Historical linguistics and language change

ROGER LASS

HISTORICAL LINGUISTICS AND LANGUAGE CHANGE

ROGER LASS

University of Cape Town

Language change happens in the spatio-temporal world. Historical linguistics is the craft linguists exercise upon its results, in order to tell coherent stories about it. Roger Lass here offers a critical survey of the foundations of the art of historical linguistics, and its interaction with its subject matter, language change, taking as his background some of the major philosophical issues which arise from these considerations, such as ontology, realism and conventionalism, and explanation. Along the way he poses such questions as: where does our data come from? How trustworthy is it? What is the empirical basis for the reconstructive techniques we standardly take as yielding facts; and how much does the historian create data rather than receive it? The paradoxical conclusion is that our historiographical methods are often better than the data they have to work with.

CAMBRIDGE STUDIES IN LINGUISTICS

General Editors: S. R. Anderson, J. Bresnan,
B. Comrie, W. Dressler, C. Ewen, R. Huddleston,
R. Lass, D. Lightfoot, J. Lyons, P. H. Matthews,
R. Posner, S. Romaine, N. V. Smith, N. Vincent

Historical linguistics and language change

In this series

To the three Js, without whom this
book would not have been written:

> Hos tibi uersiculos fidus transmisit amicus,
> Si de parte tua fidei stat fixa catena.
> Nunc precor, ut ualeas felix per saecula cuncta.

But the iniquity of oblivion blindly scattereth her poppy, and deals with the memory of men without distinction to merit of perpetuity. Who can but pity the founder of the Pyramids? *Herostratus* lives that burnt the Temple of *Diana,* he is almost lost that built it; Time hath spared the Epitaph of *Adrians* horse, but confounded that of himself. In vain we compute our felicities by the advantage of our good names, since bad have equall durations; and *Thersites* is like to live as long as *Agamemnon.* Who knows whether the best of men be known? or whether there be not more remarkable persons forgot, than any that stand remembred in the known account of time? Without the favour of the everlasting *Register* the first man had been as unknown as the last, and *Methuselahs* long life had been his only chronicle.

(Sir Thomas Browne, *Hydriotaphia, Urne Buriall,* V)

Since the Renaissance, western society has come into contact with different populations that were seen as corresponding to different stages of development; nineteenth-century biology and geology learned to discover and classify fossils and to recognize in landscape the memories of a past with which we coexist; finally, twentieth-century physics has discovered a fossil, residual black-body radiation, which tells us about the beginnings of the universe. Today we know that we live in a world where interlocked times and the fossils of many pasts coexist.

(Ilya Prigogine and Isabella Stengers, *Order out of chaos,* 208)

Systems in many respects resemble machines. A machine is a little system, created to perform, as well as to connect together in reality, those different movements and effects which the artist has occasion for. A system is an imaginary machine invented to connect together in the fancy those different movements and effects which are already in reality performed. The machines that are first invented to perform any particular movement are always the most complex, and succeeding artists generally discover that, with fewer wheels, with fewer principles . . . the same effects may be more easily produced. The first systems, in the same manner, are always the most complex, and a particular connecting chain, or principle, is generally thought necessary to unite every two seemingly disjointed appearances: but it often happens, that one great connecting principle is afterward found to be sufficient to bind together all the discordant phaenomena that occur in a whole species of things.

(Adam Smith, *The Principles which Lead and Direct Philosophical Enquiries; Illustrated by the history of astronomy* (1795))

Preface

I do feel ... that I now have a better understanding of what the key problems are than I did ten years ago. At times I even persuade myself that I can glimpse some of the answers, but this is a common delusion experienced by anyone who dwells too long on a single problem.

(Francis Crick, *The astonishing hypothesis* (1994))

As long as I can remember I've been besotted with the past: artistic, linguistic, biological. Especially the latter two. For a time it was a tossup whether the preadolescent dinosaur-freak-cum-pedant would become a palaeontologist or a philologist; after years of mistaken career choices, including a spell as a literary medievalist, I ended up a linguistic historian (so a palaeontologist of sorts). And over the years this fascination with the past itself was joined by an equal fascination with the often devious ways we come to know about it. (Even such a brief autobiography may breach decorum, but it explains some of the odder properties of this book.)

These loosely connected but thematically unified essays are a kind of retrospective on nearly three decades of both being a historical linguist and worrying about the epistemic pretensions of what historical linguists do. In them I revisit some landscapes I've been prowling for years (now I hope with a more mature and cultivated eye), as well as exploring some new (garden?) paths.

The final (I wouldn't say finished) product sits uneasily in a no-man's-land of its own creation. It was conceived as a fairly elementary textbook, but in the course of gestation, and with some editorial encouragement, apparently changed audience. It is now addressed primarily to colleagues: advanced students, grown-up linguists not historians by trade, but interested in how a specialist practitioner sees what he does; even to fellow historians, as a collection of idiosyncratic but not uninformed reflections on our common endeavour. The genre might be called 'adult textbook': there

are precedents in historical linguistics like Hoenigswald (1960), and to a large extent Anttila (1989), Hock (1991), and in other areas as well (e.g. Harris 1960, Lyons 1977). This one however is not intended to be 'exhaustive', but deals only with a selection of issues I find both important and interesting, and tractable. Hence the absence of detailed treatment of topics requiring advanced numeracy, like lexicostatistics or 'dynamic dialectology' (Ogura 1990), or specialist knowledge like creolistics or the prehistory of language.

I call this neither 'historical linguistics', nor 'language change', but both, to suggest that two partially distinct but intertwined subject-areas make up our discipline. Language change happens 'in the (spatio-temporal) world'; historical linguistics is the craft we exercise on its apparent results, in order to tell coherent stories about it. The dichotomy, however, is not actually that clear, and this gives the subject (and metasubject) much of its interest.

This then is largely a critical survey of the foundations of the art of linguistic story-telling, and how these interact with its ostensible subject matter. How do you tell good stories? What are (or ought to be) the criteria for goodness? How do you evaluate competing stories? What is the substance of our narratives and reconstructions, and where does it come from? Historical linguists are both historians and linguists, and their tradecraft combines those of both disciplines. As historians they are bound by the standard constraints on all historians (cosmologists, palaeontologists, text editors, musicologists . . .); as linguists by general linguistic theory of one kind or another. But palaeontologists are likewise constrained by biological theory, cosmologists by physics. The central theme is the subtle and complex interplay between the historian's general craft, with its special argument types, and the demands and constraints of one particular kind of historian's subject-matter. This is played out against the background of some major philosophical issues that arise from these considerations, e.g. questions about ontology, realism and conventionalism, explanation, and the like.

The book falls into two unequal parts: chapters 1–5 deal essentially with methodological issues (the nature of historiography, interpretation of documents, protocols and justifications for reconstruction, the relations between contact and endogenous change); chapters 6–7 draw on this material (and other things) and treat larger-scale and more controversial matters, such as the possibility of construing languages in time as dynamical systems, the problem of explanation, ontological relativism and commitment, and the roles of individuals and populations in the description and explication of change.

Current mainstream historical linguistics concerns itself largely either with adding to our store of historical knowledge about particular languages or language families, or trying to explain and interpret language history, often so as to make it fit into or be susceptible to manipulation by the hottest Designer Models (or competing ones). But there is a place for a more Olympian perspective, for foundation studies, correctives to excessive concentration on modelling and explanation, or concentration on them without reflection on what it is that's being modelled or explained. Neglect of such matters can lead to neglect (or for students a total ignorance) of the nasty problems lurking behind even something so elementary as a simple statement of a 'sound change' (see particularly chapter 6). The general questions about storytelling raised above suggest more refined versions. These concern issues that, if perennial, are still important, and the results aren't yet all in.

Where for instance does our 'data' come from? How trustworthy is it? What (if anything) is the empirical basis for the reconstructive techniques we standardly take as yielding 'facts' about the linguistic past, and showing us what changes have occurred? How much does the historian create his data rather than 'receiving' it? Does modelling ever really 'explain' anything, or yield 'truth'? What anyhow (in a serious ontological sense) is 'change'? Where does it occur? Is its locus intrapsychic or extrapsychic, or should such questions even be asked? What is the role of the individual, and of his social surroundings and interactions? And so on. In other words, meta-questions that serious historians (or those who use historians' data and interpretations for other purposes) ought to be aware of. Many of these have of course been dealt with in numerous places, e.g. by Henry Hoenigswald (1960, 1973 and elsewhere), Weinreich *et al.* (1968), Anttila (1989), Hock (1991), and of course myself. But there is more, some of it very different, still to be said.

Everyone knows there's no such thing as 'objectivity' or 'neutrality'; but I try less than most to achieve even as much as can be achieved. Much here is unashamedly programmatic or polemical; I push ideas I like, attack ones I don't, rather than trying for a measured, encyclopedic coverage of opinion. This book is perhaps somewhat like Anttila (1989) in intellectual style: a sales-pitch for a personal vision. As a whole though my aim is not debunking, as in my partly ill-advised attack on historical explanation over a decade ago (Lass 1980). But I do enter on a reprise of earlier criticisms of explanation by 'function', 'markedness', 'tendency' and the like, as well as a new attack on hermeneutics (chapter 7), and a full-frontal assault on

recent trends in language classification (chapter 3). The final evaluations in most areas tend however to be reasonably sanguine. This does not imply that everything is rosy, or that we don't gain something from knowing that even our most cherished and well-tried conclusions may be corrupted by inherent problems of method and the interaction of data and technique, lurking indeterminacies in what we think are our sources, or bits of bad logic that have become traditions. If anything, the conclusion might be that our historiographical methods (if not our explanatory strategies) are often better than what they have to work with, and it's this apparently paradoxical relation that guarantees, in some sense, the viability of the enterprise.

There are obvious limitations both in language-base and topical coverage: except for a few excursions into Dravidian and Uralic, I concentrate mainly on Indo-European, especially Germanic, and within Germanic on English; and there is more discussion of phonology than other linguistic levels. This is partly a matter of the history of the subject: Indo-European studies have played a foundational role in the development of the discipline (e.g. our major reconstructive techniques were developed by nineteenth-century Indo-Europeanists, we still cope with 'exceptions' to apparent sound laws using the methods laid out by Karl Verner in his epochal paper of 1875). Some emphasis on phonology is further justified by the crucial importance of the Neogrammarian Hypothesis, which is the only hard guarantee of warrantable linguistic reconstruction and the statement of genetic relationship (see chapters 3, 5). More importantly perhaps, these emphases also reflect my own interests and limitations, and a constitutional unwillingness to use standard (second-hand) examples pulled from other people's treatments of languages whose histories I don't know in detail, rather than those that grow out of my main preoccupation, the history of English.

So as a whole this is mainly 'meta-' commentary on various matters. But you can't do theory without data, and in historical linguistics this often means detailed and intricate philological discussion. This may pose some problems for readers unfamiliar with my favourite data. I will try (undoubtedly to the irritation of Anglicists and Germanists who know it all already, and maybe to the relief of others who don't) to make the denser parts as accessible as I can. I apologize in advance to members of both camps whose needs I fail to meet.

The reader will note that I often draw images, comparisons and even technical terms from other disciplines, especially evolutionary biology. This is deliberate: using unfamiliar terminology and recasting analyses in the

mode of another discipline makes the anti-parochial point that historical linguistics may be less unique than its procedures and metalanguage suggest; that it belongs (among other things) to a general science of historically evolved systems, whose principles are more or less the same regardless of the kind of system involved. Historical linguistics may have at least as much in common with text criticism and palaeontology as with the rest of linguistics (a point known to nineteenth-century linguists, and discussed in detail in many of the papers in Hoenigswald & Wiener 1987 and Naumann *et al.* 1992).

I will therefore at times be rather less 'linguistic' than (generally) 'historical', perhaps even perversely so. But the similarities with other disciplines are striking, and some of these (in particular evolutionary biology) have over the years produced an amount and quality of thought on matters of historiographical praxis and theory superior to what we linguists generally have. (For an unsurpassed example see the treatment of comparative method in biology in Harvey & Pagel 1991.) But it's just as important to note what I'm not doing: by using cladistic terminology and concepts in chapter 3, for instance, I do not imply that language families in any sense 'are' biotic lineages, that a language 'is' an organism, etc. Rather that location-in-history may impose certain attributes on any system, regardless of its others, and that because of this it might be useful to have a more or less field-neutral terminology, so we can see when we and others are talking about the same things. In any case, there may be something of a salutary shock involved in likening the familiar to what is for practitioners in one discipline the less familiar.

There is also a second agenda: to make a stab at questioning a current trend, and arguing for a somewhat reactionary position. Brigitte Nerlich (1992: xi) has suggested that one of the great virtues of nineteenth-century linguists like Whitney, Bréal and Wegener is that they saw that the 'problem' of language change could only be solved 'if linguists stop regarding language as an autonomous entity, ... and instead start to focus on the *actions,* as advocated by Whitney, and the *mind* of the language users, as stressed by Bréal, together with the *situation* in which they use it, as recommended by Wegener' (emphasis original). The perspective Nerlich praises is becoming increasingly characteristic of a major stream in historical linguistic discourse: the action-based theories of writers like Esa Itkonen and Rudi Keller, and the neo-Peircean semiotics of Henning Anderson, Raimo Anttila, and Michael Shapiro.

Interesting and suggestive as much of this work is, I see it as potentially

retrogressive, tending at times to obfuscation and woolliness, or at least misdirection of effort. I would like to resuscitate a complementary (older and more traditional, and I think better) way of looking at things, where humans are not primarily 'language builders' (Hagège 1993), but end-users of historically evolved systems of a certain kind, which share many properties with other such systems. This view is combined with an emphasis not on individuals or communities, but on languages as populations of variants moving through time (this is not of course 'original': for an essentially populational approach on larger scales and at greater time depths than I attempt here, and with somewhat different ulterior motives, see the pioneering work of Nichols 1992.) My basic claim is that the users are not themselves the primary subject matter, and acting as if they are can lead to serious methodological error (see especially chapter 7). I suppose I am a shameless 'structuralist' (i.e. not psychologistic, pragmatic or cognitivist: cf. the papers in Lieb 1992a). My main interest, and I suggest this ought to be at least one prime focus of the discipline, is in systems, not their users; the latter simply have to make do with what's historically presented to them, and cope with it when it changes. I argue that the systems and their users can, and for the sake of methodological clarity ought to, be kept largely separate. This is not of course to say that the users aren't interesting; merely that they and their properties and actions belong to another subject-area, not historical (or perhaps any) linguistics proper. This raises some difficulties about the status of pragmatics that I will largely evade, but will at least touch on in chapter 7.

Portions of this book have appeared elsewhere in preliminary or different form (this will be indicated in notes). Sometimes I have incorporated material from earlier pieces, which is not surprising, as I've been obsessed with many of the same problems for the past thirty years, and now in some cases what I'm saying is not so much radically new, as just a lot more refined than my early clumsy attempts. In some instances I've quoted myself because I couldn't find a better way of saying something than I did the first time; in others I may have done so without knowing it, since I couldn't think of another wording. A similar apology might apply to certain recurrent bits of iconography, particularly pandas, male nipples, and the bones of the middle ear.

The idea of turning an incipient textbook into something larger, more difficult and probably more confusing came originally from Judith Ayling, who can be blamed only for letting me go on after she'd seen the first chapter. I was also greatly encouraged by Sir John Lyons: both indirectly,

in that his habit of reflecting in a complex and enlightening way on what others have always thought was simple and unproblematic has been a perpetual inspiration to me, and directly, in his early encouragement of this project.

For a proper list of all the people who, to my eternal gratitude, have wormed their way into my intellectual foundations, see the acknowledgements in everything I've published since 1969. But there are some special debts, particular to the subject(s) of this book, or connected directly with its writing. Some go back a long way: the greatest perhaps is to Helge Kökeritz, late Professor of English at Yale, who first got me interested in the history of English three decades ago, taught me a scholarly rigour that I still try (not always successfully) to emulate, and communicated a love for the intricate and detailed argumentation that is the lifeblood of the dedicated historian. A second and continuing debt is to Henry Hoenigswald, whose *Language change and linguistic reconstruction* may be partly blamed for turning me into a historical linguist, and whose subsequent writing and generous comment on my own have helped continue my education; even and maybe especially when we disagree on fundamentals. The same might be said for Raimo Anttila, Raymond Hickey, Rudi Keller, Nigel Love, Bob Stockwell and Elizabeth Traugott. Any attempt to name all the other colleagues and friends who have contributed to whatever I've made out of this would be invidious, as I'd be bound to leave out huge numbers, especially those whose ideas I may have pinched without knowing it.

But in relation to this book I am especially grateful to Hans Lieb for encouraging me to keep thinking in some odd directions; to Don Ringe for elegant support of some intuitions about language families that I thought were weirder than they are; and to Nigel Love, Peter Matthews, and Rebecca Posner, who took the time to read drafts in various stages of disarray, and helped enormously; and to my students at University of Cape Town, especially Claire Cowie and Paula West, who badgered me from time to time into making some of my odder points expressible in a natural language, and shot me down when necessary.

I also owe a special debt to the Deutsche Gesellschaft für Sprachwissenschaft, for organizing a summer school in Göttingen in 1992, at which I had the privilege of spending two weeks talking, drinking, and perhaps more important teaching with Elizabeth Traugott and Raimo Anttila, an experience which among other things showed me that a lot of the issues that have been worrying me for ages are still alive for others as well.

And above all special thanks to Judith Ayling for friendship, unfailing support over a long period of sporadic and inefficient work, and a dogged insistence that this impossible object could in fact be written. At one particularly bad moment she sent me an e-mail that said 'finish the bloody book', and I did.

It's conventional, though probably unnecessary, to end with a ritual disclaimer about one's errors being one's own: who else's could they be? A huge number of these very things were sorted out by Jenny Potts, who saved me from looking like a fool quite often.

Conventions

In general written forms from various languages will be cited in the standard orthographies, or for non-roman alphabets in standard transliterations (unless the original graph-shapes are significant). The main exception is Yiddish, where I use a more or less phonemic 'European' (and partly Slavonic-based) transliteration instead of the Anglified 'Yivo' orthography that tends to be used more often nowadays. Phonetic and phonological *transcriptions*, however, generally follow IPA conventions, regardless of practice in particular fields. For example, though some traditions (e.g. Slavonic) use [č] for [ʧ], I will always use the latter; though standard Indo-Europeanist reconstructions use [y] for a palatal liquid or glide, I will use [j] ([y] in a transcription will always indicate a high front rounded vowel). If a tradition uses a macron or acute for length I will use this only in orthographic forms: so OE *dōm*, Old Icelandic *dómr* 'judgement', respectively /doːm/, /doːmr/.

In a few cases (e.g. some Uralic languages) where tampering with conventional transcriptions would make the citations too different from what is found in the literature, I will use the standard forms, but explain them when they first appear.

'English' in general (where no particular variety is at issue) will be represented by a generalized Southern British Standard type (see Lass 1987a); so *cat* /kæt/ but *pass* /pɑːs/, and so on. This should not pose a problem for non-British readers, who should be able to make the requisite adjustments in the few cases where this is necessary.

In glosses, morph(eme)s are separated by hyphens, and categories fused or cumulated on single morphs are separated by slashes:

> walk-s
> walk-pres/3/sg

Following standard practice, orthographic representations are in *italics*, phonemic in / /, phonetic in [ ], and graphemic in ⟨ ⟩. The single asterisk *