

**The  
Comparative  
Method Reviewed**

**REGULARITY AND IRREGULARITY IN LANGUAGE CHANGE**

EDITED BY  
MARK DURIE  
MALCOLM ROSS

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*Edited by*

Mark Durie

Malcolm Ross

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# Preface

Comparative linguistics tends to be a conservative field; within the context of the latter, this book contains much that will be controversial. Historical reconstruction relies upon the comparative method, which itself crucially depends upon the assumption of the regularity of change. This regularity includes the famous “Neogrammarian Hypothesis” of the regularity of sound change: “sound change takes place according to laws that admit no exception.” However, the comparative method is not restricted to the consideration of sound change, nor is the assumption of regularity thus limited: syntactic, morphological, and semantic change all are amenable, in varying degrees, to comparative reconstruction, and each type of change is constrained in ways that enable the researcher to distinguish between what are, in some sense, regular changes and irregular or exceptional changes.

The notions of “regularity” and “irregularity” are controversial ones, and so this volume takes as its focus regularity, irregularity, and the comparative method. It brings together a set of empirical studies that provide a forum for theoretical and practical discussion of the limitations and potentials of the comparative method. These studies also include applications of the comparative method under challenging conditions.

The emphases of this volume distinguish it from many standard treatments of the comparative method. An introductory chapter provides a critical commentary on these differences, the most important of which include the following: there is a preponderance of non-Indo-European comparative data; some challenging implications of more “exotic” kinds of language contact situations are explored; detailed treatments are offered of issues involved in semantic and morphological change and reconstruction; and a central group of chapters explores different kinds of irregularity in sound change and the diverse motivations for such changes, all in the context of comparative reconstruction.

Special care has been taken to provide worked examples of comparative data, with an eye to both theoretical and methodological implications. Chapters are intended to be accessible to various types of readers; they have been written in a way which assumed no knowledge of any specific language family. At the same time, a very general understanding of the comparative method is assumed, since this volume is not intended as an introduction to the method itself. It should be suitable as a source of readings in a comparative-historical linguistics course. The at times controversial formulations of its contributors should prove thought-provoking and challenging.

The original idea for this volume was conceived by Mark Durie in 1987. Toward the end of 1988 he invited a group of prospective contributors to participate. An early plan for a conference on the subject, to be held in Australia, proved impractical for both financial and logistical reasons. Instead, the project was rethought and a collec-

tion of essays was the result. In 1992 Malcolm Ross agreed to become coeditor of the book, and the final selection and editing of essays was undertaken jointly by Durie and Ross.

Many thanks go to all the contributors, some of whom waited years to see their work appear in print. The encouragement of Cynthia Read, our acquiring editor at Oxford University Press, has been much appreciated. Robert Blust provided valuable initial encouragement and helpful suggestions about how to proceed.

*Parkville, Australia*  
*Canberra, Australia*  
*April 1995*

M.D.  
M.R.

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# 1 Introduction

MALCOLM ROSS AND MARK DURIE

## 1 PRELIMINARIES

This book is about the classical comparative method of historical linguistics.<sup>1</sup> When historical linguists talk about the ‘comparative method’, what they usually have in mind is not just a method but also an associated theory, and the contributors to this volume are no exception to this generalisation. For over a century a mainstay of the associated theory has been the ‘Neogrammarian hypothesis’, set out by Osthoff and Brugmann (1878) in the manifesto which sought to bring scholarly rigour to historical linguistics: ‘[E]very sound change, inasmuch as it occurs mechanically, takes place according to laws that admit no exception.’ (translated in Lehmann 1967: 204).

This association between method and theory is captured in the title and subtitle of this volume: *The comparative method reviewed: Regularity and irregularity in linguistic change*. We find it convenient at various points in this introduction to distinguish between the comparative method in this ‘wide’ sense (the method with its associated theory) and the comparative method in a ‘strict’ sense (the method itself).

Each of the contributors has had long experience of working with the comparative method (in the wide sense) in one or more language families. In order to keep the connection between method and theory as strong as possible, contributors were asked to choose topics associated with the method which their work had brought to the fore, focusing on regularity and irregularity in linguistic change. The result is a collection of empirical studies which provide examples of applying the comparative method in its strict sense, together with discussions of the results of these applications and their bearing on the comparative method in its wide sense.

In the introduction we have drawn together a number of threads from our contributors’ discussions in a way which is intended to be provocative, believing as we

do that this is a time of theoretical change and the right time for a review of the comparative method in its wide sense. Thus, we hope that the contributions to this volume, including the introduction, will provide material for the ongoing debate about the Neogrammarian hypothesis and other theories associated with the comparative method.

The range of language families from which the contributors draw their primary data is wide. It includes two subfamilies of Indo-European, Slavic (Nichols) and Germanic (Durie); the Mayan family of Native American languages (Campbell); Chinese (Newman); several geographical regions within Austronesian, namely, its western region (Blust), Papua New Guinea (Ross), and New Caledonia (Grace); Papuan (Ross); and the Pama-Nyungan family of Australia (Koch, Wilkins).<sup>2</sup> The contributions here are all concerned, in one way or another, with issues of method. Chapter 2 examines what the comparative method *is* and, importantly, what it is not. Chapters 3 through 7 are all concerned with cases in which the data appear to challenge the Neogrammarian hypothesis. Chapter 8 looks at certain data (outcomes of language contact) which the comparative method traditionally ignores, and chapters 9 and 10 examine the application of the comparative method to morphology and to semantics, areas which have received only limited attention in previous discussions of the method.

The body of this introduction falls into two parts. In the first we examine the steps of the comparative method in its narrow sense and indicate how the contributions to this volume tie in with those methodological steps. In the second we take the suggestions and implications contained in all the contributions to this volume—that the comparative method in its wide sense is undergoing a paradigm change—and seek to discern the outlines of the coming paradigm.

## 2 THE COMPARATIVE METHOD (NARROW SENSE): WHAT IT ISN'T AND WHAT IT IS

The contributions to this volume are all concerned with the linguistic comparative method. Unfortunately, there are several extant misunderstandings about what the comparative method in its wide sense is, and we will refer to five of these before we proceed.

The first misunderstanding equates the comparative method with its application to Indo-European languages. Today, perhaps, this misunderstanding prevails only in some European universities, where the 'Department of Comparative Linguistics' is in fact a department of comparative Indo-European linguistics. The equation is also reflected in the title of Szemerényi (1990), *Einführung in die vergleichende Sprachwissenschaft* ('Introduction to Comparative Linguistics'), which is not about comparative linguistics or comparative methodology, but is an introductory survey of work in Indo-European comparative linguistics. Hopefully the range of language families to which the comparative method has been applied by the contributors to this volume will give the lie to this equation.

The second misunderstanding confuses the comparative method with the techniques of lexicostatistics and glottochronology. Lexicostatistics is the use of the percentages of assumed cognates (that is, items in related languages which are directly

inherited from a common ancestor) shared by pairs of languages on a standard word list to arrive at a 'family tree' of those languages. Although its outcome—a family tree—looks like one of the outcomes of the comparative method, the lexicostatistical method is different in both practice and principle from the comparative method (despite occasional claims by lexicostatisticians that they are practising the comparative method; e.g. Dyen et al. 1992) and there is sometimes a radical difference between the family trees attained by the two methods.<sup>3</sup> Glottochronology is a technique for dating the nodes in the family tree and has no equivalent in the comparative method.

The third misunderstanding is a more dangerous one. It confuses the comparative method with the technique of 'multilateral comparison', sometimes known as 'mass comparison', used, for example, by Greenberg (1987) in his work on the linguistic prehistory of the Americas. We will return to this later. Suffice it for now to say that multilateral comparison is not a variant of the classical comparative method of historical linguistics.

The fourth misunderstanding confuses the comparative method with the typological comparison of the phonological, morphological, and syntactic systems of different languages. This kind of comparison has assumed an increasingly important role in linguistics in the three decades since Greenberg (1963b) was published, but it is different in method and objectives from the comparative method. Practitioners of the comparative method compare languages and use the results of this comparison to reconstruct their prehistory, whereas the goal of typological comparison is to determine the universal parameters along which languages vary (see, for example, Comrie 1989 or Croft 1990). Typological comparison also has a diachronic dimension: the study of how typological change occurs in linguistic systems. Work in this area has tended to focus on morphosyntax (see, for example, Hopper & Traugott 1993). Obviously universals of synchronic systems and of system change are vitally important to historical linguists because they provide criteria for assessing the plausibility of a reconstructed protolanguage (Comrie 1993). But typological comparison itself is not a part of the comparative method, and attempts to use typological comparison to establish genetic relationships simply do not work (see below [sec. 2.1] and Ross, this volume).

The fifth misunderstanding has to do with typological comparison of another kind and with the fact that the term 'comparative method' is used in different ways in different disciplines. For practitioners of disciplines other than linguistics, especially archaeologists and ethnologists, the significance of the linguistic comparative method is that it provides independent evidence about human prehistory and culture history. Its independence resides in the fact the linguistic comparative method (in its wide sense) is radically different from the comparative method of ethnological reconstruction. The latter is based on typological similarities, whereas the linguistic comparative method operates with arbitrary historical particulars. These particulars are linguistic signs: morphemes and their lexical collocations which a group of related languages has inherited from a common ancestor. It is a characteristic of linguistic signs that their signifiers are made up of phones which have no intrinsic meaning. As a result, the morphemes of a language (other than onomatopoeic and certain other marginal types of morpheme) show arbitrariness in regard to what phoneme sequence is used to represent what meaning. Since each language has at least a dozen, and more

usually several dozen phonemes, and the rules for building phoneme sequences in any given language allow for a huge inventory of possible morphemic forms, two different languages are unlikely to have by chance any more than a miniscule percentage of (nonmarginal) morphemes which match each other in form and meaning (see Nichols, this volume).

A feature of language which is especially important for the linguistic comparative method is that, if the sounds of a language change over time, they do so in a largely regular manner (the word 'largely' here is perhaps controversial, and we will return to it later), such that sound *x* becomes sound *y* under storable conditions not just in some morphemes but in all morphemes that meet these conditions.<sup>4</sup> The result is that the sounds of reasonably closely related languages will correspond to each other in a regular manner, and this regularity allows the linguist to identify cognate items and to distinguish these from borrowings and accidental similarities. There is effectively no chance that a correspondence like the one exemplified in the initial consonants of Ancient Greek *pod-*, Sanskrit *pad-*, Gothic *fōt-*, Modern German *Fuss*, all meaning 'foot', Ancient Greek *pénte*, Sanskrit *pañča*, Gothic *fimf*, Modern German *fünf* 'five', and Ancient Greek *polús*, Sanskrit *pulu-*, Gothic *filu*, Modern German *viel* 'much, many', has arisen by accident. The same is true of the correspondence of the root-final consonant of the 'foot' set and of 'eat': Ancient Greek *ed-*, Sanskrit *ad-*, Gothic *it-* and Modern German *ess-*.

Once the sound correspondences among earlier written languages (in the case of Indo-European mentioned earlier) and/or among contemporary languages (in the cases of language groups with no written records) have been charted, the sound systems of earlier stages (protolanguages) can be inferred by established procedures. From data of which we have given a very small sample, historical linguists reconstruct the Proto-Indo-European morphemes *\*pod-/ped-* 'foot', *\*penk<sup>w</sup>e* 'five', *\*plu-/pelu-* 'much, many', and *\*ed-* 'eat'.<sup>5</sup> These reconstructions provide the baseline for working out which languages share which innovations relative to their reconstructed ancestor. For example, Gothic and German (and English, Friesian, Dutch, Afrikaans, Danish, Swedish, Norwegian, and Icelandic) share an innovation whereby Proto-Indo-European *\*p-* became *f-*. They also share several other innovations, indicating that they are descended from a more recent common ancestor, Proto-Germanic, in which these innovations occurred. As such, they constitute a genetic subgroup of the Indo-European language family.

Innovations diagnostic of a subgroup are not always regular sound changes. Other kinds of significant innovation include an idiosyncratic change in the form of a particular morpheme, the replacement of one form by another, or any type of change in which the chances of independent parallel innovation are tiny. The technique of subgrouping by shared innovations is used recursively to identify subgroups within subgroups, that is, to construct what is conventionally called the 'family tree' of a group of related languages.

The comparative method (in its strict sense) can be summarised as a set of instructions:

1. Determine on the strength of diagnostic evidence that a set of languages are genetically related, that is, that they constitute a 'family';

2. Collect putative cognate sets for the family (both morphological paradigms and lexical items).
3. Work out the sound correspondences from the cognate sets, putting ‘irregular’ cognate sets on one side;
4. Reconstruct the protolanguage of the family as follows:
  - a. Reconstruct the protophonology from the sound correspondences worked out in (3), using conventional wisdom regarding the directions of sound changes.
  - b. Reconstruct protomorphemes (both morphological paradigms and lexical items) from the cognate sets collected in (2), using the protophonology reconstructed in (4a).
5. Establish innovations (phonological, lexical, semantic, morphological, morphosyntactic) shared by groups of languages within the family relative to the reconstructed protolanguage.
6. Tabulate the innovations established in (5) to arrive at an internal classification of the family, a ‘family tree’.
7. Construct an etymological dictionary, tracing borrowings, semantic change, and so forth, for the lexicon of the family (or of one language of the family).

Fellow comparativists would doubtless come up with other breakdowns. Some might want to include the reconstruction of syntax. The steps given by Nichols, Grace, and Koch in their contributions to this volume are rather different from these, but the difference is one of focus, not of substance. Nichols, for example, is concerned with how linguistic relatedness is demonstrated and proven, whereas our breakdown attempts to codify those steps in applying the method which are assumed or referred to in the contributions to this volume (not that the contributors would necessarily agree with our codification!). These steps, listed above as sequential, are in practice often recursive. For example, as the steps beyond step 1 are executed, more diagnostic evidence for the family may well be found. As the protolanguage is reconstructed and the internal classification of the family is worked out, languages previously thought to be unrelated may also be recognised as members of the family.

We will discuss each of these six steps, looking at potential problems and matters of controversy associated with each, and especially relating them to the contributions in this volume.

## 2.1 Diagnostic Evidence

Step 1 may appear rather surprising, as it is often assumed that the whole application of the comparative method has something to do with demonstrating relatedness. However, Nichols (this volume) examines the conventional theory and practice of Indo-Europeanists—and especially of Antoine Meillet, whose work epitomises Indo-European comparative linguistics before 1960—and shows that their determination of relatedness is logically quite separate from the succeeding steps of the method. It is based on evidence of the kind that Nichols terms ‘individual-identifying’, that is,

'evidence that firmly identifies a unique individual protolanguage' because 'its probability of multiple independent occurrence among the world's languages is so low that for practical purposes it can be regarded as unique and individual'. In the Indo-European tradition the main (although not the only) source of such evidence is whole paradigms of morphological forms, preferably with multiple paradigmaticity, as in the case of the inflections which mark the intersecting categories of the Indo-European nominal system with its three genders, its phonologically defined declension classes, and its case-marking paradigm.

Nichols shows that the nature of evidence used to diagnose relatedness was clearly recognised by Meillet, both explicitly (1958: 91) and implicitly (in his practice). However, as well as naming and defining this kind of evidence, she operationalises the concept of individual-identifying evidence statistically. She defines a statistical threshold for 'individual-identifying' by combining a single language's probability of occurrence among the several thousand on earth with a conventional level of statistical significance, then calculates the probability of occurrence of various samples of individual-identifying evidence in Indo-European, including the nominal system. These samples include the various kinds of evidence which Meillet treated as individual-identifying: morphological paradigms, lexical sets (e.g. the lexemes of the counting system), and individual words with complex or lengthy forms. But, as Nichols notes, to say that something is individual-identifying is only to say that all the languages which have it have acquired it from a single source. In the case of morphological paradigms, comparativists assume that acquisition is by inheritance, discounting borrowing as improbable. In the case of individual words, however, borrowing is quite possible, and this is why Nichols argues that the evidence of individual lexical items is at best supportive of diagnosed relatedness, rather than itself diagnostic. Comrie (1988) provides an example where both lexical borrowing and syntactic restructuring have occurred, so that only morphological paradigms remain as evidence of genetic relatedness.

We referred earlier to the technique of 'multilateral comparison' applied by Greenberg (1987) to divide the languages of the Americas into families and to determine the internal structure of the largest of these, Amerind. The proponents of multilateral comparison claim to be working within the framework of the comparative method (see, for example, Greenberg & Ruhlen 1992), and publications by nonlinguists sometimes fail to distinguish between language families determined by the classical comparative method and those based on multilateral comparison or other nonclassical approaches (see Nichols, this volume, for references). By showing that the diagnosis of relatedness is logically separate from the other steps of the comparative method, however, Nichols also shows that multilateral comparison and other techniques (like lexicostatistics) which omit our step 1 are in a fundamental way not applications of the comparative method. Instead multilateral comparison takes as its evidence simultaneous sound/meaning similarities across a number of languages. And, Nichols shows, these fall short of the statistical threshold for 'individual-identifying' and are therefore not evidence of relatedness at all (for another statistical argument against multilateral comparison, see Ringe 1992).<sup>6</sup>

The attraction of multilateral comparison and other techniques of 'long-range comparison' is that they claim to reach much further back into time than the comparative

method has been able to do. However, if they do not provide evidence of relatedness which could not have arisen by chance, the 'families' they establish must be treated with skepticism.<sup>7</sup> It can of course be argued that at great time-depths the morphological paradigms which form the backbone of diagnostic evidence under the comparative method must have decayed or shifted function to such a degree that their reflexes will be nonexistent or unrecognisable, and that to demand them as a prerequisite of the comparative method is ridiculous. But whether the presuppositions of decay and functional shift are correct or not (and the degree of survival of morphological paradigms in the Indo-European and Austronesian families suggests they are not), any technique which purports to delineate language families must produce individual-identifying evidence of that family's protolanguage. One way of doing this might be to treat a potential 'macrofamily' in the same way as one would have to treat a family of isolating languages (which would have no morphological paradigms), namely to seek individual-identifying evidence in the form of lexemes organised into paradigmatic sets. But in such cases, as Nichols emphasises for isolating languages, each set would have to be reflected in its (near) entirety in all language families claimed as members of the macrofamily. Such reflexes would also presuppose regular sound correspondences.

Quite distinct from multilateral comparison are the long-range comparison techniques used to reconstruct etyma in Proto-Nostratic (the putative protolanguage superordinate to Proto-Indo-European, Proto-Uralic, and various other language families) and the protolanguages of certain other putative macrofamilies (e.g. Sino-Caucasian; Starostin 1982, 1984). Multilateral comparison compares morphemes with similar sounds and similar meanings, but makes no attempt to generate sound correspondences. It thereby bears only the most superficial resemblance to the comparative method and cannot meet any of the criteria mentioned in the previous paragraph. On the other hand Nostratic long-range comparison seeks to follow the comparative method from step 2 onward and presents rudimentary morphological paradigms and cognate sets which may take it towards the eventual fulfilment of step 1 and meeting the individual-identifying criterion.<sup>8</sup> It is therefore regrettable that some writers do not distinguish clearly between the two approaches (for example, Shevoroshkin & Manaster Ramer 1991).

If the comparative method simply does not provide results which stretch back beyond, say, Proto-Indo-European or Proto-Austronesian, then it may well be that linguists will have to resort to quite different methods in order to research earlier periods, as Nichols (1992) suggests, but the results will not be in terms of genetic relationships. It is worth asking, however, *why* the time-depths accessible to the comparative method are limited. The received answer is that beyond a certain time-depth lexical replacement erases cognate sets to such a degree that there is insufficient material to work out sound correspondences, let alone to recognise morphological paradigms. But if this were uniformly true, then we would expect in a large language family like Austronesian, within which a diachronic series of interstage protolanguages is reconstructable, to find progressive degradation of the evidence throughout the family as we reconstruct successively earlier interstages. But we do not find this: Instead we find great variation in the rate of lexical retention in different parts of the Austronesian family (Blust 1981), correlating with factors which have to do with language contact (Pawley & Ross 1993).

The data require a more sophisticated hypothesis about the time-depth limitations of the comparative method. Bellwood (1984,1991) and Renfrew (1987) propose that the world's major language families achieved their status because of the demographic muscle which their speakers gained through the acquisition of agriculture. As these speakers expanded, slowly but inexorably, in search of new meadows or paddy-fields, they outnumbered and often absorbed hunter-gatherer populations, eliminating their societies and their languages. It is implicit in this model that speakers of early Indo-European, early Afro-Asiatic, early Austronesian, and their peers eliminated their own neighbour-relatives, quite possibly without trace. If this is so, the fact that practitioners of the classical comparative method cannot find the immediate relatives of Proto-Indo-European, Proto-Afro-Asiatic, and Proto-Austronesian is not surprising: It is because these relatives have disappeared without trace. There are, however, areas of the world where the agricultural expansion model does not apply. These include Australia (without agriculture) and New Guinea (with agriculture but little expansion). In New Guinea there appears to have been continuous and relatively undisturbed human habitation from much greater time-depths than in the cases of the world's major language families. The comparative method has yet to be systematically applied across the entirety of such an area,<sup>9</sup> but it would not be surprising to find that it can penetrate to a time-depth considerably greater than received wisdom allows. The difficulties we would expect the comparative method to encounter here are not so much those of simple degradation as those caused by ongoing language contact, discussed later in section 3.6.<sup>10</sup>

## 2.2 Cognate Sets and Sound Correspondences

In practice, step 2—collecting putative cognate sets for the family—overlaps with step 3 because the researcher inevitably develops hypotheses about sound correspondences in the course of searching for cognates and uses these hypotheses to further that search. Indeed, comparativists have tended to be rather cavalier about the semantics of potential cognates, assuming 'that any vocabulary set displaying the regular sound correspondences is in fact cognate, however far-fetched the semantic correspondences' (Nichols, this volume). The presupposition which underlies this assumption is that semantic change is not characterised by any crosslinguistic commonalities and is therefore unlike phonological change, where the comparativist recognises that some kinds of change are far more likely to occur than others. The contributions to this volume suggest, however, that this presupposition is false, and that there are at least two aspects of semantic change which can be codified and used by the linguist in judgments of cognation. The first of these aspects has to do with natural semantic shifts, the second with contact-induced change.

Wilkins, in his contribution to this volume, argues that the meanings of lexical items are subject crosslinguistically to natural shifts about which we can make generalisations parallel to those which comparativists make about phonological and morphological changes. These natural semantic shifts can be used, as Wilkins demonstrates from Australian data, to justify the inclusion in a single cognate set of items with different meanings. Conversely, they may sometimes imply the implausibility

of such an inclusion. He thus demonstrates the role of a science of comparative etymology in the comparative method.

In Ross' contribution it is suggested that one aspect of certain kinds of contact-induced change resulting from bilingualism is that the semantic structures of one language may become the model for another. When this happens, the ranges of meaning of the lexical items of one language are expanded, contracted, or otherwise altered to bring them into line with their perceived equivalents in the model language.

Probably every experienced comparativist has learned not to take step 3—working out sound correspondences from cognate sets—for granted. Chapters 3 to 7 are all concerned with cases where sound change appears to have been 'irregular', resulting in cases where words clearly form cognate sets but produce irregular sound correspondences.

In his contribution Campbell argues that, although irregularities in sound change undoubtedly occur, we should not give up the basic concept of the regularity of sound change since many irregularities can be explained by linguistic and sociocultural factors which may 'interfere' with regularity but do not undermine the principle of regularity itself. Linguistic factors include morphological conditioning (discussed from a different perspective in Koch's contribution) and the avoidance of homophony. Sociocultural factors mentioned by Campbell are (i) onomatopoeia and affective/expressive sound symbolism, including institutionalised forms where the relationship between two or three sounds expresses a semantic relationship in a number of word pairs or triplets; (ii) entry into the phoneme system of new phonemes as a result of extensive lexical borrowing, followed by the replacement of inherited phonemes by the new phonemes in some inherited words; and (iii) language death, where imperfect learning of the language can result in considerable irregularity. Campbell also takes issue with those who have suggested that the regularity of sound change is somehow suspended in 'exotic' languages on the mistaken assumption that 'primitive' or unwritten languages behave differently from 'civilised' or written languages.

In his chapter Newman describes the traditional Chinese philological tradition, arguing that, where the forms it attributes to Middle Chinese differ from those that would be reconstructed by a comparativist from modern Chinese dialect data, the Middle Chinese forms need to be taken seriously as preserving information which has been lost in all modern dialects. With this in mind, he examines a number of irregularities in modern Cantonese forms. Some of these are true irregularities, in the sense that they are due to factors which have interfered with regular development: The factors he exemplifies are the influence of Chinese writing on a few forms and the avoidance of taboo forms. Others are only irregularities at first sight: If comparativists are careful about the way they order the sound changes they reconstruct, then some 'irregularities' become regular by the ordered application of natural changes.

Blust discusses two cases of lexically sporadic sound change in Austronesian languages which he calls 'pandemic', that is, they occur independently in a large number of different languages. He shows that one of these, the crossover of initial /k/ and initial /g/, is probably the result of a natural perceptual difficulty due to the acoustic similarity of the two segments. The other—the widespread but sporadic tendency for

a homorganic nasal to be inserted before a word-medial stop in a range of languages—has occupied a number of scholars, but Blust finds none of their explanations satisfactory, leaving this phenomenon to tantalise future students of Austronesian.

Durie presents a case of a rather different kind, taken from one of the longest-studied language families. He suggests that there are genuine sound changes which are not categorically regular, yet it is inappropriate to speak of ‘exceptions’ because the phonetic conditioning of the change is probabilistic in its effect. Durie thus seeks to blur the traditional distinction between regular and exceptional change. Grace presents us with a case where, when he put the ‘irregular’ cognate sets on one side, he discovered that he had nothing left: There were plenty of cognate sets, but none that he could label as ‘regular’. We will consider Durie’s and Grace’s contributions below, when we turn to the comparative method in its wider sense.

### 2.3 Reconstruction

Step 4 is the reconstruction of the protolanguage of the family. Describing approaches to the reconstruction of prehistoric languages, Anttila (1989: 341) suggests that there are two schools of thought among linguistic prehistorians. ‘Formulaists’<sup>11</sup> assume that the symbols used in reconstruction are simply cover-symbols for sound correspondences. ‘Realists’ assume that the symbols used in reconstruction not only represent the phonemes of the protolanguage but also tell us something about the articulation of these phonemes. Indeed, realists assume in general that they are reconstructing something approximating a ‘real’ language. The formulaist/realist debate receives little attention today, but the interest which comparativists take in checking the plausibility of reconstructed protolanguages against the findings of language typologists betrays an almost universal acceptance of some version of the realist position. A recent and thoughtful discussion of this issue is provided by Lass (1993). Our statement of step 4a—reconstruct the protophonology from the sound correspondences worked out in step 3, using conventional wisdom *regarding the directions of sound changes*—betrays a realist bias, since one cannot discuss the directions of sound changes unless one believes that one’s reconstructions consist of potentially ‘real’ phonemes affected by ‘real’ sound changes.

At step 4b the comparativist uses the newly reconstructed protophonology to reconstruct the morphemes of the protolanguage from the cognate sets of step 2. These morphemes include both lexical items and paradigms of grammatical morphemes. If steps 2, 3, and 4a have been successfully executed, then the reconstruction of lexical items usually follows almost automatically from them. The reconstruction of morphology, however, is a different matter. In his contribution to this volume, Koch distinguishes between sound change in morphemes and morphological change. Where different diachronic stages in the development of a morpheme are shown to be related by regular sound changes, then Koch speaks only of ‘sound change’, reserving the term ‘morphological change’ for cases which, viewed phonologically or semantically, are ‘irregular’. However, what is phonologically irregular is not necessarily morphologically irregular. Just as the comparative phonologist uses a typology of sound change as a check on the plausibility of the phonological history he reconstructs, so the comparative morphologist may use a typology of morphological change to decide

among reconstructive hypotheses, that is, to determine which encapsulates greater diachronic regularity. Koch provides us with just such a typology.

Ross, in his contribution, touches on morphological changes induced by language contact. These are changes in grammatical function rather than changes in form, and they arise in the context of the more general morphosyntactic change ('metatypy') which occurs when one language is restructured by bilingual speakers on the model of another. Ross makes the point that when, in the course of step 4b and step 4c (the reconstruction of syntax), the comparativist finds sudden restructurings of morphosyntax, he should not put them aside as unexplained residues but, if his data allow, investigate the possibility that metatypy has occurred. The other side of this coin is that syntactic change can occur with great ease, is far from meeting the individual-identifying criterion, and should never be used to establish genetic relationships (controversy over the putative Altaic and Trans New Guinea genetic groupings illustrates this; see respectively Unger 1990 and Foley 1986).

Because the focus of this volume is on regularity and irregularity in linguistic change, the contributors have mostly focussed their attention on steps 2, 3, and 4 of the comparative method, and so discussion of steps 5 and 6 lies outside the scope of this introduction.

### **3 THE COMPARATIVE METHOD (WIDE SENSE): A PARADIGM SHIFT IN PROGRESS?**

We remarked at the beginning of this introduction that the term 'comparative method' is often used in linguistics to refer not only to the method itself but also in a 'wide' sense to its associated theory. For most of the method's lifetime, the central pillar of the theory has been the Neogrammarian hypothesis of the regularity of sound change. In recent years, however, the hypothesis has been subsumed by a growing body of sociohistorically based theory. The regularity hypothesis has been questioned ever since it was proposed, but historical linguists have continued to work with it because regularity *is* a major feature of language change and because there has been no systematic substitute for the hypothesis. Comparativists have constantly encountered data which seemed to challenge the hypothesis, and eight of the contributions to this volume (chapters 3 to 10) deal with such data.

The causes of irregularity advanced by the contributors are many and complementary. Apart perhaps from the effects of analogy and morphological conditioning (Koch), none of the causes has its locus in the language system itself. Instead, most refer in one way or another to the *speaker* of the language. This reference may be to the speech organs and speech processing (Blust), to cognition (Campbell on onomatopoeia and affective/expressive sound symbolism; Wilkins on universal semantic change), to communicative exigency (Campbell on avoidance of homophony), to politeness (Newman on taboo), or to bilingualism and to contact between speakers of different languages (Campbell on borrowed phonemes; Ross; Grace). Comparativists find themselves moving from the quasi-Darwinian Neogrammarian paradigm which underlies the regularity hypothesis to a speaker-oriented paradigm which has a place for both regularity and irregularity.

The basic assertion of the Neogrammarian hypothesis is that a sound change occurs without exceptions, apart from the outworkings of analogy and borrowing. Whilst the regularity hypothesis certainly reflected its authors' desire for rigorous scholarship, its conceptual roots go deeper. The Neogrammarians adopted it from the work of August Schleicher, who—in the heyday of Darwinism—argued that languages are natural organisms following natural (and therefore exceptionless) laws, and that linguistics is therefore a natural science (1863: 6). Hermann Paul, whose *Prinzipien der Sprachgeschichte* is a Neogrammarian manual of the comparative method and of historical linguistics, regarded language as a *psychischer Organismus* ('psychic organism'), subject to a selection process analogous to Darwin's natural selection. The functionality of a linguistic pattern, defined by how well it was suited to the organs of speech, determined its extinction or survival (Paul [1880] 1920: 25, 32, 56).<sup>12</sup>

The view of language as an organism had its continuation (at least metaphorically) in the Saussurean concept of language as a system and in the idea that paradigms have their own internal dynamic (Lass 1980: 120). This 'Neogrammarian heritage' is recognised by Weinreich et al. (1968: 119), and our use of the term 'Neogrammarian' from this point also subsumes Structuralist historical linguistics (as described, for example, by Bynon 1977: chap. 2).

The metaphorical power of this idea has remained strong, and historical linguists have often been loath to look outside language systems to explain changes within those systems.<sup>13</sup> As numerous commentators have pointed out, there has long been an unease with the organism metaphor. There has also been a recognition that language is spoken by speakers and language change is subject to socio-historical factors. Paul and Meillet both knew this (Weinreich et al. 1968: 104–8, 176–77), but historical linguistics waited until the publication of Weinreich, Labov, and Herzog's 'Empirical foundations for a theory of language change' in 1968 for the first outline of a speaker-oriented, socially embedded paradigm.

Why did the shift to a speaker-oriented approach take so long? In retrospect we see that the shift was the outcome of changes over which linguists had no control, especially the reorientation of anthropological and sociological theory away from social Darwinism, a reorientation associated with the names of Franz Boas and Emile Durkheim. A practical result of the new paradigm in cultural anthropology was a growth in fieldwork in unwritten languages and consequently in knowledge and experience of spoken languages in a range of community contexts. The new sociology provided a framework for interrelating language, speakers, and the community; concomitant developments in social statistics allowed the analysis of linguistic data collected in that framework. But it is increasing access to computing technology which has made it possible for historical linguists to start processing the immense amounts of data which must be analysed if a speaker-oriented historical linguistics is to have an empirical foundation, as we see from the publications of Labov and his associates, Le Page & Tabouret-Keller, Lesley and James Milroy, and Durie (this volume).

Keller's (1990) study of the conceptualisation of language change depicts historical linguists as trapped between two unviable alternatives. On the one hand, a language is clearly not an organism. On the other hand, it is not an artefact like art, sci-

ence, or philosophy, since these have a history directly attributable to human intention (Keller 1990: 73). Instead, Keller argues, language change is usually an ‘invisible hand process’ (the term comes from the eighteenth-century philosopher-economist Adam Smith), the kind of process which takes place when members of a population act in a similar way with similar intentions, yet their collective acts have a result beyond their intentions. A simple invisible hand process occurs when many people take the same short cut over a grassed area, with the unintended result that they trample a track across it. A language change takes place because, for example, a sufficient number of speakers perform the same ‘act of identity’ (Le Page & Tabouret-Keller 1985), that is, adopt a feature from the speech of some other group of speakers in order to identify more closely with that group, with the unintended result that the sum of their adoptions becomes a change in the language system.<sup>14</sup>

When people perform similar actions with similar intentions in what turns out to be an invisible hand process, their behaviour can be influenced by an awareness of others’ behaviour in the circumstances: A sees B taking a short cut and follows him, or A sees the beginnings of the trampled track, recognises its use, and follows it. Similarly in a language change, one speaker hears an innovatory feature in the speech of another and adopts it. As Milroy and Milroy (1985: 345–48) argue, it is necessary to distinguish between speaker ‘innovation’ and language ‘change’. A speaker innovation may or may not be copied by other speakers. If it is copied, it may be adopted only by people in the speaker’s immediate community, or it may be copied into other communities via other innovators. The linguist who observes the result of this invisible hand process labels it ‘language change’.

### 3.1 Explaining Language Change

This brings us to what Weinreich et al. (1968: 102) call the ‘actuation problem’: ‘Why do changes in a structural feature take place in a particular language at a given time, but not in other languages with the same feature, or in the same language at other times?’ As one can infer from the foregoing discussion, the explanation of an invisible hand process is always complex.<sup>15</sup> If the *explanandum* is the change in the language system, then the immediate efficient cause of that change is the cumulative adoption of the relevant innovation by a large number of speakers. But the adoption itself is explicable in terms of a final cause, namely the speakers’ motive(s) for adopting the innovation. Also to be explained are the identity of the innovator and the eligibility of the innovator(s) to be copied by whom (what are these speakers’ positions in the speech community? why them and not others?) and how the innovation comes to be in the innovator’s speech in the first place (what classes of innovation can occur? how do they get there?).

When we question these elements in relation to a particular language change, then we are asking historical questions, and these require historical answers. A historical answer applies generalisations to events after they have happened in order to help explain them: it does not predict specific future events.<sup>16</sup> For ease of presentation, we will discuss these elements and the kinds of generalisations relevant to them in the following order:

1. the identity of the innovator and the copiers and their relationships within the speech community;
2. the entry of innovations into the innovator's speech and human constraints on possible or preferred innovations;
3. speakers' motives for adopting innovations.

When explanations of particular language changes are given within the Neogrammarian framework, they are largely given in terms of a 'speakerless' version of (2) alone, that is, in terms of the language system to the exclusion of human (physiological and cognitive) constraints. The Neogrammarian framework has no explicit theoretical place for the speaker, as required by our formulation, nor for the speech community as required by (1). On the other hand, when explanations are given within the speaker-oriented paradigm, they often consist only of answers to (1) and (3), to the neglect of (2). That is, emphasis falls on sociohistorical factors, to the neglect of physiology and cognition. However, as Wilkins (this volume) argues with regard to semantic change, both human (in the case of semantics, cognitive) and sociocultural factors play a role in explaining language changes.

### 3.2 Speech Communities and Innovators

Our earlier formulation of (1) presupposes a definition of the speech community, but a rigorous definition is difficult to formulate. Grace (this volume) writes of the definition of 'speech community': 'In its modified form it might read, "A speech community consists of those people who communicate with one another or are connected to one another by chains of speakers who communicate with one another." But to be entirely satisfactory it would also need to recognize that community is a matter of degree. . . .'

As Grace says, the speech community has commonly been defined by its speakers' language, but this definition fails for the case he describes because the speech community uses a *pool* of languages and there is no one language coterminous with the community. The last sentence of the quotation refers to the fact that a speech community can be viewed schematically as a series of roughly concentric circles (e.g. hamlet, village, village group, region, and so on) to which there is no specified upper limit—a view which will work just as well in a traditional village situation as in a metropolitan urban setting.

A current speaker-oriented view of the speech community which meshes with the previous paragraph (and on which Grace draws) is one which has been developed through research in Belfast and views the speech community in terms of a social network of links between speakers (Milroy 1992, Milroy 1993, Milroy & Milroy 1985, Milroy 1980, Milroy 1987, Milroy & Milroy 1992). A network is characterised by the density of its links (the number of people who relate to each other) and by the intensity (in terms of time spent and intimacy) and the multiplexity of those links (the number of purposes for which two people relate to each other) (Milroy 1980: 20, 49–52, 139–44). A virtue of this model, unlike the social-class model of urban sociolinguistics, is that it can be applied to speech communities everywhere (see, for example, Schooling 1990).

Milroy and Milroy (1985) suggest that innovators are people who are marginal to the local community which will adopt the innovation, but who have a large number of weak and uniplex ties outside it to people who in general do not know each other. Their outside ties make them candidates for adopting innovations from people outside the community; their marginal position means that they are less susceptible to norm-enforcement within the local community and that innovations have less significance for them as social markers. If a speaker innovation diffuses into the community, it does so because it is copied from the innovators by people whom Milroy and Milroy call 'early adopters'. These are people who are central to the network: They have strong ties with a relatively large number of people who also have ties to each other, they conform to community norms, and they provide a model for conservative members of the community. Once an innovation is adopted by them, the middle of the S-curve of innovation diffusion has been reached, and the innovation diffuses with increasing speed into the rest of the community (Milroy 1992: 172–85).<sup>17</sup> This account presupposes, of course, that innovations are always introduced to the community from without, but this is not necessarily the case.

Neogrammarian explanations of (2) look at the entry of the innovation into the language system as a whole, rather than into the *innovator's* speech. They appeal to either borrowing, system-internal change, or analogy (which is also a form of system-internal change). But the speaker-oriented model of language change renders the well-worn Neogrammarian distinction between borrowing and system-internal change less clear-cut, as every change begins (from a community perspective) in the speech of innovators, diffusing into the community via early adopters. As Wang (1979: 362) remarks, it doesn't matter how a sound change is actuated ('internally' or 'externally')—its method of implementation (spread) will be the same. This is vividly clear in the situation described by Grace (this volume).

### 3.3 The Innovator's Language System and Constraints on Innovation

The question of how an innovation comes to be in an innovator's speech in the first place presupposes a theory of the individual's speech in relation to the language system of the community. It is by now a truism of sociolinguistics that the language system of a community entails orderly variation. But it is also true that each speaker's speech is characterised by orderly variation, even though individual speakers may not have access to all the variants of the system (Weinreich et al. 1968: 100–1, 159; Milroy & Milroy 1985: 346–47). Apart from the introduction of new lexical items, which may be *additions* to an open system, innovations arise as *variants* in the innovating speaker's language system. These variants may be lexical, phonological, or morphological, but our discussion here will be largely limited to phonological innovations, as scholars operating with a speaker-oriented model have paid more attention to these. Whether an innovator acquires a new variant from outside the community or whether it arises spontaneously within his speech makes little difference to its diffusion within the community or to the constraints to which it is subject within the innovator's linguistic system.

Weinreich et al. (1968: 183–84) labelled the question of what language changes are likely and why they are likely 'the constraints problem'. This is an area of histor-

ical linguistics which has received little attention since the Neogrammarian heyday, and so it is gratifying that this volume contains several contributions which refer to it. Blust touches on articulatory and acoustic constraints, Durie on systemic constraints, Wilkins on constraints on semantic change, and Koch on constraints on morphological change.

Experienced comparativists know that certain sounds (e.g., nasals; cf. Baldi 1990a 1990: 6–8) are less prone to change than other sounds and that certain sound changes are far more likely to occur than others, and they use this knowledge in doing reconstructions. What the comparativist knows, informally, is what sound changes are ‘natural’, that is, normal within the constraints of human physiology and cognition (or sometimes just common within the language family the linguist works with). It was evidently this insight which inspired attempts during the Neogrammarian era to catalogue and classify sound changes which, it was assumed, were universal,<sup>18</sup> but these endeavours declined with the growth of Structuralism, and there has been little interest in the phonological aspect of the constraints problem in recent literature, with the exception of Ferguson (1978, 1990) and the work of Stampe, Donegan, and Ohala referred to later.

Structuralist historical phonology has emphasised the relationships between changes within a language’s phonological system, either because one change seems to trigger the next (as in the chains of changes recognised by Martinet [1952, 1955]). Or because several changes entail an alteration of the same componential feature. Foley (1977) also shows that a feature change affects a set of phonemes in a predictable diachronic sequence. But further progress with the problem of phonological constraints is only possible if the innovating speaker is allowed to emerge from the wings, complete with organs of speech and hearing and with a mind which perceives, processes, and produces. Mainstream phonology has tended to take the physiology of speech for granted (or, like Foley, to declare it irrelevant) and to take account of naturalness only by incorporating markedness conventions into phonological representations. But markedness conventions only capture the outcomes of naturalness; they cannot characterise naturalness itself. What the comparativist needs is a characterisation of naturalness in terms of the physiology and cognition of speech and an account of the relationship between naturalness and sound change. For this we have to look beyond mainstream phonology to research directions which offer speaker-oriented insights into the constraints problem. Two such directions are Stampe’s ‘natural phonology’ (see, for example, Donegan 1993, Donegan & Stampe 1979, Stampe 1979) and Ohala’s laboratory phonology (see, for example, Ohala 1993, 1974, 1989), both of which take the interactions between the organs of speech, hearing, and cognition as basic elements of theory.<sup>19</sup>

We are aware that objections of various kinds have been raised against natural phonology (for a sympathetic statement of these, see Anderson 1985: 342–47), but for the comparativist it provides a sensible research framework in a way that its competitors cannot. Firstly, it reserves a special place for naturalness, rather than treating it as merely a formal convention. Fundamental to natural phonology is a distinction between phonological processes, which have their basis in natural constraints imposed by the organs of speech and hearing, and morpho-phonological rules, which are purely conventional.<sup>20</sup> Secondly, two kinds of phonological processes are recog-

nised, fortitions, and lenitions, which, as we will note, are relevant in the description of certain sound changes.

Fortitions (or paradigmatic processes) are processes which work to limit the inventory of ‘possible’ (intentionally pronounceable/perceivable) segments in a language. For example, the vowel [ɒ] requires both opening the mouth wide (the feature ‘low’) and narrowing the lips by rounding (the feature ‘round’). These gestures are obviously in conflict, and there are several fortitive processes which can be used to remedy this difficulty: after a period of variation, one of the features is abandoned, giving [a] or [o], or they are sequenced in a diphthong, giving [aɔ̃] or [oã]. Fortitive processes are not learned, but follow naturally from the physical features of the organs of speech. Instead, what *is* learned is the ability to overcome the difficulty of producing and perceiving, for example, the vowel [ɒ]. The fortitive process applies when this ability is not acquired or is lost. Fortitive processes ‘are part of the speaker’s native *incapacity*’ (Donegan 1993: 109). Fortitive processes allow us to account for the fact that there are ‘default’ phoneme inventories which are particularly well distributed among the world’s languages (e.g., the system of five cardinal vowels).

Lenitions (or syntagmatic processes) in natural phonology are processes which overcome difficulties associated with producing and perceiving segment sequences. Lenitive processes are, for example, vowel nasalisation before nasals or stop fricativisation between vowels. Hearers unconsciously ‘undo’ lenitive (allophonic) processes in order to recognise a phoneme sequence.

The means by which natural processes become speaker innovations has been described by Ohala (1993, 1989).<sup>21</sup> Phonological processes result in innovations when hearers misinterpret what they hear. Modern instrumental phonetic analysis shows that rule-governed phonetic variation, resulting from the competing demands of fortition and lenition, is a ubiquitous trait of speech. Hearers constantly make perceptual corrections for this variation. Sporadically, however, they ‘hypocorrect’, that is, they fail to correct what they hear. One kind of hypocorrection occurs when speakers fail to detect the difference between two acoustically similar sounds and treat one as the other.

Hypocorrection may apply to both fortitive and lenitive processes and may lead to the creation of new phonemes. If a hearer fails to recognise [a] or [oã] as an allophone of /b/, a natural fortitive process is allowed to operate, and some or all instances of /b/ are reinterpreted (variably in the first instance) as /a/ or /oa/.<sup>22</sup> If a hearer fails to interpret nasalisation on a vowel as a feature of the adjacent nasal consonant (i.e., fails to ‘undo’ a lenition) and instead interprets it as a feature of the vowel, then the hearer’s phonological system may acquire a new set of nasal vowel phonemes (especially if nasal consonants in this context are underarticulated). It is worth recalling in all this, as Donegan, Ohala, and Blust (this volume) all do, that although phonetic events occur in the organs of speech and hearing, the acts of perception, interpretation, and instructions to produce sounds are essentially mental.

Sometimes hypocorrection leads to the confusion or merger of two phonemes. Blust (this volume) points out that the acoustic difference between voiced and voiceless velar stops is smaller than at other points of articulation, with the result that the hearer fails to recognise a lightly voiced [g] as /g/ and instead interprets it as /k/. This results in the merger of Proto-Austronesian \*k and \*g in many daughter languages

(allowing a fortitive process to operate), or, in languages which retain the distinction, in their confusion word-initially in a number of etyma.

We are now in a position to understand the actuation of the chains of changes described by Martinet (1952, 1955) and Foley (1977). We have no means of predicting when the first shift in a chain will arise in the orderly variation of an innovator's speech, but we know that once it has occurred, the probability of the next shift occurring is much greater than if the first shift had not occurred. Once the 'hardest-to-articulate' sound in a sub-system has undergone fortition or lenition, the next hardest is likely to follow, for reasons which evidently have to do with economy in the interaction of the speech organs and cognition. It is sometimes argued that chains of changes are provoked by the need to fill a 'gap' in a subsystem. In the speaker-oriented paradigm, however, a gap is nothing exceptional: complete effacement is simply the most extreme form of lenition.

The kind of chain which most readily springs to mind is a vowel fortition chain like the Great Vowel Shift in English. But another kind of chain is exemplified by the intervocalic lenition of stops, where it seems that the probability of the lenition of /t/ or /d/ to [r] or [ð] is close to zero if the corresponding velar /k/ or /g/ has not already been lenited (usually to [ɣ]), but quite high if it has. Of course, this does not tell us how quickly or slowly the next shift in the chain will occur. Eckert's (1980) careful study of a chain of vowel shifts in southwestern France shows that a chain shift can continue over generations, each sound change eventually triggering the next.

Our intent in the preceding paragraphs has been to indicate the shape of theories which may offer answers to the phonological constraints problem, and we have touched only in the briefest way on research in both natural and laboratory phonology.<sup>23</sup> The claims implicit in these paragraphs are (i) that there is a growing body of theory, albeit away from the mainstream, from which we can predict which sound changes (and which chains of changes; see Labov 1981: 299) will be more common; and (ii) that innovations do not have to be 'borrowed' to enter the innovator's language system.

### 3.4 Motives for Innovation

The third element in the explanation of language change consists of speakers' motives for adopting an innovation. When the innovation is the adoption of a new lexical item, a fairly straightforward sociohistorical-cum-linguistic explanation seemingly suffices. For example, as Keller (1990: 109–27) shows in eloquent detail, the word *englisch* 'angelic' was replaced in mid-nineteenth-century German by *engelhaft* not simply because it was homophonous with *englisch* 'English' (speakers do not invariably avoid homophony; cf. Malkiel 1979) but because at this time *englisch* 'English' and *englisch* 'angelic' both increased in usage, one because of the rise of English/German industrial competition, the other because it had become an aspect of the female ideal and had a more readily available morphological alternative. Such an elegant answer presupposes generalisations (as Keller says). One is that speakers seek to avoid homophony when homophonous words rise in frequency of usage (see Campbell, this volume). Another is that a regular way of avoiding homophony is the adoption of a morphological alternative. Whether these particular generalisations

about speaker motivations are valid is a matter for investigation. However, we turn now to some powerful generalisations in recent literature about the circumstances in which speakers will or won't adopt innovations and the kinds of innovations they will accept.

It occurs to us that there need not be particularly strong or specific motivation for an innovation. The network model of the speech community offers a direct correlation between network structure and the probability of language change. The stronger the network, measured in terms of the density, intensity, and multiplexity of its links, the more effective the enforcement of norms and, it follows, the lower the probability that innovations will diffuse enough to become changes. And the weaker the network, the weaker the norm-enforcement and the higher the probability of language change (Milroy & Milroy 1985). We would expect that far less motivation is needed for a speaker innovation to become a language change in a weak network than in a strong one.

One of the standard answers to the question of what motivates speakers to adopt an innovation is 'prestige', but this is often too strong and too specific. Le Page and Tabouret-Keller's (1985) characterisation of many innovations as 'acts of identity' is more appropriate. For example, Andersen (1988: 41–3) describes the spread of a northeast Polish form for 'hog', an important commodity, along the trade routes of the Vistula river system in the Middle Ages. He argues that prestige is too strong a motive to appeal to in this case and suggests that the fact that the northeastern form was also pronounceable for speakers further south (whereas the southern form was not pronounceable for northerners), combined with 'a community solidarity among speakers, a bonding strong enough to find expression in a leveling of speech differences', is sufficient to account for the spread of the form. On Andersen's account, the mediaeval Vistula river area constituted a large, relatively weak network through which innovations could spread without strong motivation.

However, the preceding paragraphs continue to imply that an adopted innovation is normally borrowed into a lect (i.e. dialect or language) by its innovating speakers from another lect. A major point of Andersen's 1988 article is that there are phonological innovations which are not *borrowed* at all: they arise (as fortitions or lenitions) in the speech of innovating speakers of the lect and are adopted by other speakers precisely because they simplify or complicate the lect.

We owe to Jakobson the observation that there is a correlation between the *kinds* of innovation which a speech community will adopt and the *attitudes* its members have towards their language: 'Not enough attention has been given in linguistics to the essential difference in structure and evolution which exists between dialects which are moving towards the role of a koiné or common language and those of purely local use' (Jakobson [1929] 1962: 82). This correlation is discussed from various perspectives by Thurston (1987, 1989), Andersen (1988), and Ross (this volume). We will adopt Thurston's (1989: 556–7) terminology here in order to avoid proliferating terms. He distinguishes between 'exoteric' and 'esoteric' lects. An exoteric lect is the lect of a community with plentiful external ties which is also spoken by people from groups outside the community; it is accordingly valued for its use across group boundaries. An esoteric lect, on the other hand, is the lect of a strong community which is valued by its speakers as an emblem of group identity and is not shared with

outsiders: in Milroy's words, 'the maintenance of *social identity* may in some circumstances be important enough to devalue the message-oriented function: to put it crudely, some groups in society may not particularly want to be understood by other groups' (1993: 219).<sup>24</sup> The term 'esoteric' is applied by both Thurston and Ross to the languages of small, geographically separated communities in traditional societies, but it can equally well be used of the nonstandard vernacular dialects of urban communities which persist (and continue to diverge) as a result of social conflict (Milroy & Milroy 1992), for instance between blacks and whites in Philadelphia (Labov & Harris 1986).

Because an exoteric lect is subject to greater variability through its use by a wider range of speakers, innovations leading to reduction in phonological or morphological irregularity or complexity are liable to be preferred, and those favouring their increase dispreferred. Andersen cites Jakobson's account of Ukrainian vowel systems, which have become simpler in dialects 'moving towards the role of a *koiné*' but remain more complex elsewhere.

Speakers of esoteric lects adopt innovations which make their lects more irregular, more complex, and less easy for outsiders to understand or learn. Andersen's main example is the development of 'parasitic' consonants out of the offglide occlusion of long high vowels. For example, in the German dialect of Waldeck in Hesse, we find *iks* 'ice' from earlier \**i:s* (Standard German *Eis*), *fukst* 'fist' from \**fu:st* (Standard German *Faust*), *likp* 'body' from \**li:p* (Standard German *Leib*), and so on Andersen (1988: 65). What is significant is that this seemingly odd sound change has been recorded in a number of lects, including dialects of Rhaeto-Romance, Upper Rhone dialects of Provençal, Low and Middle Franconian dialects of Dutch/German, the Rømø and Samsø dialects of Danish, and Maru, closely related to Burmese. All of these are genetically quite closely related to a 'standard' language, but for geographical and/or political reasons are likely candidates for esotericity: the Rhaeto-Romance and Provençal dialects are in remote valleys high in the Swiss Alps, the Danish dialects on small islands, and Maru in the mountainous Kachin region of northern Burma (Burling 1966), whilst the Rhaeto-Romance, Provençal, and Franconian dialects are all in regions which were politically fragmented in the European Middle Ages.

It is instructive to review these innovations in the light of the approach to the constraints problem outlined above. What distinguishes exoteric from esoteric innovations is not the opposition between fortitions and lenitions but the amount of work which hearers have to undo in order to recognise a phoneme sequence. The sound changes which reduced the vowel systems of exoteric Ukrainian lects were fortitions entailing feature loss (so that two vowels merged), whereas the changes which result in parasitic consonants are fortitions in which co-occurring features are redistributed onto sequential segments, complicating the form of the morpheme. Ross (this volume) discusses Maisin, an esoteric language of Papua New Guinea. Here, the complicating sound changes are *lenitions* (loss of word-final consonants with compensatory lengthening of the preceding vowel and neutralisation of word-final nasals to [ŋ]). What they have in common with the fortitive addition of parasitic consonants is that both need more work to 'undo' the phonological processes which have given rise to them, and both increase the allomorphy of the languages they occur in.

Andersen describes the sound changes typical of esoteric languages as 'exorbi-

tant', but it is worth noting that from the point of view of phonological theory they are perfectly normal: they appear unusual only because they do not occur in the exoteric languages which have been the grist of the comparative method.

### 3.5 The Regularity Hypothesis and Lexical Diffusion

Scholars working within a speaker-oriented paradigm usually say very little about the Neogrammarian regularity hypothesis. This is not surprising. Although the hypothesis fits neatly into the organism paradigm, such that all tokens of a sound gradually change under the effect of a natural law, it is less easy to see its application in a speaker-oriented paradigm, in which every language change is the result of an innovation's diffusion, speaker by speaker, across a speech community. However, a speaker-oriented version of the regularity hypothesis can be formulated as follows:

- i. that each speaker who adopts a sound change does so first as part of the orderly variation of that speaker's speech;
- ii. that this variation progressively shifts in favour of the 'new' sound;
- iii. that (i) and (ii) apply equally and immediately to all items in the lexicon, which contain the relevant sound in the relevant environments.

The regularity hypothesis has long since been called into question by scholars working in German and Romance dialect geography, who replaced the maxim 'sound laws admit no exceptions' with the catch cry 'every word has its own history'. But only much more recently has this position been translated into a theory which can be directly opposed to the regularity hypothesis, an opposition expressed most trenchantly in a number of articles by Wang and his associates in their work on Chinese dialect data (Chen & Wang 1975, Cheng & Wang 1977, Wang 1969, Wang 1977, Wang 1979, Wang & Cheng 1977, Wang & Lien 1993). In place of the regularity hypothesis they propose the hypothesis of 'lexical diffusion'.<sup>25</sup> In the form in which they present it, it is no more speaker-oriented than the regularity hypothesis. It holds that a sound change is phonetically abrupt, not gradual, but that it applies to lexical items containing that sound over a period of time, not simultaneously. The Chinese (and other) evidence for the lexical diffusion hypothesis consists of a number of cases where a sound change has applied to some lexical items but not others (Wang, 1977). The conclusion is that, at least in some of these cases, a lexically diffusing sound change has been halted before it completed its journey through the lexicon. Again, a speaker-oriented version of the hypothesis can be formulated. It says

- i. that each speaker who adopts a sound change does so first as part of the orderly variation of that speaker's speech;
- ii. that this variation progressively shifts in favour of the 'new' sound;
- iii. that (ii) applies initially only to certain items in the lexicon which contain the relevant sound in the relevant environments, then progressively to other items.

It is assumed that the change will be extended to new items in more or less the same sequence in the speech of all speech community members who adopt it.

The reader will note that this reformulation differs from the reformulated regularity hypothesis only in (iii), and that it thereby does not do complete justice to the original. The lexical diffusion hypothesis asserts that sound change is phonetically abrupt, whereas the speaker-oriented version holds that change affects the speech community speaker by speaker and via variation, as in (i) and (ii), and must in these senses be gradual. The main claim of the lexical diffusion hypothesis is that sound change is 'lexically gradual', but the speaker-oriented version also claims that, like all innovations, it is 'speaker-gradual'.

The two claims are supported nicely by Lincoln (1973), who shows that Proto-Oceanic *\*t* has become /l/ in certain words in a chain of closely related small language communities of the Bel family along the north coast of Papua New Guinea. This innovation not only proceeds word by word; the fact that the daughter languages of Proto-Bel today are spoken in a chain of communities also allows us to see that the innovation proceeded from one small community to the next within the larger Proto-Bel speaking community. At one end of the chain is the community in which the largest number of words have undergone the change. At the other end is a community in which the change has not occurred at all. In between are communities in which progressively fewer words are affected by the change. The significant point is that there is an implicational relationship among the languages of these communities: Where a word is affected by the change in one of the less affected languages, the same item is also affected by the change in all the more affected languages.<sup>26</sup> At the same time, we know that other sound changes in the Bel family seem to support the regularity hypothesis (Ross 1988: 167–71). They could of course all be completed cases of lexical diffusion, but it is curious that only *one* of many changes shows evidence of incomplete lexical diffusion.

Labov (1981) examines the competing claims of the two hypotheses in the light of various speaker-oriented studies of sound change in English. He concludes that *both* hypotheses are correct, but apply to different classes of sound change: 'We have located Neogrammarian regularity in low-level output rules, and lexical diffusion in the redistribution of an abstract word class into other abstract word classes' (1981: 304). We will return to Labov's use of 'abstract' later.

Labov's main example of lexical diffusion concerns the redistribution of words in some forms of English from the class containing 'lax' or 'short' /æ/ into the class containing 'tense' or 'long' /æ:/ (in more orthodox terms, a sound change from /æ/ to /æ:/). The environment which most favours redistribution in Philadelphia English is a following tautosyllabic /m/ or /n/ (284). Thus in Philadelphia English the /æ:/ (Philadelphia [e:ə]) class includes *man*, *can* ('tin can') and *slam*. When the following /m/ or /n/ is not tautosyllabic, redistribution usually does not occur: *manner*, *camera*, *flannel*, and *damage* all have Philadelphia /æ/. But there are exceptions to these generalisations. Morphemes whose /æ/ can be reduced to schwa, for example *can* and *am*, remain in the /æ/ class, and past tense forms of strong verbs whose present tense stems have a lax/short vowel, for example *ran*, *swam*, and *began*, have been resisting redistribution for a millennium (285–86). On the other hand, morphologically complex words which on phonetic grounds should have resisted redistribution but