema*, it is apparent that these two elements are being positioned not by a single, monolithic "Wackernagel's Law," but rather by two different algorithms, which place the elements from these two classes of clitics ("disjunctive"/"conjunctive" vs. pronominal) in two distinct "second positions." That the distinction between these two positions only emerges under certain structural conditions is consistent with many other discoveries about the nature of syntactic phenomena in the modern era.

Again, we see that the traditional approach to linguistic matters – now in syntax, as we saw earlier in phonology – fell short in developing an empirically or structurally satisfying account of a particular phenomenon. It is important to appreciate the massive amount of analytical work which supported Wackernagel's pioneering research regarding clitic distribution,[11] of course, but it is also, in my opinion, worth considering just what stood in the way of the development of a fuller account of the data. At the end of Section 8.1, I discussed the fact that I thought that one explanation for the lack of progress in research on syntactic change had been the failure of pregenerative scholars to recognize the importance of null functional elements – i.e., of the inaudible structural elements which provide the basic structure of the clause.[12] The idea that the position into which wh-elements move is present *regardless of whether or not it is filled* (and thus "visible" in the data set) and that one must try to understand the placement of clitics relative to this often phonologically null piece of clause structure is radical and modern. It is as significant a development for our understanding of linguistics, diachronic and synchronic, as similar moves towards ever more abstract models in the natural sciences. By embracing this more abstract understanding of what, exactly, a sentence is, new insights can be gleaned into well-known diachronic phenomena such as "Wackernagel's Law."

In the coming sections of this chapter, the basic framework within which one may account for the behavior of disjunctive/conjunctive clitics such as Rigvedic *vā* 'or' and *ca* 'and,' as well as that of the pronominal clitics we have seen above, will be presented.

9.1.1 "Simple" clitics and "special" clitics

It has been traditional since the late seventies to adopt the terminology proposed by Zwicky (1977) in discussions of the typological properties of clitic systems.

[11] Although we would be remiss not to point out that the original generalization now known as "Wackernagel's Law" was formulated in Bartholomae (1886), for Avestan, well before the much more comprehensive and cross-linguistically supported analysis presented in Wackernagel (1892).

[12] Just as, in much the same way, the failure to recognize the importance of the structural and computational properties of the abstract system which underlies phonological behavior has been an impediment to progress in *that* domain.

While we will see that the terminology is, in the end, somewhat inadequate, the widespread use of these labels makes the classification proposed by Zwicky a useful jumping-off point for our survey. Building explicitly on Zwicky, Anderson (1992) gives the following "working" definitions of clitic subtypes (assuming all clitics are prosodically deficient):

- **Simple Clitics** "are elements belonging to some syntactically well-defined type . . . , appearing in a position where the normal principles of the syntax might in principle sanction a member of the relevant category" (Anderson 1992: 18)
- **Special Clitics** "appear in some special, designated position where no rule normally applicable to items of their lexical class would locate them. The best-known class of such special clitics is the set of **second-position** clitics that appear in many languages" (emphasis in original; Anderson 1992: 20).

A classic example of Zwicky's "Simple Clitic" type can be seen in English third-person object pronouns with the phonological form **/əm/**. This pronoun is used for both singular and plural objects (whereas the stressed, or tonic, pronouns *him*, *her*, *it*, *them* distinguish number): *He has seen [***əm***]* 'He has seen him/them.' This clitic is prosodically deficient (a necessary property for a clitic), as can be seen from its ill-formedness under contrastive stress (focus stress is indicated by the acute accent in this example):

(9.13) *I didn't see hér, I saw [**ʔə́m**].

The "simple" (as opposed to "special") status of Modern English **/əm/** can be deduced from the fact that in general this clitic pronominal object occurs in the same position as fully tonic NP objects: *He has seen the articles.*

This contrasts sharply with a classic example of a "special" clitic: French object pronouns. Compare the following sentences:[13]

(9.14) *Il* ***les*** *a vus.*
he them_{cl} has seen
"He has seen them."

(9.15) **Il les articles a vu(s).*
he the articles has seen
"He has seen the articles."

[13] The parentheses on *vu(s)* in (9.15) and (9.17) are merely intended to indicate that the (normally inaudible) agreement *-s* is irrelevant to the grammaticality of these strings – they are ungrammatical with, or without, agreement on the participle.

(9.16) *Il a vu les articles.*
he has seen the articles

"He has seen the articles."

(9.17) **Il a vu(s)* ***les.***
he has seen them$_{cl}$

"He has seen them."

The contrast in grammaticality between (9.14) and (9.15) reveals that the clitic *les* may occupy a position which the phrasal object (i.e., the nonpronominal, nonclitic object) may not. On the other hand, the contrast in grammaticality between (9.16) and (9.17) reveals that the phrasal object may occupy a position which the clitic object may not. Since the nonclitic object occupies the position which all other (nonclitic) verbal complements occupy, it seems sensible to take the clitic objects as having been "displaced" from this base position. It is this "displacement" – lacking in the case of English [əm] – which makes the French object clitics "special," as opposed to "simple," clitics. While the word order of Rigvedic Sanskrit is much more opaque (to us) than is that of contemporary varieties of French, it seems likely that the pronominal clitics discussed for the language of the *Rigveda* are "out of position" (since they are consistently placed in a position near the start of their clause and are fully stressed, whereas nonclitic arguments in the same case are not), and thus represent "Special Clitics."

9.1.2 Mechanisms of displacement: ways to be "special"

If the essence of "special"-ness, with respect to clitics, is being "out of expected position," it makes sense to ask the question: what are the available grammatical mechanisms which could lead to the displacement we see with such clitics?[14] There appear to be two general types of mechanisms which have been invoked.

Mechanism 1: syntactic "movement"

While the details are quite complex and needn't detain us here, the standard analysis of "special" pronominal clitics is that they undergo *head* movement to

[14] I am attempting to be relatively neutral about the precise meaning of "displacement" – the usual metaphor in East Coast circles is "movement." Any model of syntactic knowledge would appear to require a procedure or system for indexing the "expected position" of an element (e.g., object position after "see" for "what" in "What did John see?") and its "surface" position. The relationship between surface position and "expected" position is what I am calling "displacement."

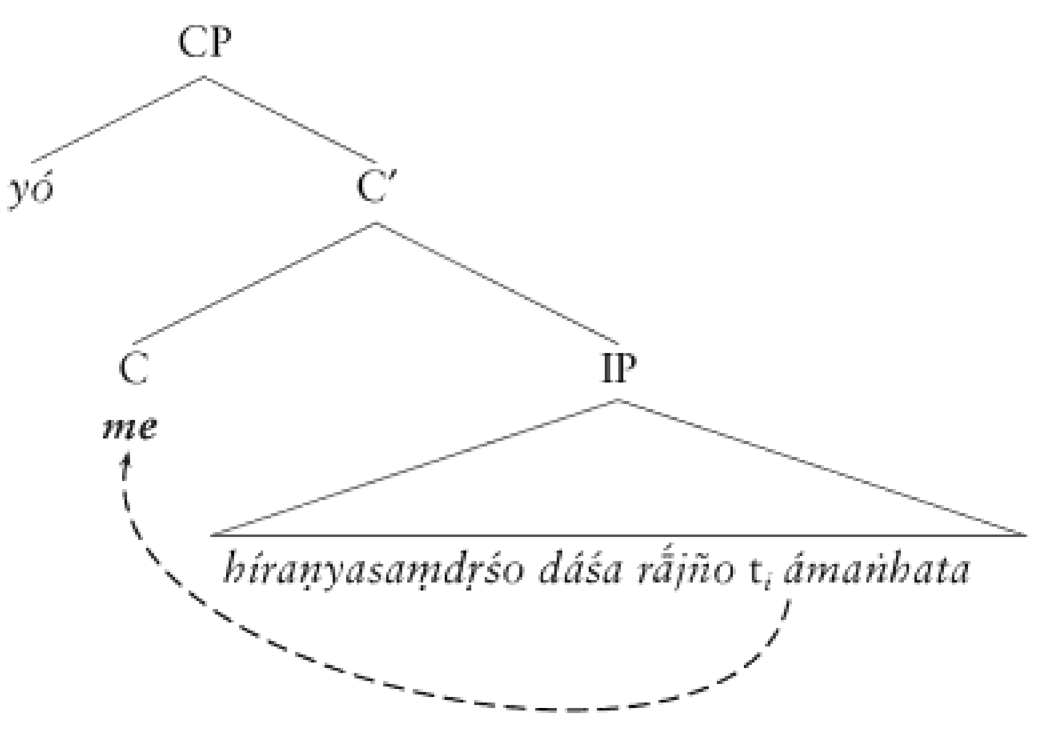
some well-defined syntactic position. Nonclitic arguments, by contrast, being (and remaining) phrasal, cannot undergo "head" movement and thus can never occupy "the same" position as pronominal clitics. Movement (or adjunction) to C is a popular version of this pronominal clitic movement.

We will continue to use examples from the *Rigveda* as the basis for our discussion of Wackernagel's Law. This gives us a direct connection to the claims and analyses of Wackernagel himself (who used a great deal of Rigvedic data in his original paper), and, as noted earlier, provides us with a particularly rich source for data on clitic behavior. Example (9.18) shows the pronominal clitic ***me*** in "second position," as predicted by the traditional formulation of Wackernagel's Law (henceforth sometimes abbreviated as WL). The tree in (9.19) shows how such a positioning of the pronominal clitic would come about under the assumption that pronominal clitics are displaced into C.

(9.18) *yó* ***me*** *híraṇyasaṃdṛśo / dáśa rā́jño ámaṅhata*
who-NSg me gold-looking-APl ten king-GSg granted

"who granted me ten gold-looking ones of the king" (RV 8.53.7ab)

(9.19)



Note that although "Wackernagel's Law" (which allegedly gives rise to the "second position" clitics alluded to by Anderson above) states that WL clitics go "in second position," this is (in the case of these pronominal clitics) an accidental byproduct of the fact that placing the clitic in C will have the effect of causing it to be very near the start of the clause, separated from it only by whatever material may appear in the specifier of CP (i.e., SPEC, CP), or in an even higher functional projection, such as the "topicalization" site mentioned in the last section.[15] Support for the notion that clitics move to C, an idea initially

[15] The reader might well wonder what happens if there is nothing in the specifier of CP. This issue will be treated in some detail in what follows.

as (9.23), which involve the coordination of *mātúr upásthe* 'in the lap of the mother' and *vána ā́* 'inside the wood':

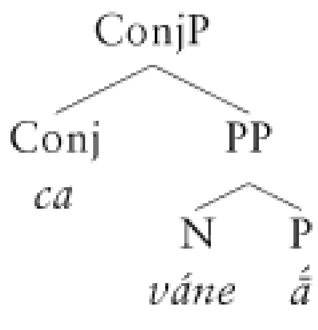
(9.23)	*sahásradhāro*	*asadan*	*ny*	*àsme /*	*mātúr*	*upásthe*
	1000-flowing-NSg	sat	down	us-LPl	mother-GSg	lap-LSg

	vána	*ā́*	*ca*	*sómaḥ*
	wood-LSg	P	and	soma-NSg

"thousand-flowing Soma sat down by us in the lap of the mother and inside the wood" (RV 9.89.1cd)

The second element of this conjunction can be represented as in the tree in (9.24).

(9.24)



Just as *vā* sat, unhosted, at the left edge of its domain (the DisjP) in (9.22), *ca* in (9.24) is also unhosted at the left edge of *its* domain, the conjunctive phrase (ConjP). We thus anticipate the need for a "prosodic flip" in such a structure such that *ca* will be appropriately hosted within its domain. Such a flip, as Halpern argues in detail, and as I have mentioned above, should be *minimal*. However, as we can see by its accentuation (and know, in any event, since it is an "open class" lexical item), *váne* would represent a perfectly good prosodic host for the conjunctive clitic *ca* in this string. Why, then, do we not find *váne ca ā́* in the output (like the *ugró vā índraḥ* of (9.22))?

The reason for this seemingly unexpected behavior of *ca* in (9.23)/(9.24) is to be derived from a special property of the prosodic relationship between postpositions and their objects in Rigvedic Sanskrit. It appears, in particular, that postpositions which govern nonbranching N's in Sanskrit form a *phonological word* with that N. We can see this from, among other things, the fact that, whereas word-final *s* normally becomes *ḥ* before *p* (and some other segments) in Sanskrit, in (9.25) the seemingly word-final *s* of *divás* is preserved.

(9.25)	*divás*	*pári*
	heaven-AblSg	from

"from heaven"

The preservation of the *s* of *divás* finds a straightforward explanation if it is the case that this *s* is *phonological word internal* after phonological words are constructed and does not, therefore, meet the conditions ("word final") for the debuccalization. This, in turn, entails that postpositions and their nonbranching N complements are built up into a single phonological word by the prosodic phonology of the language of the *Rigveda*.

Given this, *vána ā́* should also represent a phonological word. The first possible host for *ca* in the tree above is thus not *váne* (which is no longer a phonological word – i.e., is something less than a phonological word) but rather the newly-created phonological word *vána ā́*. If *ca* placement were *syntactic* in nature, this would make no sense, since the syntax has no access to information about *phonological* (as opposed to *syntactic*) words.

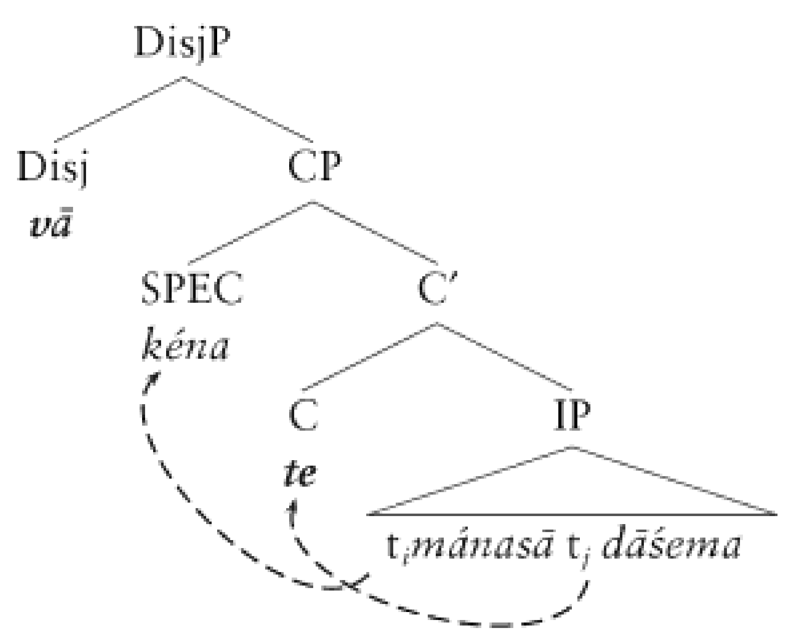
We are now in an excellent position to return to a consideration of the data on "disjunctive" vs. "pronominal" Wackernagel's Law clitics from the previous section. Let us first consider the data of apparently well-behaved clitics (well-behaved from the perspective of the traditional statement of Wackernagel's Law) which we saw in examples such as (9.12), repeated below as (9.26).

(9.26)	*kéna*	***vā***	***te***	*mánasā*	*dāśema*
	by what-ISg	or	you-DSg	intent-ISg	we-worship

"or by what intent would we worship you?" (RV 1.76.1d)

In sharp contrast to the traditional analysis, which takes such clauses as an instance of the "piling up" in second position of Wackernagel's Law clitics, we now see that a plausible syntactic analysis of this string would have the form seen in the tree in (9.27).[19]

(9.27)



This would yield the following string as input to the prosodic phonology, where I have marked the disjunctive phrase, since this is the domain of the clitic *vā*:

[19] Although I have taken a position on the ordering of elements before they have moved out of the IP, this is for convenience only. The detailed analysis of Sanskrit word order remains a desideratum.

(9.28) [$_{\text{DisjP}}$ ***vā*** *kéna* ***te*** t_i *mánasā* t_j *dāśema*]

Clearly, *vā* is unhosted on its left within its prosodic domain (the DisjP). As expected, the "prosodic flip" is thus triggered, as represented in (9.29).

(9.29) ***vā*** *kéna* ***te*** t_i *mánasā* t_j *dāśema*

The result of the "prosodic flip" is the observed phonological linear order, *kéna* ***vā te*** *mánasā dāśema*. One can see just how different this analysis is from one which invokes a monolithic Wackernagel's Law.

Key support for this break-up of Wackernagel's Law into a set of component processes is provided by structures of the type seen in (9.11) above, repeated here as (9.30).

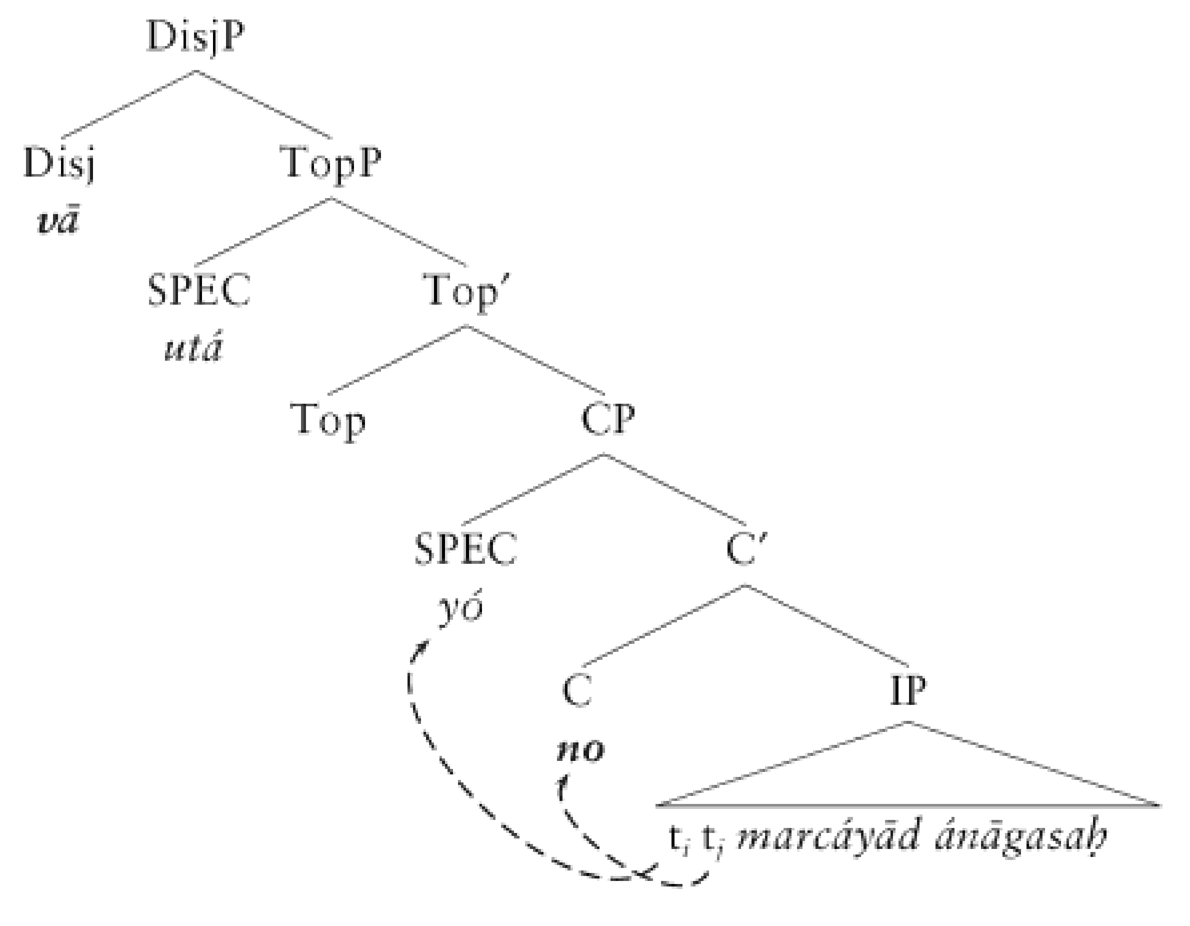
(9.30)

utá	***vā***	*yó*	***no***	*marcáyād*	*ánāgasaḥ*
also	or	who-NSg	us-APl	would-harm	innocent-APl

"or also who would harm us, though innocent" (RV 2.23.7a)

In this example the adverb *utá* 'also' has been "topicalized." The entire relative clause *utá yó no marcáyād ánāgasaḥ* is being disjoined from another relative clause, and thus represents a DisjP. As expected, the wh-word *yáḥ* (which surfaces in this context as *yó*) has undergone movement to the specifier of C position, *utá*, having been "topicalized," sits in the specifier of the Top position, and of course the pronominal clitic *naḥ* (surfacing as *no* in this context) has moved to C. All of this can be seen from the syntactic output tree for the sentence in (9.31).

(9.31)



This gives us the following input to the prosodic phonology, once again with the left edge of the disjunctive phrase (the relevant prosodic domain for the disjunctive clitic *vā*) indicated:

(9.32) $[_{\text{DisjP}}$ ***vā*** *utá yó* ***no*** t_i t_j *marcáyād ánāgasaḥ*

Here again, ***vā*** is not properly hosted within its domain, it moves in the phonology to a position after the first appropriate host in that domain (*utá*), giving straightforwardly the attested linear output, as in (9.33).

(9.33) ***vā*** *utá* *yó* ***no*** t_i t_j *marcáyād ánāgasaḥ*

Note that every operation is precisely the same as that seen in the discussion of (9.26) above – wh-movement to the specifier of C, movement of the clitic pronominal to C, and the prosodic flip of *vā*. The *apparent* difference in the placement of clitics in the output, with (9.26) seeming to respect Wackernagel's Law for all of its clitics, and (9.30) seeming to respect it for *vā*, but violate it for *naḥ*, is based purely on the absence of overt elements in the specifiers of C or Top in (9.26) which could serve as actual signposts as to what precise position in the string the clitics in question are occupying. The optimal analysis in such a case is not to posit a rather ill-defined "Wackernagel's Law" (with stipulations as to the ordering of clitics, e.g., and a language-specific definition of "second position") with empirical exceptions (such as the placement of *naḥ* in (9.30)), but rather the more unified approach advocated here.

The interaction of mechanisms 1 and 2

The clitic pronominals of Rigvedic Sanskrit, and other archaic Indo-European languages which show the effects of Wackernagel's Law, move to C syntactically. However, they are also prosodically deficient and therefore must be hosted on their left. If they are not properly hosted when they emerge from the syntax, they will undergo prosodic adjustment in the phonological component in the manner just outlined. Such clitics will thus be doubly dislocated from their base-generated position – once through the syntactic operation which adjoined them to C and once through the phonological process which "flips" them over the first available host.

A plausible example of this phenomenon can be seen in (9.34). This appears to involve normal movement of the subject Noun Phrase (*ayám deváḥ savitā́* 'this god Savitar') into subject position (the specifier of IP) as well as movement of the dative clitic *me* to C, as expected. The resulting syntactic structure is well-formed, syntactically, but when the string is sent to the phonology, the prosodic ill-formedness which results from a left-leaning clitic in initial position in its

3. a "prosodically displaced clitic" as one of either of the above types which has undergone prosodic displacement in the phonology (we expect, therefore, to find "prosodically displaced syntactic clitics" and "prosodically displaced simple clitics").

We have seen an example of each type:

- nonprosodically displaced simple clitics: English *'em* in (9.13);
- nonprosodically displaced syntactic clitics: the French object clitics in (9.14)–(9.17); Sanskrit *me* 'me' in (9.18), Sanskrit *sīm* 'them' in (9.20);
- prosodically displaced simple clitics: Sanskrit *vā* 'or' in (9.22) and *ca* 'and' in (9.23);
- prosodically displaced syntactic clitics: Sanskrit *me* 'me' in (9.34).

This list of examples reveals an important fact: we find all four types of clitic distribution, but it would be inaccurate to say that we have a typology of *clitics* here. One and the same clitic (e.g., Sanskrit *me*) can show up under more than one classification (as is the case in the above-cited examples with Sanskrit *me*). The typology is rather that there are two clitic types (as often assumed since Zwicky's work, though we will adopt the conceptual innovations regarding this distinction introduced above), either of which may undergo, under appropriate prosodic conditions, displacement.

9.2 What Can('t) be a Clitic?

The typology given in the previous section makes no claims regarding what types of linguistic entities may be clitics, and what types may not, beyond the requirement of prosodic deficiency. Nevertheless, it seems certain that the contrast between these two classes of entities will play a crucial role in the development of a constrained theory of clitic diachrony (assuming what we are about to show – that there are in fact at least two distinct classes of elements). It appears from a survey of the literature on the clitics, that we do indeed find some entities which can be clitics, and another set which cannot:[21]

Can: pronominals, auxiliaries, modals (including "evidentials"), "adverbial particles" (including "interrogative" particles), articles (D), conjunctions, disjunctions, emphatic "particles";

Can't: "open class" lexemes (N, V, A, . . .).

[21] Obviously, it is not the case that every element which *could* be a clitic in any given language is in fact a clitic.

The generalization seems to be that the more "functional" head syntactic entities may be clitics, whereas the more so-called "lexical" heads may not. We might further ask whether there is a difference between what types of elements can be "syntactically displaced" (i.e., "special") clitics, and what types cannot. It does seem that we are not completely in the dark on such matters, and I will discuss in general the contrasts between these various categories below, but let me point out a possible difference between the "syntactic" and "simple" types of clitic which has not been given much attention in the literature.

9.3 What Can('t) be a "Syntactic" Clitic?

I turn again to a Sanskrit example, that of the indefinite pronominal *tva- . . . tva- . . .* which is used to express "one . . . another . . ." An example of the use of this pronoun can be seen in (9.36) below.

(9.36) *pī́yati tvo ánu tvo gṛṇāti*
mocks one-NSg PV another-NSg praises
"one mocks, one praises"

In this example, both of the *tva-*'s are in the nominative case, as is appropriate to their clause. The attested word order can be readily derived from the "prosodic displacement" of these clitics from the regular subject position which they would be expected to occupy if they were "simple" rather than "special" clitics. I'll represent this as in (9.37), where the dotted arrows indicate the prosodic displacement expected for left-leaning clitics in this context.

(9.37) tvaḥ pī́yati / tvaḥ ánu gṛṇāti

Sanskrit has another atonic (stressless) indefinite pronoun, *sama*-'any,' which shows distribution patterns quite similar to those of *tva-*. It is of some interest to note that these indefinite clitics show the full range of case forms we expect to find for pronominals in Sanskrit. Plural forms of these indefinites are rare, so to make clear the range of morphological cases attested I give the pool of singular forms in table 9.1.

One can compare the wide-range of case forms we see in table 9.1 with those we find for the fully-stressed (i.e., tonic) pronouns of Sanskrit, presented in table 9.2. And contrast it rather sharply with the attested case-forms of the clitic personal pronouns of Sanskrit, seen in table 9.3.[22]

[22] Similarly: Serbian/Croation has a full range of tonic pronouns (NOM, ACC, INS, DAT, GEN, LOC), but clitic pronouns *only* in ACC, DAT, and GEN function.

Table 9.1: Sanskrit clitic indefinites (no tonic correlates)

Case	sama- *"any" and* tva- *"one"*
Nominative	*same* (Mpl), *tvas* (M), *tvā* (F), *tvad* (N)
Accusative	*samam* (M), *tvam* (M), *tvad* (N)
Instrumental	*tvena*
Dative	*samasmai*; *tvasmai* (M), *tvasyai* (F)
Ablative	*samasmāt*
Genitive	*samasya*
Locative	*samasmin*

Table 9.2: Sanskrit tonic personal pronouns

Case	*Isg*	*IIsg*	*Idu*	*IIdu*	*Ipl*	*IIpl*
Nominative	*ahám*	*tvám*	*vā́m*	*yuvám*	*vayám*	*yūyám*
Accusative	*mā́m*	*tvā́m*	???	*yuvā́m*	*asmā́n*	*yuṣmā́n*
Instrumental	*máyā*	*tváyā*	???	*yuvā́bhyām*	*asmā́bhis*	*yuṣmā́bhis*
Dative	*máhyam*	*túbhyam*	???	???	*asmábhyam*	*yuṣmábhyam*
Ablative	*mád*	*tvád*	*āvád*	*yuvád*	*asmád*	*yuṣmád*
Genitive	*máma*	*táva*	???	*yuvós*	*asmā́kam*	*yuṣmā́kam*
Locative	*máyi*	*tváyi*	???	???	*asmé*	*yuṣmé*

Table 9.3: Sanskrit clitic personal pronouns

Case	*Isg*	*IIsg*	*Idu*	*IIdu*	*Ipl*	*IIpl*
Nominative	–	–	–	–	–	–
Accusative	*mā*	*tvā*	*nau*	*vām*	*nas*	*vas*
Instrumental	–	–	–	–	–	–
Dative	*me*	*te*	*nau*	*vām*	*nas*	*vas*
Ablative	–	–	–	–	–	–
Genitive	*me*	*te*	*nau*	*vām*	*nas*	*vas*
Locative	–	–	–	–	–	–

The table of attested case forms for the Sanskrit clitic personal pronouns differs dramatically from the table for the indefinites *tva-* and *sama-*. In addition, the former, but not the latter, are "syntactic" (i.e., "special") clitics. It appears that the contrast in attested case forms for pronominals which are syntactic clitics and those which are not, either because they are tonic (as in table 9.3) or

Table 9.4: The (partial) complementarity of clitics and applicatives

Case	*"syntactic" clitics?*	*applicatives?*
Nominative	yes	no
Accusative	yes	no
Dative	yes	yes
Genitive	no??	no?? or maybe yes??
Instrumental	no	yes
Ablative	no	yes
Locative	no??	yes
"Misc"	no	yes

because they are "simple" clitics (as in table 9.1) may be quite general. This allows us to see an interesting complementarity between two phenomena: the syntactic clitic status of certain case forms, and the ability of arguments bearing that case to undergo "applicative" raising to object status. In table 9.4 below I have laid out the attested patterning of specific case forms with respect to syntactic clitic status and use in applicative constructions.[23]

Some evidence for the claims about applicatives can be gleaned from the following Rotuman applicatives (data from Kissock 2003):

Instrumental: *lemi* 'to lick NP-object$_{\text{licked thing}}$' → *lem'ạki* 'to lick-with NP-object$_{\text{thing you lick with}}$'
Ablative: *ararua* 'to be in doubt' → *ararua'ạki* 'to be in doubt from NP-object$_{\text{source of doubt}}$'
Dative: *mamāe* 'to mourn' → *mamāe'ạki* 'to mourn for NP-object$_{\text{e.g., dead person}}$'
Dative: *mh* interjection, expressing dislike of a smell → *mh'ạki* 'to say *mh* at NP-object$_{\text{some smell or another}}$'

It would appear that applicatives (sometimes taken to be the result of "preposition incorporation") may require an argument which stands in (or could be taken to be standing in, at the time the applicative comes into being) for a PP

[23] The following comments are in order regarding this data: putative locative clitics (e.g., French *y*) may be plausibly taken as adverbs ('(t)here'); genitive applicatives might be represented by cases of "possessor raising" (though I do not know of a language which morphologically marks such ascensions with an applicative morpheme); genitive clitics in table 9.3 for Sanskrit are NP-clitics and do *not* undergo syntactic movement to C (and thus are "simple clitics") – probably.

the computational component of human phonological knowledge would not be able to do its computations (feature deletion, feature insertion, spreading, structure-building operations, etc.) over such a lexicon. The second possibility seems precluded *a priori*, because phonological computation is not an operation over *the entire lexicon*, but rather only over individual elements drawn from that lexicon. Having been fed a form such as /bot/ to compute an output representation for, the phonology itself simply has no way to know whether elsewhere, in the portion of the lexicon it is *not* computing over, there is the requisite number of height contrasts in the front space.

The first possibility also does not fare particularly well as a simple thought experiment either, in my opinion. Imagine that the first word a child was exposed to was [mu] and the second was [to]. If humans cannot actually construct representations with more back distinctions than front, the second word would presumably be unlearnable, since, if the child stored the word as /to/, his/her entire lexicon would be /mu/ and /to/, with a two-way distinction in the back, and no distinction in the front. The question is, I suppose, an empirical one, but it's hard to imagine what else, confronted by [mu] and [to], a child might construct as a lexicon, or by what algorithm.

Under the second hypothesis, a human mind with a grammar in it which performed its computations over a lexicon which contained representations having more back height distinctions than front is trivially possible from the computational perspective, but cannot come into being because of (1) the initial conditions and (2) the learning algorithm.

To make this plausible one only need imagine that, because of the limited articulatory space in the back of one's oral cavity (compared to the space at the front), the acoustic correlates of a high:mid contrast in the back space will be less salient (i.e., more prone to misparsing) than the same contrast in the front space. For a system to merge a height distinction in the front and not merge the same height distinction in the back would require that the learner *hear* and *attend to* a contrast which is minimally expressed in the back vowels, but ignore the much more salient expression of the same formal contrast in the front vowels. I assume this is not possible. Thus it is extremely unlikely that, except by some artificial means, a grammar with more height distinctions in the back space than in the front could come into being, and, perhaps (the data is a little unclear on the matter), unsurprisingly, none has. I will refer to this type of explanation for universal generalizations as the "diachronic filter" explanation.

We can now return to our questions above regarding 'frog' in light of the foregoing. Two possible explanations for the absence of a clitic 'frog' emerge:

1. *frog* [+clitic] (or whatever) is *computationally intractable* – i.e., out because of conditions imposed by UG;
2. or, *frog* [+clitic] is out because of the "diachronic filter" (i.e., no evidence can, given existing conditions, give rise to a lexicon in which 'frog' has this property).

Since bearing a high degree of stress is, generally speaking, a strong counter-indication to clitic status, it seems safe to assume that in order to be reanalyzed as a clitic, a lexical item must have come to occupy with some regularity a prosodic "trough" in the phonetic output. If this is correct, there are a number of factors which stand strongly in the way of 'frog' ever coming to be a clitic.

First, let us consider the question "why can't 'frog' be a clitic in a grammatical system in which 'allomorph' is not?" I argued above that there is a grammatically significant contrast between the "encyclopedic" and the "grammatical" lexicon. The lexical semantic differences between 'frog' and 'allomorph' are aspects of the "encyclopedic" rather than the "grammatical" lexicon, and are thus not visible to the grammatical computation (so no language has 'frog' fronting or 'allomorph' movement). Those aspects of the "grammatical lexicon" representations of 'frog' and 'allomorph' which are used by the prosodic domain-constructing algorithm (a grammatical computation) are the same. It is therefore impossible for that prosodic domain-constructing algorithm to regularly place 'frog' in a trough, but 'allomorph' not (since they are *the same* entity for that algorithm).

Second, we can consider the question "why can't 'frog' (and N's in general) be a clitic in some grammatical system?" Here we can build on the fact that the prosodic domain-constructing algorithm results in some elements – allegedly a function of the element's status as a "lexical" vs. a "functional" head (and computationally independent of clitichood) – receiving "lower" degrees of sentential stress, others "higher" degrees of sentential stress. Nouns (which come into the prosodic computation as "lexical" heads) will never consistently occupy positions in the constructed prosodic domains in which they receive "low" sentential stress. Therefore, they will not be reanalyzed as prosodically deficient (i.e., as clitics).

Like the "front" vs. "back" height distinctions, this is a function of (1) initial conditions (i.e., the fact that there were no N clitics in the past) and (2) constraints on diachronic developments (i.e., the fact that N's are not likely to have a distribution within prosodic domains that would allow them to be reanalyzed as clitics). One should not, therefore, conclude that the apparent absence, cross-linguistically, of N clitics provides evidence for the properties of UG itself, which may be perfectly capable of computing over an N which happens to be prosodically deficient, but which, because of the "diachronic filter," never gets a chance to do that computing.

This provides a plausible enough explanation for why 'frog' is not likely to be a clitic in any human linguistic system, but the set of clitics it predicts to exist are only the "simple" clitics like English *'em* 'him/them.' Since pronominals are functional heads, and since the prosodic-domain constructing algorithm regularly places such elements in positions in which they receive low degrees of sentential stress, the regular occurrence of 'him' or 'them' in this low-stress position could easily be reanalyzed as a property of those specific lexemes, rather than being simply attributable to the fact that they are functional elements. Once reanalyzed in this way, those lexemes would be explicitly marked as prosodically deficient (i.e., as clitics). But of course they would not undergo *syntactic movement* by virtue of this reanalysis. So the question naturally arises, where do "syntactic" clitics come from?

Using our familiar two possibilities in framing an explanation for why Sanskrit *ca* 'and,' e.g., cannot be a "syntactic" clitic (which I assume to be the case, knowing of no attested syntactically displaced conjunctive clitics) we would get:

1. a grammar in which *ca* 'and' undergoes syntactic movement to C (like the "syntactic" pronominal clitics) would be computationally intractable (i.e., is UG blocked); or,
2. a grammar in which *ca* 'and' undergoes syntactic movement to C cannot come into being diachronically (but would be tractable, computationally, if it could).

Note that there is no issue here as to whether or not *ca* can become a clitic. As a functional, rather than lexical, head, the prosodic-domain constructing algorithm could trivially construct its phrases (and their stress contours) such that a conjunctive element like 'and' regularly occupied the type of trough which is needed to get reanalysis as a clitic. The question we are interested in is why can't *ca* become specifically a "syntactic" clitic.

If we examine a typical occurrence of clause-conjoining *ca*, such as that seen in (9.38) below, we can see that our prosodically deficient *ca* occupies a position in the string which is immediately adjacent to C (the position to which "syntactic" clitic pronominals regularly move) – which presumably immediately follows the relative pronoun *yé* which must be in the specifier of C position.

(9.38)	. . . *yé*	***ca***	*rātím̥*	*gr̥ṇánti*
	. . . who-NPl	and	gift-ASg	sing

"... and who sing (you) a gift"

It would seem that having *ca* in such a position – the position it normally occupies in cases of clausal conjunction – would make *ca* trivially reanalyzable as occupying a position appropiate for "adjunction to C" (as with pronominal clitics). Diachrony thus does not preclude the reanalysis of conjunctive/disjunctive clitics as "syntactic" clitics *except through the effects of UG*. Grammars cannot come into being in humans which are not possible given the human genome (whose linguistic expression is UG). There are no mechanisms of syntactic computation which would allow conjunctions to move out of their domain to adjoin to higher functional heads, thus the acquirer, confronted by data such as that in (9.38), does not have the option of analyzing the position of *ca* as being due to any such movement. *ca* must, therefore, be *in situ*, so, if it is a clitic, is must be a "simple" one, rather than a "syntactic" or "special" one.

Of course, if UG did not impose such a constraint, then nothing in the surface string itself would prevent a diachronic reanalysis whereby *ca* came to be taken as occupying C. Diachrony alone, unlike the cases surveyed earlier, cannot prevent the evolution of a "syntactic" conjunctive clitic. The absence of such elements, if that absence turns out to be well-grounded in the cross-linguistic facts, must be attributed therefore to UG itself.

One will have doubtless noticed that there is no discussion of "parameter"-resetting or the like in this discussion. The elements which undergo changes are the features of individual lexical items. Providing an account of the causes of these changes requires a sophisticated understanding of how syntactic computation works as well as how phonological computation works, especially as regards the process or processes whereby prosodic domains come into being. It is perhaps not surprising, given the current rather immature state of our theories of syntax, prosodic phonology, and their interaction, that many questions about the evolution of clitic systems remain. I hope, however, that this chapter has provided a somewhat clearer analysis of what we do understand about such systems, with the goal that this clarity will allow for more rapid progress on the large number of outstanding issues in this area.

9.4 Discussion Questions and Issues

A. Consider your own native language. What "clitic"-type elements appear to be present in it? What types of clitics are these elements? Do these elements seem to follow the generalizations of the chapter about what can and what cannot be a clitic?

B. Consider the role of the "diachronic filter" in shaping the attested set of human languages. If some types of systems which are computationally possible grammars cannot arise because there is no diachronic path which, from current conditions, can lead to their coming into existence, how are we to learn the properties of UG? How do we protect ourselves from the distortions diachrony introduces into our pursuit of the knowledge of that underlying human computational competence? Can you imagine how this might be related to issues about "statistical universals" (like "most languages that have property X also have property Y")?

10 Reconstruction Methodology

> serious doubts have also been raised about the status of the methods used to establish [reconstructed] forms, which seem to demand an interpretation of the nature of language change which is at variance with currently acceptable theories.
>
> *Fox (1995: 122)*

10.1 Introduction

Since traditionally the question of linguistic classification and reconstruction concerned itself with the genetic relatedness of "languages," and I have argued above that the sociopolitical concept of "language" does not provide a useful basis for empirical linguistic research, the question arises of whether linguistic reconstruction, subgrouping, and related issues can be pursued at all within the framework proposed here. Since I believe the Comparative Method – the primary tool of linguistic reconstruction, itself strongly dependent upon linguistic subgrouping and classification – has established its usefulness in empirical research with great regularity, if it turns out that the proposals made in chapter 1 fail to generate similar empirical predictions, they must be deficient in some serious way. In this chapter, I would like to demonstrate how the Comparative Method (and the related issues of classification and subgrouping) are to be made sensible within the framework adopted in this document and to show that these well-established tools survive – in perhaps slightly different (and in my view improved) guise – the conceptual transformation proposed in chapter 1.

In addition, since our theory of reconstruction methodology in a particular domain (e.g., phonology or syntax) is presumably to be developed with reference to our theory of change in that domain, this chapter will also discuss the implications of our treatment of phonological and syntactic change above for our theory of phonological and syntactic reconstruction.

10.2 The Genetic Hypothesis

The genetic hypothesis[1] is designed to account for observed similarities in the output of different grammars.[2] It claims that the grammars in question share at least a subset of their features[3] because they have acquired these features through inheritance from a common ancestor. It is a very costly hypothesis – making an extreme claim (the details of which will be covered below). As such, it is to be invoked only when other possible hypotheses which could, in principle, account for the observed similarities have been excluded. The alternatives to the genetic hypothesis are the hypothesis that the observed similarities are the result of "borrowing" and the hypothesis that the observed similarities are due to "chance."[4] In traditional terms, the genetic hypothesis consists of the following claims:

1. Grammar G_1 and grammar G_2 have some similarities.
2. The similarities are too numerous and too systematic to be due to chance.[5]

[1] Although it should not be necessary to point this out, the genetic hypothesis is not a theory about gene flow within human population groups. In fact, there is no reason, given what we know of the history of human civilization, to believe that there is any relationship between the physical transmission of genetic material from one generation to the next and the transmission of a grammar from one generation to the next. The two are completely independent of one another and only accidentally coincide in monolingual and monodialectal communities – which probably do not exist (and never have). I point this out only because one continues to see evidence from human genetic lineages cited as support for (or refutation of) theories of human linguistic lineages. This appears to be grounded on the assumption that at some point in the past humans lived a more isolated existence, giving rise to less intense or frequent language contact. The linguistic record, as well as the archeological record (to the extent it gives any linguistic information at all), is seriously at odds with this assumption. The term "genetic" in this context is derived not from "gene," but from the verbal base of "genesis," i.e., "origins."

[2] Technically, it should not restrict itself to output similarities and differences, as I have argued above, but rather should focus on similarities and differences in input representations and processes. For our purposes at this juncture the terminological simplification should not matter.

[3] Usually referred to as the "directly inherited" features.

[4] When the data are insufficient to allow exclusion of these two hypotheses, but also inadequate to adopt one or the other, the status of the relationship is indeterminate. Note that this is not equivalent, as it is often taken to be, to the claim that the languages may or may not be related. In the sense in which the term is used in linguistics, languages are related only if such a relationship is demonstrated empirically. Languages for which no such demonstration is possible are (until evidence comes to light which allows us to reevaluate the claim) unrelated.

[5] I include under the rubric "chance" instances of what Harrison (1986) calls "iconism," though it may be more precise to separate such cases as Harrison does. The similarities in

3. The similarities pattern in such a way that they are inconsistent with known borrowing patterns.[6]
4. The similarities can be accounted for by assuming that G_1 and G_2 are "descendants" of a common ancestor (the protolanguage, in this case, e.g., Proto-G).

I will not dedicate any additional effort to clarifying the empirical content of the components of the genetic hypothesis given as (1)–(3) above. The traditional conception of these notions is generally adequate, though regarding (3) a more heavily constrained theory of the possible effects of various types of contact would be helpful. The rest of this chapter will be dedicated to attempting to explicate the nature of the claim given as (4).

There are two central issues which must be dealt with if we are to make claim (4) an empirical one. What is a descendant–antecedent (or "ancestor") relationship between grammars? And what is a "protolanguage"?[7]

Protolanguages are reconstructed on the basis of the evidence provided by their "descendants" using the Comparative Method (a technique whose details are to be discussed in detail below). It is well known that the comparative method does not allow full recovery of all features of a given protolanguage; e.g., there may be (and invariably are, in my view) features which have been lost without a trace in all descendant grammars.[8] Such features will of course not be recoverable. Nevertheless, such features are assumed to have been present in the protolanguage (though their precise form will not be known). Thus reconstructed protolanguages can be assumed to have had features of two types: recoverable (let us call these features α_1, α_2, α_3, . . .), in that there is evidence for such features in the attested historical or living record,[9] and unrecoverable (let us call the unrecoverable

such cases (onomatopoeia, e.g.) occur at a higher rate than is typically found in other parts of the lexicon. Nothing prevents us, of course, from interpreting "too numerous and too systematic" as requiring sensitivity to our expectations for each lexical domain. The statement can probably stand as is, therefore.

[6] The borrowing hypothesis may also be weakened or strengthened by known facts about contacts (or lack thereof) between the two grammars or their "antecedents" – on which more below.

[7] The discussion to follow assumes general familiarity with traditional work on the Comparative Method, subgrouping, and the reconstruction of protolanguages. The questions above are geared towards the issue of how we are to derive such notions within the framework adopted in this document, rather than how traditional historical linguistics conceives of these matters.

[8] Where "without a trace" means, of course, without a trace sufficient to allow the feature(s) to be recovered. "Traces" of features which cannot be recovered could not, by definition, be identified as traces.

[9] The reader should be aware that "evidence for" the presence of a set of features in the protolanguage does not entail presence of those very same features in the attested daughters. The evidence of the daughters taken as a whole may point to features in their common ancestor which are not preserved as such in the daughters.

features β_1, β_2, β_3, . . .), in that there is no such evidence. Grammars antecedent to the attested record which differ only in "unrecoverable" features will of course be treated as identical for all historical linguistic purposes (since it cannot be known if, or how, they differed in features which cannot be recovered). Thus, taking a simple case of three descendant grammars (G_1, G_2, and G_3) with an uncomplicated transmission history,[10] the protolanguage for these grammars will be the set of all (chronologically) anterior grammars which do not differ in recoverable features.[11] They of course may or may not have differed in ways which the linguistic record does not allow us to recover. Such a case is represented schematically in figure 10.1 below.

The protolanguage is the sole box found at Stage I. It consists of a set of grammars (G_1, G_2, and G_3) which agree in all recoverable features (the agreement status of unrecoverable features is unknowable, by definition, and therefore irrelevant to all subsequent scientific use of the "protolanguage"). At Stage II of this figure, grammar G_1 shows a recoverable feature (α_x) not shared by G_2 and G_3.[12] That is, in the course of transmission of G_1 an innovation occurred, giving rise to the feature α_x. It can easily be shown (by a simple iteration of the schema in figure 10.1) that given a long enough period of transmission, helped along by the fact that each grammar need not be transmitted to only one descendant as well as by the fact that transmission is normally imperfect, the daughter grammars will become increasingly diverse. If the attested descendant grammars

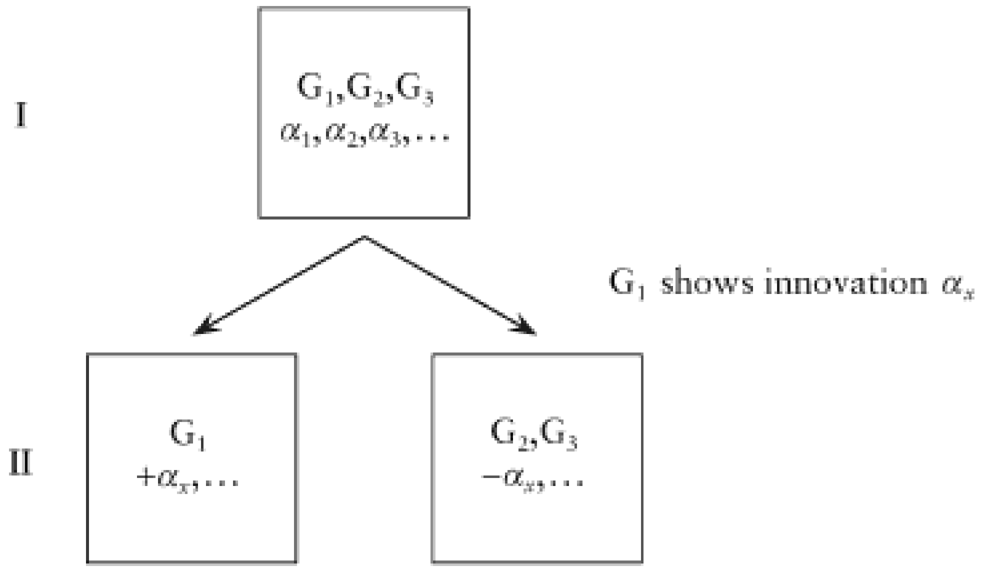


Figure 10.1: A simple innovation

[10] The precise meaning of "uncomplicated" in this context will be discussed in some detail below.

[11] The set may contain as little as one element, but the numbers will turn out to be quite irrelevant.

[12] It is assumed that the reconstruction of the protolanguage based on all daughters did not result in positing feature α_x for the protolanguage, i.e., the protolanguage was $-\alpha_x$. For the purposes of the discussion to follow, I will assume that the directionality of the change from $-\alpha_x$ to α_x is fixed, i.e., the change posited could not have proceeded in the other direction (from α_x to $-\alpha_x$).

grammars do not collapse into a single protolanguage in spite of the identity of (recoverable) features in the descendant grammars (G_1 and G_2 at Stage III). Figure 10.3 thus differs from figure 10.2 in that G_1 and G_2 collapse through the "masked" diffusion of figure 10.2, but remain distinct "branches" (i.e., lines of descent) in parallel independent development cases such as that sketched in figure 10.3.

The contrast between these two figures makes clear the key role that changes, as opposed to diffusion events, play in reconstruction and subgrouping methodology. Whereas a diffusion event is not, in a scenario such as the one outlined above,[22] relevant to linguistic subgrouping (and thus not to reconstruction either), all established changes are, since they introduce new lines of descent into the descent tree. Thus the difference between "trivial" and "nontrivial" innovations will be critical, since this contrast plays such an important role in distinguishing "masked" diffusion from parallel independent innovation.

10.4 Subgrouping

Moving on to slightly more complex scenarios, figure 10.4 presents a "family tree" diagram for a system in which two changes have taken place, each of which has diffused in a slightly different way.[23]

There is a critical issue involving the situation sketched in figure 10.4 which has not come up in the discussion thus far (since we have been considering changes in isolation only). The two changes posited for the system above, α_x in G_1 and α_y in G_2, may be changes which, if found in the same system, would interact in some way (e.g., one may feed or bleed the other), or they may be changes which could not interact. We will consider these cases in turn. For all cases we will assume that the protolanguage had the forms **kibi*, **sib*, and **bigit*.

Considering first the cases of two innovations which could potentially interact, we will assume that innovation α_x was the intervocalic lenition of voiced stops and innovation α_y was the loss of final vowels.[24] (See table 10.1.)

Although the first diffusion event, the spread of final vowel deletion from G_2 to G_3, "collapses" G_2 and G_3 into a single protolanguage (in part because it is a "masked" diffusion – i.e., impossible to figure out which language innovated and which borrowed), as discussed above, the second diffusion event (from G_1 to G_2) does not. It is quite clear, if our hypothesis that the intervocalic lenition in G_2 is

[22] We will show below that diffusion events can play a role in more complicated scenarios.

[23] It is clear that I am working with an assumption that there is a prestige hierarchy involved in these schematic examples, such that, in terms of prestige, $G_1 > G_2 > G_3$. All three systems are in sufficiently close contact that diffusion is possible.

[24] Both of these events are well-attested cross-linguistically.

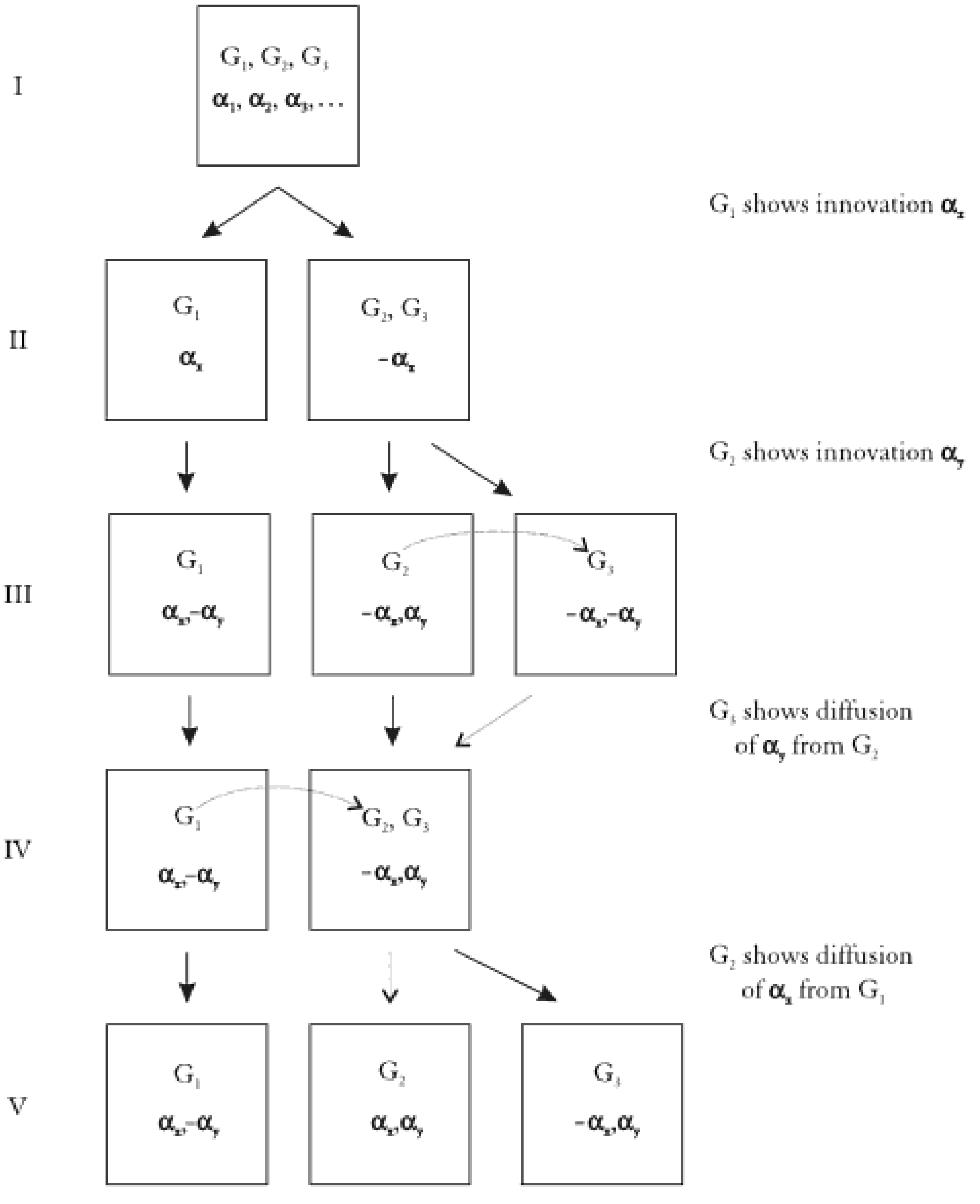


Figure 10.4: Two diffusion events

Table 10.1

	G_1	G_2	G_3
I	*kibi, sib, bigit*	*kibi, sib, bigit*	*kibi, sib, bigit*
II	*kiβi, sib, biɣit*	*kibi, sib, bigit*	*kibi, sib, bigit*
III	*kiβi, sib, biɣit*	*kib, sib, bigit*	*kibi, sib, bigit*
IV	*kiβi, sib, biɣit*	*kib, sib, bigit*	*kib, sib, bigit*
V	*kiβi, sib, biɣit*	*kib, sib, biɣit*	*kib, sib, bigit*

Table 10.2

	G_1	G_2	G_3
I	*kibi, sib, bigit*	*kibi, sib, bigit*	*kibi, sib, bigit*
II	*kiβi, sib, biɣit*	*kibi, sib, bigit*	*kibi, sib, bigit*
III	*kiβi, sib, biɣit*	*kebi, sib, begit*	*kibi, sib, bigit*
IV	*kiβi, sib, biɣit*	*kebi, sib, begit*	*kebi, sib, begit*
V	*kiβi, sib, biɣit*	*keβi, sib, beɣit*	*kebi, sib, begit*

the result of diffusion rather than parallel independent innovation is supported by the evidence,[25] that the change α_y *preceded* the change α_x in G_2 – that is, G_1 and the protolanguage (G_2, G_3) had already been differentiated by a change (different from the one which subsequently diffused) before the diffusion occurred. DIFFUSIONS WHICH TAKE PLACE INTO A PROTOLANGUAGE WHICH IS ALREADY DIFFERENTIATED FROM THE SOURCE BY SOME CHANGE OTHER THAN THE DIFFUSING ONE HAVE NO EFFECT ON SUBGROUPING RELATIONS. This is the reason why loanwords from, e.g., some variety of French play no role in the subgrouping of the English grammars which did the borrowing: the ancestors of the French and English grammars are already differentiated by a long series of recoverable change events.

We must now consider the case in which α_x and α_y do not show any interaction. Retaining the hypothesis that α_x is intervocalic lenition of voiced stops, consider a case in which α_y is the dissimilatory lowering of *i* (to *e*) before an *i* of the following syllable.[26] (See table 10.2.)

It is clear that, unlike the case outlined above in which the two innovations show a potential for interaction, there is no inherent ordering between the events α_x and α_y or their diffusions. Any ordering of the stages in the figure above (so long as the diffusion of an innovation follows that innovation, obviously) will produce the same results. Because of the ambiguity of the ordering of the innovations, it is impossible to determine a unique subgrouping for the three daughter grammars in such a situation. This is not, however, equivalent to subgrouping methodology producing no result: the application of strict subgrouping to this situation produces two, and only two, equally probable subgrouping hypotheses: on one, G_1 and G_2 form a subgroup (and the event α_y, shared by G_2 and G_3 is the result of diffusion either of subsequent innovation of G_2 into G_3, or of an innovation of G_3 into G_2); on the other, G_2 and G_3 form a subgroup by sharing

[25] It is for purposes of this discussion merely an assumption.

[26] I realize that one might not reconstruct the proper protolanguage from the resulting data set in this case – I will assume that the proto-*i*'s which become *e* are recoverable (and thus not reconstructed as *e*'s, e.g.) from the existence of a superordinate protolanguage.

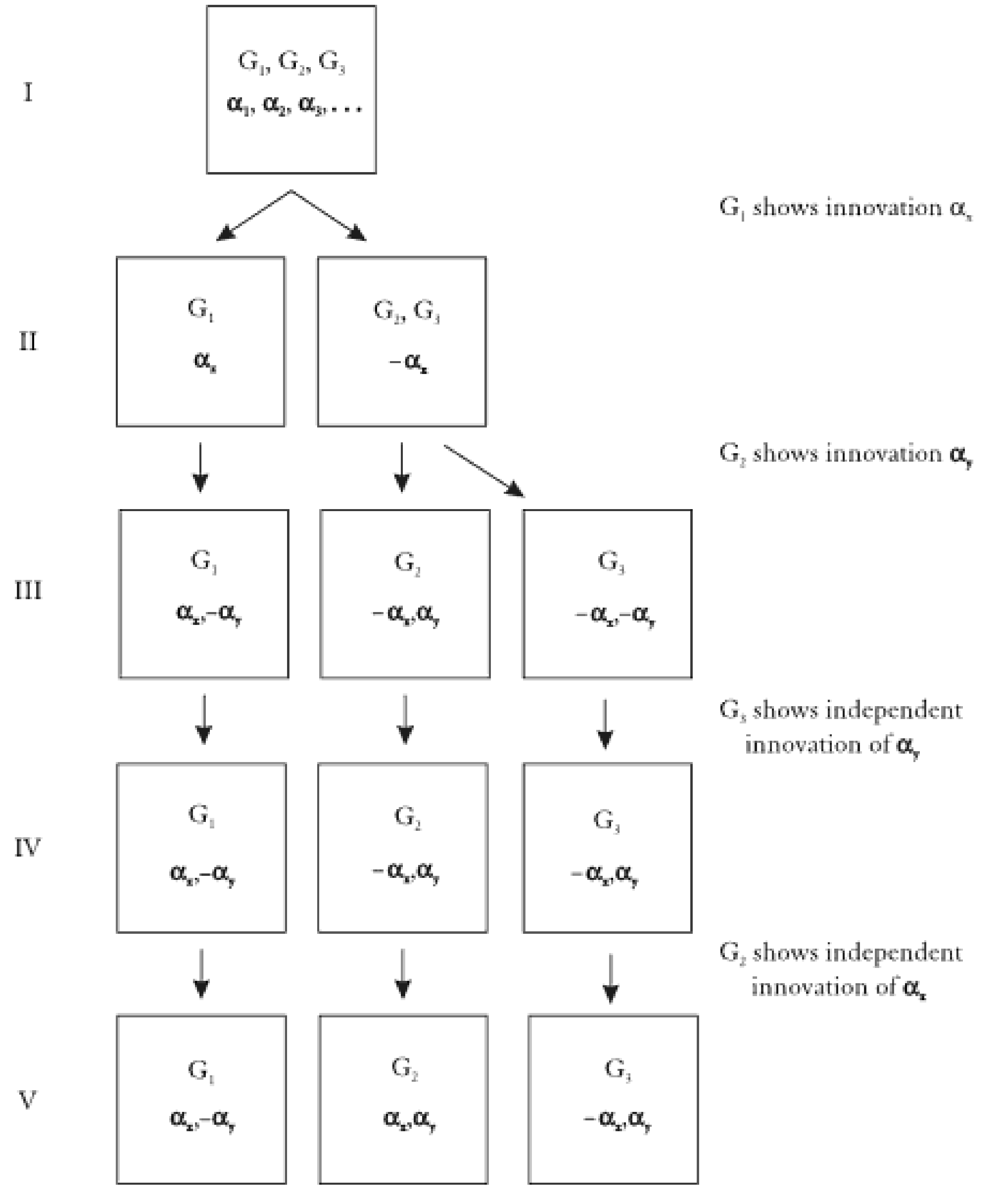


Figure 10.5: A set of parallel independent developments

the event α_y (and the event α_x is the result of diffusion of either a subsequent innovation of G_2 to G_1, or of a later innovation of G_1 to G_2).[27]

Thus when dealing with multiple innovations which subsequently propagate (the normal case for attested instances of nontrivial changes), only the evidence provided by rule interactions, which allows us to deduce ordering between the events, will generate unique subgroupings.

The situation is somewhat different when we modify the figure above slightly so that instead of diffusion events, it contains parallel independent developments. This is represented in figure 10.5.

As the reader can easily confirm in this figure, the forms of each grammar at each stage are identical to what they were in figure 10.4. The difference is that the evidence favors parallel independent innovations over borrowing in the

[27] All this assumes of course that we are dealing only with diffusion, not with parallel independent innovation.

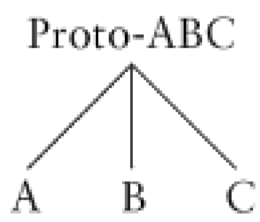


Figure 10.6: Proposed subgrouping with nonbinary lines of descent

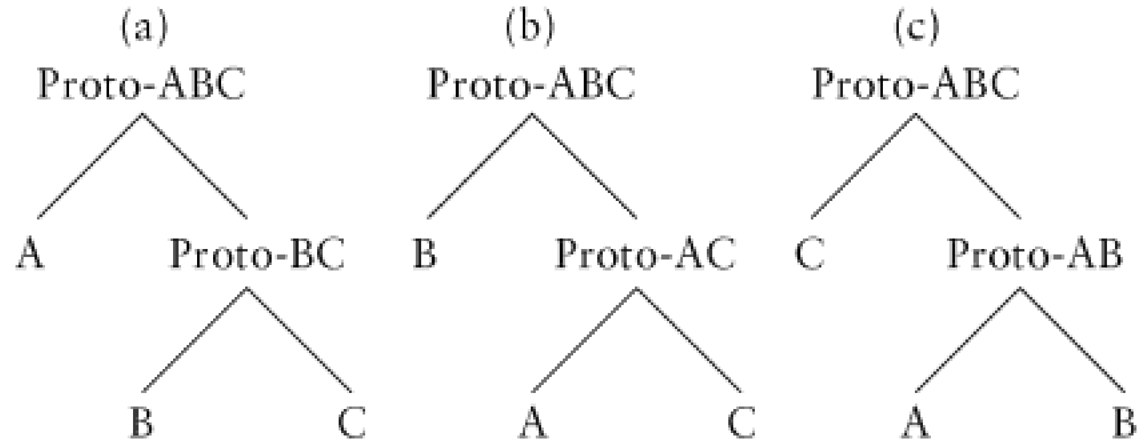


Figure 10.7: Expansion of a nonbinary descent tree

literature? A tree such as that in figure 10.6 in fact merely indicates that the order of the events (and each ordering would of course induce a different subgrouping) is indeterminate – i.e., the nontrivial changes which define the subgroups do not interact. It thus abbreviates either situation (a), (b), or (c) in figure 10.7.

I would propose that while the schematic in figure 10.6 appears at first sight to be a harmless simplification, the procedure of licensing nonbinary branching descent trees has two serious side-effects and should therefore be avoided.[31] The first negative effect of such figures is that they have given rise to the notion, presumably intended as mere metaphor by many scholars, of the "break-up" of the protolanguage. This metaphor – which implies the simultaneous dispersal of populations – is generally interpreted by nonlinguists (and occasionally by linguists) as a involving some social or even ecological catastrophe, some triggering cause. In fact, this kind of diversification (a series of changes whose precise order is unrecoverable) is found within those urban centers which have been subjected to detailed sociolinguistic study in the past 20 years – no movement, no "break-up," is required to trigger this type of diversification. It is a trivial and normal effect of grammar transmission.

The second problem which the "simplified" trees of the type in figure 10.6 give rise to concerns reconstruction methodology. It is to that problem that we turn next.

[31] Minimally, authors proposing such subgrouping should explain that such a tree reflects the indeterminacy of the ordering between the innovations involved, rather than representing an actual claim about the protolanguage.

of possible change, as well as the triviality index assigned a given change, be derived from a principled account of how a human organism, confronted by a set of linguistic data, might construct a grammar to produce that data (and how such an instance of grammar transmission might go awry). The principles which should be developed will not be language-specific, since the human "responsible" for the changes has, at least in the case of first-language acquisition, no language-specific information before the data in question is presented to him/her.

The second major criticism of reconstruction methodology which has surfaced with some regularity is that under the traditional approach it assumes a homogenous, undifferentiated protolanguage speech community. Since we know that such things do not exist, we are reconstructing something that never could have been. Lichtenberk (1994) provides a detailed criticism of traditional methodology from this perspective, pointing out an instance in which he believes it possible to demonstrate that a particular "protolanguage" was nonhomogeneous. This is, in my view, quite a valid criticism of traditional reconstruction methodology which operates with the notion "language."[37] We will now turn to a fuller consideration of Lichtenberk's challenge to a core assumption of traditional reconstruction methodology: the belief that the reconstructed language is homogeneous.

10.7 "Realist" and "Formalist" Views of Reconstruction

For most of its history, it has been a core assumption of comparative linguistics that reconstructed protolanguages were dialectally uniform, indeed, that the method necessarily gives rise only to protolanguages with this property. Fox (1995: 135) summarizes the standard reasoning in this area:[38]

[37] One wonders how scholars working with such a method feel they can reconstruct the sociopolitical context of the protolanguage with sufficient confidence to know whether or not it was "a language" – note that Lichtenberk, while confronting traditional methodology, apparently accepts this, its weakest, assumption. He asserts that the differentiated speech area he assumes to have been the "protolanguage" was a "language" with "subdialectal" variation. These terms are never, however, defined.

[38] It is not at all clear *how* Fox believes "a protolanguage with dialectal differences" could ever be reconstructed, given the methods used by comparativists which he so carefully lays out in his book, let alone be iteratively produced by that method in the manner his thought experiment seems to envision. Nevertheless, it is certainly the case that for any instance in which someone might *propose* protolanguage dialect differentiation, one could respond to that proposal – which as far as I can see could never result from the application of the Comparative Method – by engaging in the cyclic reduction of dialect differentiation he outlines.

Cont. on p. 245

> variability is incompatible with the *Stammbaum* ["family tree" – MRH] model, and with the Comparative Method itself. Given a protolanguage with dialectal differences the method could in principle be applied again to the different dialect forms, on the assumption that any such dialects would in turn be derived from a single unified source, and a further protolanguage would be reconstructed. The procedure would be repeated until all dialect variation was eliminated. The existence of dialects *within* the protolanguage would thus seem to be in principle excluded by the operation of the method.

Converted into the terminology we have been using in this chapter this means simply that, since the protolanguage is the set of attributes reconstructable for a point in diachrony *before* there were any recoverable changes which serve to differentiate the daughter languages, there can be no recoverable dialects for a protolanguage (since the existence of such dialects would entail that there had already been recoverable differentiating changes, and thus that the entity under discussion is not in fact a protolanguage).

Fox considers the assumption of uniformity of protolanguages, in light of evidence (e.g., from our general understanding of sociolinguistic reality) for variability, "an embarrassment to the Comparative Method." He deals with this embarrassment by essentially declaring protolanguages to be "abstractions" and "necessary idealizations" from "reality." Indeed, he warns against confusing the two spheres:

> What we are *not* entitled to do, of course, is to mistake our idealizations for reality. It is all too easy to interpret our idealization of reality as though it were reality itself, and to draw inappropriate conclusions on this basis. Thus, because our method of reconstruction demands a tree model of linguistic relationships we may – as indeed earlier linguists did – wrongly assume that the relationships are actually of this form.

It is hard not to wonder, if the protolanguages we posit are not consistent with reality, and the family trees we construct are similarly "counterfactual" (Fox uses this word, 1995: 140), why we would, or should, bother doing the extensive work required to establish them. Fox addresses this concern in a couple of different ways, none of them particularly comforting. For example, he notes that "no method can be expected to be infallible or universally applicable" and that we have no alternative method at this time. I don't agree with the former claim, and

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It should not go uncommented upon that Fox's discussion (1995: 135ff.) of some possible cases when dialect differentiation might be postuled for a protolanguage, "even with the application of the Comparative Method," are not probative. For example, his treatment of the case of Indo-Iranian voiceless aspirates is factually wrong on numerous points (it is not the case that they are attested only in Indo-Iranian, nor is it the case that "there is, in fact, no conclusive evidence to enable us to derive these sounds from others by regular processes"). The voiceless aspirates arise regularly from sequences of voiceless stops + $*H_2$.

the latter is hardly a defense for continuing to use known bankrupt methodology – surely, if we know our methods to give counterfactual results and yet have no alternative at this time, our efforts should be being invested in the task of developing more reliable methods, rather than continued application of our existing faulty ones. He notes additionally that, after all, the Comparative Method, for all its weaknesses, appears to *work*. Unfortunately, he seems not to realize that the fact that it *works*, if it gives results which consistently and necessarily deviate from "reality," is more a problem in need of explanation than a felicitous fact.

Lichtenberk approaches his critical assessment of the assumed homogeneity of protolanguages (1994) by contrasting two positions which have been adopted by historical linguists about the status of reconstructed languages:

> Do we assume that at some time in the past there really was a language that had the properties that we have reconstructed (the realist view), or is such an assumption irrelevant to our concerns (the formalist view)? (Lichtenberk 1994: 1)

It would appear that Fox would qualify as holding the "formalist" view – thus accounting for his assertions that protolanguages are inconsistent with reality. Historically, it has been common, under the "formalist" view, to see a reconstructed entity as a mere formal "shorthand" for the correspondence set which justifies the reconstruction. A protolanguage, under such an assumption, is merely the set of all such abbreviatory conventions of this type which can be constructed for a given set of related languages. By contrast:

> On the realist view, reconstructed protolanguages are viewed not as formal devices but as real entities, as real as the languages around us. (Lichtenberk 1994: 2)

Interestingly, it should be clear from all that you have read in this book so far that like Lichtenberk, I favor a "realist" conception of protolanguages – they are, indeed, "as real as the languages around us." Of course, unlike Lichtenberk, I believe that "the languages around us" are grammars, i.e., computational devices in the mind/brain of individuals.[39] In what follows, we will see from Lichtenberk's example, I believe, the kinds of difficulty one creates for oneself when one adopts the "sociopolitical" conception of "language" and attempts to do comparative linguistics.

10.7.1 Some data

Lichtenberk draws his data from the Cristobal-Malaitan family of languages which are spoken on the Solomon Islands. The subgrouping of the languages is

[39] It is true, as has been sketched above in some detail, that protolanguages are only partially recoverable given available evidence and existing techniques – but, of course, this holds of synchronic mental grammars as well, at least for the present.

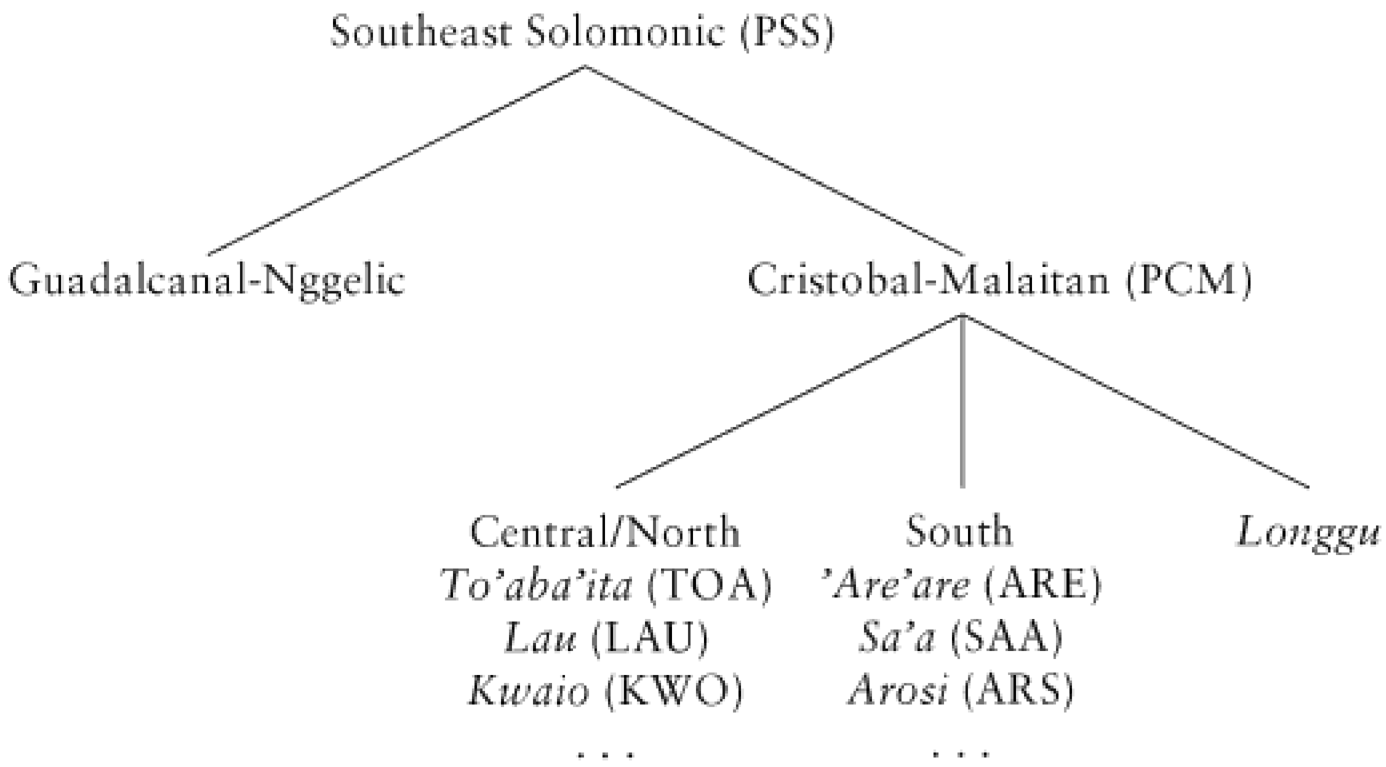

Figure 10.9: The Cristobal-Malaitan family

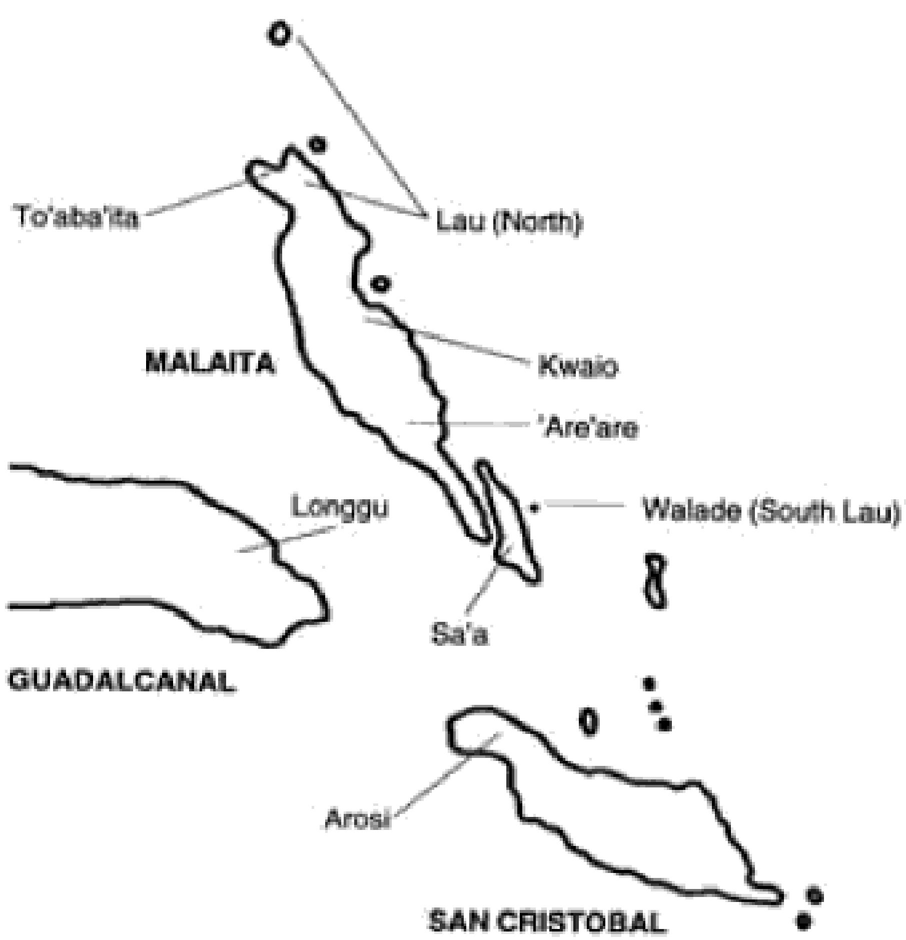

Figure 10.10: The geography of the languages under discussion

given by Lichtenberk as in figure 10.9. The geographical distribution of the languages can be seen in the map presented in figure 10.10. These languages show a number of well-established changes from Proto-South Solomonic, including the loss of PSS ***t**, via a process which Lichtenberk calls, sensibly enough, *t*-loss. Representative examples are given in table 10.3.

After *t*-loss had taken place, PSS ***s** developed in t in the San Cristobal-Malaitan languages whenever it was followed by a nonhigh vowel. PSS ***s** remains s before high vowels, so Lichtenberk labels this change *s*-split. That *s*-split must

Table 10.3

PSS	*TOA*	*LAU*	*KWO*	*SAA*	*ARE*	*ARS*
*tolu 'three'	ulu	olu	olu	oru	—	olu
*mate 'die'	mae	mae	mae	mäe	mae	mae

PSS **t* > Ø in PCM ("*t*-loss")

Table 10.4

PSS	*TOA*	*LAU*	*KWO*	*SAA*	*ARE*	*ARS*
*sala 'path'	tala	tala	tala	tara	tala	tara
*susu 'breast'	susu	susu	susu	susu	susu	susu

PSS **s* > *t* / _V[–hi] in PCM ("*s*-split")

have followed *t*-loss is given by the fact that it does not feed the loss of *t* – that is, **t**'s created by *s*-split are not subject to loss by *t*-loss. Again, representative examples are given in table 10.4.

There are two additional changes of relevance to our discussion. Lichtenberk posits a process which he calls "*y*-prothesis." At first blush, the choice of name here is somewhat strange, since the prothetic consonant involved has a variety of forms, including **θ**, **s**, **r**, and **l**. None of these reflexes have the fortune of being **y**, however.[40] The prothesis takes place only before ***a**. Representative examples are given in table 10.5.

Table 10.5

PSS	*TOA*	*LAU*	*KWO*	*SAA*	*ARE*	*ARS*
*a- an article	θa-	sa-	la-	sa-	ra-	sa-
*ansa 'grate'	θata	sata	—	sataaʔi	—	—
*aRu 'Casuarina tree'	—	salu	lalu	sälu	raru	saru
*ane 'termite'	—	sane	nale	sane	nare	ane

y-Prothesis before word-initial **a*

[40] In spite of the parallel development of Proto-Oceanic (POC) **y* – on which, see table 10.6 – this is not what *I* would reconstruct in this situation. Lichtenberk 1988 reconstructed **θ*.

Table 10.6

PSS	*TOA*	*LAU*	*KWO*	*SAA*	*ARE*	*ARS*
*koyo 'go through'	koθo	koso	kolo	—	kokoro	—
*ʔayai 'mango'	[ʔasai]	asai	ʔalai	asai	arai	ʔaai
	PSS *y in San Cristobal-Malaitan					

Table 10.7

PSS	*TOA*	*LAU*	*KWO*	*SAA*	*ARE*	*ARS*
*ʔatu 'bonito'	θau	sau	lau	säu	rau	sau/atu
*ʔansan 'name'	θata	sata	lata	sata	rata	ata
*ʔatop 'thatch'	θao	sao	lao	sao	rao	ao
PSS *ʔ > Ø in PCM (ʔ-loss) [with subsequent *y*-prothesis]						

To see why Lichtenberk chose the name *y*-prothesis, one may compare the development of PSS *y (table 10.6). Finally, Lichtenberk presents evidence that PSS *ʔ was lost in initial position, and that this loss fed the process of *y*-prothesis. We can see some of that evidence in table 10.7.[41]

The changes we have discussed have a well-defined relative chronology, which can be summarized as follows:

- Relative Chronology:
  1 *ʔ*-loss
  because it feeds *y*-prothesis

[41] In Arosi, the "expected" form of **ʔatu*, *sau*, is in widespread use only in personal names, the "borrowed" form *atu* being the general term. On a possible prothesis-less reflex, *waiau* 'tuna,' Lichtenberk (1994: n. 17) says the following:

> Arosi also has the form *waiau* 'tuna', which – despite appearances – does not contain a reflex of **atu*. According to Barnett (1978: 79), *waiau* literally means '(it) takes me' (*wai* 'take', *au* 1sg pronoun), and it "denotes the compelling need to get amongst a school of bonito."

Never having felt this "compelling need," I myself would be inclined to see a folk etymology here (unless Barnett dreamed up this analysis on his own, in which case it's that peculiar kind of folk etymology where by "folk" we mean "linguist with an overly vivid morphological imagination").

2 *y*-prothesis
because it is not fed by *t*-loss.
(POC *taliŋa > TOA,LAU,KWO,SAA *aliŋa*, ARE *arina*; POC *ta(n)sik > TOA, LAU, KWO, SAA, ARE, LGU *asi*; POC *taŋis > TOA, LAU, LGU *aŋi*; POC *tampu 'sacred, forbidden' > TOA, LAU, KWO *abu*, SAA *äpu*, ARE *apu*)
3 *t*-loss
because it is not fed by *s*-split (see *sala 'path' above).
4 *s*-split

So far, the situation seems to be of a type which readily lends itself to analysis using the standard tools of comparative linguistics. However, Lichtenberk notes a difficulty regarding the data for each of these changes:

> while *t*-loss and *s*-split are nearly perfectly regular throughout Cristobal-Malaitan, the distribution of the prothetic segments in the present-day languages is far from regular.

Evidence to support this assertion can be seen in table 10.8.

It seems unlikely that the difference in percentage of reflexes showing prothesis in Kwaio (KWO), Lau (LAU), 'Are'are (ARE), and Sa'a (SAA) is statistically significant (a fact which Lichtenberk does not comment upon); however, the point Lichtenberk is making persists: generally speaking, as one moves from north to south, there is less and less of the expected prothetic consonant development, although *t*-loss and *s*-split appear to be essentially categorial throughout the region.

Lichtenberk has presented two different analyses of this phenomenon. The first, in 1988, involved incompleteness of *y*-prothesis (at that time analyzed by Lichtenberk as *θ*-prothesis) throughout the area:

> Theta-prothesis was a lexically and geographically gradual change, which died out before it had had a chance fully to affect the whole of the PCM area. This means that after the demise of the prothesis PCM was not a homogeneous language. It

Table 10.8

Language	*total possible prothesis cognates*	*percentage showing prothesis*
TOA	41	90.2
KWO	61	67.2
LAU	78	65.4
ARE	71	64.8
SAA	70	60
ARS	82	20.7

> was comprised of a number of dialects characterized by the different degrees to which they had been affected by theta-prothesis. In spite of this complex situation, the area where Early PCM was spoken was still cohesive enough for the language as a whole uniformly to undergo *t*-loss and *s*-split. (Lichtenberk 1988: 54)

In 1994, Lichtenberk favors instead an analysis under which *y*-prothesis was regular and across-the-board, but a process of *y*-loss began and failed to spread uniformly throughout the area.

> One possibility – the one that will eventually be adopted here – is this: sometime after *y*-prothesis had taken place, another change began to operate in PCM whereby *y* – prothetic or not – was lost. This change originated in a certain part of the PCM-speaking area, and it was implemented gradually: it began to diffuse lexically, through the eligible lexical items, and areally, from the area where it had originated. *y*-loss was incomplete; it ceased to operate before affecting the whole of the eligible lexicon and the whole of the PCM-speaking area, thus leaving a residue. The present-day instances of the zero reflex are the result of *y*-loss; the full reflexes continue the residue. (Lichtenberk 1994: 11)

It makes little difference which of these analyses we adopt for discussion here, since the methodological point Lichtenberk is raising remains the same in either case. *t*-loss and *s*-split should be attributed to Proto-San Cristobal-Malaitan, since they are shared by all of its daughters, yet they postdate what we would call in this book a "differentiating" sound change (i.e., one which left recoverable reflexes in the daughters). The relevant claim of Lichtenberk's is stated quite clearly in his 1994 paper:

> The incompleteness of *y*-loss resulted in regional variation with respect to the presence and absence of **y*, but this variation did not amount to dialect differentiation. There were no isoglosses dividing PCM into dialects on the basis of the fate of **y* . . . However, even though the result of the incomplete loss of *y* was not dialectal differentiation, there was areal heterogeneity. Different areas where PCM was spoken had been affected to various degrees by loss of *y*. (Lichtenberk 1994: 17)

The key issue here is the concept, for which I can imagine no justification on theoretical or practical grounds, that the "regional variation" which resulted from *y*-loss (or θ-prothesis, under the 1988 analysis) "did not amount to dialect differentiation." Indeed, as near as I can tell, "regional variation" is the *definition* of (geographical) dialect differentiation. Lichtenberk has uncovered a very interesting case of "convergence" – i.e., common development based on language contact *independent of genetic considerations.* Such developments do not make English a "Romance" language, and they do not make *t*-loss and *s*-prothesis – which demonstrably took place *after* a change not shared by all Proto-San Cristobal-Malaitan daughters – anything but *irrelevant*, like all recoverable convergence events, for genetic questions such as subgrouping and reconstruction.

10.7.2 General considerations

What is a "real" language, then, and are "protolanguages" an example of such objects? Here's is how Lichtenberk characterizes a "real language":

> In the realist approach, protolanguages are viewed as having both spatial and temporal extensions. Like present-day languages, they were spoken over a certain area, and they had their own histories; for example, they may have undergone sequentially related sound changes. (Lichtenberk 1994: 2)

Within the perspective I have been developing in this book, we would say rather that a "language" is a "grammar." A "grammar" is a property of an individual mind/brain. Except in the trivial sense that people actually take up "a certain area" (which I don't think is what Lichtenberk has in mind), much of what Lichtenberk labels "realist" is thus not.

10.7.3 Conclusion

- Recoverable protolanguage diversification is excluded in principle under certain assumptions about the natures of (1) the object of linguistic study and (2) the comparative method.
- If one doesn't like the assumptions outlined here *and* one wants to exclude protolanguage dialects, one must find some *principled* way to get that result. One may not simply assert as a "methodological add-on" that protolanguage dialects are precluded.

As I have shown above, the problem of protolanguage dialects does not arise under the conception of protolanguages outlined in this chapter. The protolanguage is underdifferentiated (*vis-à-vis* current spoken languages) because not all of the features of the grammars included within it are recoverable, not because there was no variation. Any variation that is recoverable (as in Lichtenberk's case) simply makes it clear that the entity in question is not the protolanguage, but a set of already differentiated daughters of the protolanguage.[42]

10.8 Final Remarks

The conception of subgrouping (and reconstruction) outlined above differs in a very crucial respect from traditional notions. This difference can clearly be seen from the following quote from Harrison (1986: 15):

[42] This point is made explicitly by Eichner (1988).

Part V Concluding Remarks

11 Synchronic and Diachronic Linguistics

> Change is inevitable, except from vending machines.
>
> *Anon.*

11.1 The Mirage of Apparent Identity

It seems clear from the contents of this book up to this point that there is, in my opinion, a great deal of confusion about the precise demarcation of historical linguistics relative to related enterprises like sociolinguistics, synchronic linguistics, and the study of language acquisition. The reason for this can be seen in part from an examination of the phenomena in (11.1)–(11.4) below.

(11.1) $l \rightarrow w$ in codas

(11.2) $l \rightarrow w$ in codas

(11.3) $l \rightarrow w$ in codas

(11.4) $l \rightarrow w$ in codas

Now, I would not be at all surprised to learn that you are hard-pressed to see the subtle differences between these examples which seem, in fact, identical (except for their labeling number), and, indeed, should be identical, since they were created by a cut-and-paste operation. It is precisely this confusing appearance of what looks like identity that has been the root of many conceptual difficulties, not just regarding historical linguistics, but in the proper demarcation of all four fields represented by these examples. In (11.5)–(11.8) below I tell you what each of the examples above is intended to represent.

(11.5) My speech *c.* 1959: /bɛl/ pronounced [bɛw]
(i.e., a cross-linguistically common rule of so-called "child phonology")

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