

Explaining Language Change

An Evolutionary Approach

WILLIAM CROFT



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Explaining Language Change

An Evolutionary Approach

WILLIAM CROFT

To the memory of Keith Denning

1955–1998

Scholar, colleague, friend

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Preface and acknowledgements

This book presents a framework for understanding language change as a fundamentally evolutionary phenomenon. It is part of a larger research program, outlined in 'Autonomy and functionalist linguistics' (Croft 1995a). In that article, I argued that there is a variety of theoretical positions that are unfortunately lumped together under the labels 'formalism' and 'functionalism'. These positions fall into two general classes. The first class comprises positions taken on the relationship between the conventional syntactic and semantic knowledge in the minds of individual speakers (their grammar). I argued for a position on this issue which I then called 'typological functionalism'. This position is defended in Croft (to appear b), which should be thought of as a pendant to this book.

The second class comprises theoretical positions taken on the nature of the relationship between a speaker's grammar and language use, manifested in the utterances produced by a speaker in discourse. In Croft (1995a), I argued for a position on this issue which I called 'integrative functionalism'. Specifically, I argued that: one cannot deny the existence of grammar, that is, knowledge of linguistic conventions in a speaker's mind; one cannot disentangle this knowledge from the act of using language; language function influences language form through the dynamics of language use and language change. The evolutionary framework of language presented here is intended to fulfill the promise of an integrative functionalist model offered in that article.

In fact, the main task of integration in writing this book was to acquaint myself with various aspects of pragmatics, discourse analysis, semantics, language acquisition, historical linguistics and several sub-areas of sociolinguistics, not to mention evolutionary biology, in order to work out the consequences of an evolutionary model of language. I hope that I have not committed too many scientific gaffes in the process, and that the framework presented here, programmatic though it is, will be of interest to specialists in those areas. Nevertheless, I believe that specialization has gone much too far in contemporary scientific research, and I hope that in this book, particular specialists will find interesting and important connections to ideas and facts in sub-areas of linguistics other than their own, as well as connections to ideas in evolutionary biology and philosophy of science.

The central thesis of this book originated with an idea which first occurred to me as a doctoral student taking a course in sociolinguistics from the late Charles A. Ferguson – that the proper linguistic equivalent to the genome was

not a speaker's grammar but the utterance. I owe a great debt to him and to Joseph Greenberg and Elizabeth Traugott. Still, I would not have gotten anywhere without some biological thinking. I was first inspired by the evolutionary biology common core course at the University of Chicago taught in 1975–76 by Charles Oxnard (thank Chicago for general college education!). Susan Stucky introduced me to Mayr (1982) at Stanford. My thoughts were fertilized in the late 1980s by two biologists at the University of Michigan, Elena Tabachnick and Barbara Lundrigan, both of whom I thank for bringing me further up to date in developments in evolution and systematics. (Elena is to be thanked in addition for bringing Grant 1981 to my attention, and inspiring the title for §8.1.)

But the catalyst that led to this book was a fortuitous accident (evolutionary biologists are fond of emphasizing fortuitous accidents). I read a review of Harry Collins & Trevor Pinch's *The golem: what everyone should know about science*, and went to seek it out at the Manchester University library. Fortunately, Manchester is an open-stack library, and since I had just arrived in Manchester, I browsed the philosophy of science shelves. In this way I discovered that in *Science as a process*, David Hull, a biologist turned philosopher of science, had worked out a generalized theory of selection for the evolution of concepts as well as organisms, which gave me the framework that I needed. Meanwhile, the needs of the Department of Linguistics at the University of Manchester obliged me to teach courses in sociolinguistics, discourse analysis and grammatical change, which gave me the opportunity to flesh out my recreational reading in those areas, discover new and interesting ideas that fit in with the evolutionary framework, and present some of those ideas to students. I would like to thank the Department of Linguistics and the students in those classes. I would also like to thank the University of Manchester and the Humanities Research Board of the British Academy for funding sabbatical leave and research leave respectively, which allowed me to complete this book.

I have also benefited from audiences at the University of Manchester, Università di Pavia, Lunds Universitet, Cambridge University, Stanford University, the Max-Planck-Institut für Psycholinguistik in Nijmegen, the Conference on Functional Approaches to Grammar in Albuquerque, the XII International Conference on Historical Linguistics in Manchester, and from three anonymous referees for an anonymous journal, who were exposed to earlier versions of parts of this work. Thanks go to many others who individually commented on or discussed various parts of this work with me, including Mira Ariel, Melissa Bowerman, Joan Bybee, Herb Clark, Bernard Comrie, Sonia Cristofaro, David Denison, Guy Deutscher, Martin Durrell, Ray Gibbs, Joseph Greenberg, Martin Haspelmath, Bernd Heine, Masja Koptjevskaja-Tamm, Tania Kouteva, Yaron Matras, Lesley Milroy, Jim Milroy, John Ohala, J. C. Smith, Elizabeth Traugott, Peter Trudgill, Åke Viberg, Nigel Vincent and Tess Wood. I hope that those whose names elude me at this moment will accept my apologies. I would also like to thank Henry, Melissa, Mira and especially Carol for the emotional support they gave me while I was writing this book.

Last, but not at all least, was the inspiration of the late Keith Denning, my best friend in linguistics. As doctoral students together at Stanford, and later as nearby colleagues in Michigan, we talked incessantly about typology, historical linguistics and sociolinguistics and the great need for our field to integrate all of these approaches to the dynamic and variable essence of language. Keith was generous, funny, enthusiastic, and passionately dedicated to his students and to the study of language. This initial attempt at integration was inspired to a great extent by Keith. He was the first person I would have wanted to read this book. I still can't believe I won't hear his voice giving me constructive criticism on this book. I dedicate this book to his memory.

Introduction

This book presents an evolutionary framework for understanding language change, interprets major current theories in the context of this framework, and makes certain new proposals for aspects of the theory of language change. Although this is an ambitious agenda, there has been much interesting work in the theory of language change in the past two decades, and I believe the time is ripe for an attempt to integrate the various advances and insights into the nature of language change.

1.1 On theories of historical phenomena

Why do languages change? This is a difficult question to answer. But part of the difficulty lies in our view of the thing about which the question is being asked, namely, a language.

Language change is a historical phenomenon. Hence the study of language change – historical linguistics – must satisfy certain basic requirements. The first requirement is that one must clearly distinguish historical entities from the types they represent. A historical entity is a spatiotemporally bounded individual, that is, it is a specific entity that exists in a specific place for a specific period of time. This is basically the distinction between a token of something and the type it belongs to. The distinction is clear when describing physical phenomena: a droplet of water is a collection of tokens of the molecular type H_2O . Tokens of water can come into existence and pass away via various chemical processes. There are general chemical laws to describe the chemical processes undergone by water.

The distinction is not so clear when dealing with entities such as languages. Certainly, it is recognized that particular languages exist during a particular time and place. Proto-Germanic arose somewhere in northeast Europe (or farther east) at a certain time, expanded its range, and came to an end with its breakup into what ultimately became English, German, Swedish, etc. More precisely, Proto-Germanic was identified as the language of a finite speech community, whose origin, spread and breakup determined the spatiotemporal boundaries of the historical entity of Proto-Germanic. Its daughter languages, that is the languages used by certain other speech communities, in turn arose in certain locations, perhaps migrated or spread, and will eventually die out or break up themselves.

But this historical view is often set aside for a different characterization of a language. In this characterization, a language such as German is a system of rules and forms, divided into a phonology, morphology, syntax, semantics and so on (in varying ways depending on one's grammatical theory). This system is treated as an idealized entity, abstracted away from any particular speaker's mental knowledge or its uses in particular discourse contexts. Is this entity a token or a type? It is neither. The German language system is not a token because it has been removed from its historical context: one is not analyzing the mental knowledge of actual speakers, or actual occurrences of use. The German language system is not a type because it is still a specific entity: German is still a specific language; its rules do not apply to language in general but to the particular language of German. To the extent that its rules apply at all, it is only to an abstract yet particular entity that has only an ideal existence.

One consequence of the idealization of a particular entity is that it gives the impression that there could be laws governing processes of change in the German language system, that is, the impression that one of the tasks of historical linguistics is to predict what changes to the German language system will occur and at what time, just as one predicts what will happen to molecules of H_2O when subjected to electrolysis. Yet this is patently not the case: we cannot predict what changes will happen to the German language system. H_2O is a type; the German language system is not a type. This fact has led some linguists to argue that historical linguistics cannot be a science because it cannot provide a predictive explanation (Lass 1980:xi).

I suggest that a reason why historical linguistics – and perhaps linguistics itself – as practiced by some might not be a science has to do with what the object of study is taken to be. When linguists analyze language as an abstract system, they are not looking at a historical entity, nor are they looking at a type about which predictions can be made. In fact, they are not looking at anything that is real at all, either as a type or as a token. An empirical science must examine real, existing entities, and then construct generalizations about what types are involved, as well as principles and constraints governing the structure and behavior of those types.

In the study of linguistics, the real, existing entities are utterances as they are produced in context, and speakers and their knowledge about their language as it is actually found in their minds. From these basic tokens, we can describe more complex tokens, such as a specific language or a speech community; and then we can construct types, such as 'language' or 'grammar' (mental representation), over which constraints and generalizations can be made.

I am not arguing that in doing so, we can construct a predictive model of language change. In all probability we will not be able to make detailed predictions, any more than historical sciences of natural phenomena, such as meteorology, astrophysics or geology, are able to do. There are two possible reasons for this, one 'optimistic', one 'pessimistic'. The 'optimistic' one is that we simply do not know the facts in particular cases in enough detail to predict the changes; if we did know, we would be able to make predictions (cf. Keller 1990/1994:159).

The 'pessimistic' one is that we would never be able to predict the change because there is at least some element of randomness in the process, as is the case with the randomness of mutation in biological reproduction and the random factors for survival of individual organisms.

I am inclined towards the pessimistic view with respect to language change, which implies that even with perfect knowledge of the initial state, we would not be able to predict a language change. But not all explanations of historical phenomena need predict the outcome of individual cases. With some types of processes, what really matters is probabilities of change: the cumulative effect of the probability leads ultimately to an overall change. Historical explanations can be, and often are, probabilistic. Probabilistic explanations are particularly effective when the object of study is a population: a gene pool or a population of organisms, for example – or a population of speakers, or of utterances. Both speakers and utterances form populations, and that is what allows probabilistic mechanisms of language change to be effective explanations.

Processes can be roughly divided into two types (see Hull 1988:410). One involves INHERENT CHANGE: a single object that exists over time changes in some way or other. An example of inherent change is human physiological development over its lifetime. Another example is human linguistic development, that is, the development of mental structures that we interpret as representing linguistic knowledge, over the lifetime of the speaker.

The other type of process involves REPLICATION: the creation of a new entity that preserves in large part the structure of its parent entity (or entities). An example of replication is the creation of the DNA of the offspring of an organism, replicated in MEIOSIS. Another example of replication is the production of a linguistic utterance, which replicates grammatical structures of previously occurring utterances of the language, according to patterns of knowledge in the minds of their producers. Another example which is often thought of as replication, but is much more indirect, is the learning of grammatical knowledge on the part of a child, compared with the grammatical knowledge in the minds of its parents and/or caregivers.

One of the problems with treating language as an idealized abstract system is that it makes language change into an inherent process: a single object – the abstract language system – changes over time. But the real, existing entities of linguistics are utterances and speakers' grammars. The evolution of both of those entities, particularly utterances, occurs through replication, not inherent change.

Replication can result in change at two levels. At one level, change can occur because the structure of the replicate is not exactly the same as the structure of the original. For example, I may pronounce *bad* in an utterance with a slightly higher vowel than in earlier utterances which I heard and internalized. I will call this ALTERED REPLICATION. Altered replication produces variants of a structure. At the other level, change can occur by the shift in the frequencies of variants of a structure. For example, more and more utterances occur with a higher vowel in *bad* than with the lower variants. Perhaps *bad*

with the lower vowel variants will eventually die out. This sort of change is called DIFFERENTIAL REPLICATION (Hull 1988:409).

The position taken in this book is that the study of language is about empirically real entities, not idealized abstract systems. The real entities of language are utterances and speakers' grammars. Language change occurs via replication of these entities, not through inherent change of an abstract system. In chapters 2–3, I will argue that the primary replicators are in fact utterances, not speakers' grammars; but the point that matters here is that a theory of language change must be a theory of replication of empirically real entities, either grammars or utterances.

1.2 Desiderata for a theory of language change

We are now in a position to outline some desiderata for a theory of language change.

First, a theory of language change must avoid the reification or hypostatization of languages. If one speaks of 'forces' causing a language to change, such as Sapir's concept of drift (Sapir 1921), then one is speaking as if language change is an inherent change applying to an abstract system. Descriptions such as '[a phoneme's] performance represents an extreme phonetic possibility as when it is an /i/ badly pressed by an invading /e/ with surrounding diphthongs which block all way of escape' and 'Isolated phonemes do not rush into structural gaps unless they are close enough to be attracted' (Martinet 1952/1972:147, 159) are examples of reification taken to excess. Languages don't change; people change language through their actions (Croft 1990:257). Keller describes this desideratum as the principle of methodological individualism: 'the explanation [of a language change] is based on acting individuals, not languages, structures, processes, or collectives' (Keller 1990/1994:121).

Second, a theory of language change must explain why languages do NOT change in many ways, sometimes over many generations of speakers (see Milroy 1992b:10–13). Many theories of language change focus their attention on mechanisms to bring about change. But if those mechanisms were the only mechanisms around, then languages would be changing constantly in all of their respects. Yet they are not. A theory of language change must provide for mechanisms that act as forces for stability as well as for change, and ideally get the proper balance in order to account for rates of change. Another way of putting this desideratum is to say that a theory of language change must provide mechanisms of NORMAL (identical) REPLICATION as well as altered replication (and also mechanisms for nondifferential replication as well as differential replication).

Third, a theory of language change must distinguish the two processes of change, that is, it must distinguish altered replication from differential replication. To use the terminology more typically found in linguistics, the two processes are INNOVATION or actuation – the creation of novel forms in the language – and PROPAGATION or diffusion (or, conversely, loss) of those forms in the

language. Both processes are necessary components of the process of language change. The distinction between these two processes, and the fact that both are necessary components of language change, is very rarely recognized in models of language change (but see Jespersen 1922:166–7). Because of this, some apparently contradictory positions have been taken on the nature of language change.

Some linguists argue that only innovation is a language change. Joseph writes, 'language change always takes place in the present, i.e. it always occurs in some speaker's (or group of speakers) present' (Joseph 1992:127; see also Joseph & Janda 1988). Joseph can only be speaking of innovation, as propagation of a novel form does take place over long periods of time, exceeding the life spans of individual speakers. Others argue that only propagation constitutes a language change. James Milroy writes, 'a change in the output of a single speaker might be regarded as the locus of a change in the system, whereas of course a change is not a change until it has been adopted by *more than one speaker*' (Milroy 1992a:79, emphasis original; see also Labov 1982:46). Others do not recognize the distinction, leading to misunderstandings such as that in the following passage:

How can one 'understand' or get at 'the intention behind' or discover the 'meaning' of a shift from SOV to SVO, or a monophthongization, or a vowel-shift, etc., in any 'cognitive' or empathetic way, or 're-enact' them, or attribute them to 'reasons' and 'beliefs'? Especially as linguistic changes . . . typically unfold over very long periods of time, most often beyond the lifetime of any human 'actor'. (Lass 1997:339)

Lass is criticizing hypotheses about the innovation of a language change – hypotheses appealing to speaker intentions – by citing a fact about the propagation of a language change – propagation typically extends over many generations. Lass may be correct that innovation does not involve intention; but not for the reason he gives. Lass is not alone in this misunderstanding; but neither do the advocates of such explanations (in this case, Anttila 1989) tend to distinguish innovation and propagation, thereby inviting the erroneous criticism.

One consequence of recognizing that innovation and propagation are distinct but jointly necessary processes for language change is recognizing that language change is both a synchronic and a diachronic phenomenon. Innovation is a synchronic phenomenon, as Joseph notes in the quotation above: it occurs in speaker action at a given point in time. Propagation is a diachronic phenomenon: it occurs sometimes over a very long period of time, even centuries. On the other hand, sociolinguistic research has demonstrated that one can observe propagation occurring in less than the lifetime of a speaker (see e.g. Trudgill 1988).

Fourth, a comprehensive framework for understanding language change must subsume structural, functional and social dimensions of language change, or their equivalents. Most current approaches to language change address only

one or another of these dimensions. Sociohistorical linguistics examines almost exclusively the social dimensions (although Labov 1994 discusses the phonetics and phonology of vowel shifts and mergers in some detail). Traditional philological and structuralist approaches focus on structure and to a lesser extent on function. Generative theories focus on structure exclusively, though largely because they deny function or social factors any interesting or central role in language change. Grammaticalization theory focuses on structure and function, with a stronger emphasis on the latter. To the extent that function and social factors play a role in language change – and there is strong evidence that both do – they must be integrated in a single framework.

Last, a comprehensive framework for understanding language change must subsume both INTERNAL and EXTERNAL causes of language change. Theories of internal causes are varied, and most attention has been focused on them. Externally caused changes, that is, changes caused by contact such as borrowing and substratum phenomena, tend to be discussed relatively little in theories of language change, and are typically placed in separate chapters from internal causes in textbooks on historical linguistics. Contact-induced change appears to have an obvious source – the other language. Nevertheless, mechanisms for the innovation and propagation of contact-induced change, preferably mechanisms comparable to those posited for internal changes, must be established.

The framework presented in this book satisfies all five desiderata for a theory of language change, and thus can be counted as a comprehensive framework for understanding language change. However, it is only a framework; it is hoped that future work will be able to flesh out theories of specific types of changes in this framework. The last section of this chapter briefly outlines the framework.

1.3 An utterance-based selectional theory of language change

The framework for understanding language change to be presented here is based on a generalized theory of selection for all types of evolutionary phenomena, originally developed in biology and applied to the history of science by David Hull (Hull 1988; see Dawkins 1976). The generalized theory of selection, described in chapter 2, distinguishes variation (altered replication) from differential replication. It specifically provides a model of selection, which Hull argues is the mechanism for differential replication for most types of biological evolution and also conceptual evolution in the history of science. Hull also emphasizes that replication and selection involve historical entities (tokens), not types (except for very general types such as 'gene', 'species', 'population' and so on: see chapter 2). The generalized theory of selection thus satisfies the first three desiderata enumerated in §1.2.

The framework presented in chapter 2 can be used to interpret the major extant theories of language change; they are surveyed in chapter 3. However, in chapter 2, I also present the first of four major theses about the nature of

language change set forth in this book. I propose that UTTERANCES, more precisely the replication of linguistic structures in utterances in language use, play a central role in the theory of language change. A widely held view treats language change as occurring in the process of 'replicating' a grammar in child language acquisition. There are however serious empirical problems with this view, discussed in §3.2. For this reason, the Theory of Utterance Selection, which is more or less compatible with most aspects of sociohistorical linguistics, grammaticalization theory and the invisible hand theory (see §3.3), is pursued here.

In the Theory of Utterance Selection, CONVENTION is placed at center stage. Normal replication is in essence conformity to convention in language use. Altered replication results from the violation of convention in language use. And selection is essentially the gradual establishment of a convention through language use.

Convention does not generally take center stage in linguistic theory, formalist or functionalist. The reason is that conventions are essentially arbitrary (see §4.2.4). Saussure considered arbitrariness to be central to the understanding of language: '[The arbitrary nature of the sign] dominates all the linguistics of language; its consequences are numberless' (Saussure 1916/1966:67–8). Yet neither formalists nor functionalists have shown much interest in arbitrariness per se. Functionalists are chiefly interested in the nonarbitrary, functionally motivated aspects of grammatical structure. Formalists find arbitrariness useful in criticizing functionalist analyses, but they are chiefly interested in general, formal universals that have an innate basis. But a central aspect of a speaker's use of language is convention. When I say *Who did you meet yesterday?*, I put the interrogative pronoun *Who* at the beginning of the sentence because that is the convention of my speech community, I know the conventions of my speech community, and my use of language will serve its purpose best most of the time if I conform to the conventions of my speech community. It may be that the initial position of *Who* is partly motivated by pragmatic universals of information structure, or partly specified by an innate Universal Grammar. In fact, one (or both) of those factors may be the motivation for the origin of the convention. But that is not why I have put it there in that utterance. As Saussure wrote: '[arbitrary conventions] are nonetheless fixed by rule; it is this rule and not the intrinsic value of the gestures that obliges one to use them' (Saussure 1916/1966:68). Convention – whether conforming to it, violating it, or establishing it – plays a key role in language use and in language change. (It also plays a key role in our knowledge of language; see Croft to appear b.)

Convention is a property of the mutual knowledge or COMMON GROUND of the speech community. Of course, common ground is found in the minds of speakers, albeit shared with other members of the speech community. Thus, there is an interplay between convention and individual speakers' knowledge, or COMPETENCE as it is usually called. There is also an interplay between conventional and nonconventional aspects of language use, which plays a critical

role in the understanding of how replication of linguistic structures in utterances occurs. All of these concepts are discussed in some detail in chapter 4.

The second major thesis of this book pertains to the nature of grammatical (and lexical) change. The causal mechanisms for innovation involve the mapping from language structure or form to language function, that is, meaning in context (Croft 1995a; see §4.3 for some discussion of other senses of the word). This mapping occurs at two levels or interfaces. One is the mapping from phonological structure to phonetic reality (articulatory and auditory). The other is the mapping from grammatical (morphosyntactic) structure to its semantic/pragmatic/discourse function in context. Neither mapping has been found by linguists to be simple to represent. But nor is it simple for speakers to represent these two levels of mapping. In chapters 4–6, I argue that altered replication is essentially a result of speakers adjusting the mapping from language structure to external function. I focus chiefly on grammatical change, with a few references to sound change.

The mechanisms for innovation in language change involve both structure and function. The mechanisms for propagation, on the other hand, are essentially social, namely the various factors discussed by sociolinguists (see §3.4.3, §7.4.2). In other words, there are two distinct mechanisms operating in language change – this is my third major thesis (see also Croft 1995a:524; 1996a:116–17). The mechanism for innovation is functional, that is, involves the form–function mapping. The mechanism for propagation is a selection mechanism, in the evolutionary sense (see §2.3), and it is social. Thus, the integration of structural, functional and social dimensions of language change is achieved largely by integrating the two distinct processes of change, innovation and propagation/selection.

The last major thesis about language change presented in this book pertains to the relationship between internal and external (contact-induced) sources of language change, which is more complex than is usually assumed. In chapter 4, I argue (following sociolinguistic theory) that a speech community is defined in terms of domains of use, not in terms of collections of speakers. All people in a society are members of multiple speech communities, whether those communities are conventionally described as representing a single language or multiple languages. In other words, all speakers command multiple varieties or codes, and thus some of the mechanisms for internal sources of change are the same as those for external sources of change. These mechanisms are discussed in chapters 6–7. The blurring of the line between internal and external changes also allows us to integrate the study of language contact and genetic linguistics. The naturalness of this view becomes more apparent when one recognizes that language ‘speciation’ is more like plant speciation than animal speciation. This view of language speciation is discussed in chapter 8.

Chapter 2

An evolutionary model of language change

The formation of different languages and of distinct species, and the proof that both have been developed through a gradual process, are curiously parallel . . .

Charles Darwin, *The descent of man*

2.1 Introduction

The relationship between language change and biological evolution has been debated since the emergence of linguistics as a science in the nineteenth century, at around the same time as the emergence of evolutionary theory. The debate has increased in recent times. One can identify three separate ways in which biological evolution has been connected to linguistic evolution in recent discussion.

First, interest has revived in the evolution of language, that is, the evolution of the human linguistic capacity (Pinker & Bloom 1990; Hurford, Studdert-Kennedy & Knight 1998; Kirby 1999). The evolution of human linguistic capacity is directly a biological process: some genetic change among ancestral primates led to the creation of a social and cognitive capacity for language or a language-like system for communication, and some process selected those primates with that capacity, leading to humans as a speaking species. This topic, while interesting, is also extremely speculative, and will not be surveyed in this book: this book is concerned with language change itself, not the evolution of a certain biological capacity of human beings.

Second, interest (and controversy) has arisen over the so-called genetic origin of contemporary human languages. Here the evolutionary connection is one of historical association. The internal structure of genetic families such as Austronesian are compared to the distribution of biological traits, such as alleles in mitochondrial DNA, or blood types (Bellwood 1991). It is assumed that, for the most part, transmission of biological traits through offspring is historically paralleled by transmission of language from parents to children, and hence family trees of human communities based on biological traits should roughly parallel family trees constructed on linguistic evidence.

Of course, all know that this parallelism in the history of languages and of human biological traits is not necessary. Languages are not transmitted via an individual's DNA; biological traits are. Rather, languages are transmitted to new speakers through exposure to their use. A group of people may abandon

their language and adopt one of another group to whom they are not biologically closely related. For more recent families such as Austronesian, where the demographic history is better known, the relative contribution of language shift vs parent-to-child transmission can be more easily sorted out. In Austronesian, for example, it is clear that Melanesians are biologically closer to Papuans, and presumably have shifted to the Austronesian languages that they now speak (cf. Melton *et al.* 1995; Redd *et al.* 1995).

Relationships have also been observed between proposals for historically deeper linguistic families such as Amerind (Greenberg 1987) or even Proto-World (Bengtson & Ruhlen 1994) and proposals for historically parallel biological phylogenies (Greenberg, Turner & Zegura 1986; Cavalli-Sforza *et al.* 1988). These are much more controversial, chiefly because of challenges to the linguistic classification; the classification based on some biological traits (in particular mitochondrial DNA) is also questioned. Again, this relationship between biological evolution and language change will not be surveyed in this book: this book is concerned with the mechanisms of language change, not the origin and spread of specific families of languages.

The third connection between language change and biological evolution is found where the theory of biological evolution itself has been adopted, or adapted, in order to construct an evolutionary theory of language change (see e.g. Keller 1990/1994:141–52; McMahon 1994:314–40; Lass 1990, 1997 *passim*; Ritt 1995). Evolution is recognized as a process that occurs with certain types of entities. The process is probably best understood as it occurs with populations of biological organisms; that is evolutionary biology. The hypothesis is that language change is an example of the same process, or a similar process, occurring with a different type of entity, namely language. It is this hypothesis that forms the starting point of this book.

A number of approaches have been taken to an evolutionary model of language change. The first approach is literal: language is a genetic capacity, and hence obeys certain principles of biology. This approach is associated with Chomskyan linguistics, because Chomsky argues for the biological basis of quite specific linguistic properties (e.g. certain syntactic structures and constraints). The literal approach also makes developmental claims: for instance, the hypothesis that the human language capacity in all its detail emerges in maturation.

However, the main goal of the literal approach is to claim a biological basis for the universal properties of languages. The ways in which contemporary human languages are divergent, and have diverged or will diverge in history, cannot be accounted for in the literal approach. A literal approach to language diversity would amount to claiming that the differences among languages reflect genetic differences among their speakers. This is patently false, as can be seen from the aforementioned fact that a person can learn a second language, and learns whatever language is spoken in their surroundings. For this reason, the literal approach generally turns to questions of the evolution of the human linguistic capacity, that is, what gave us the genetic basis for the properties common to all languages, whatever those may be.

The second approach is essentially analogical: there are analogies between certain biological processes as described by evolutionary theory and certain processes of language change that call for description. Hence, linguists seeking better descriptions and analyses of those processes can borrow or adapt the descriptions and explanatory mechanisms that evolutionary biologists have proposed. However, no deeper claim is made about the relationship between the theory of evolution in biology and the theory of language change in linguistics. There are simply analogies or metaphors between a process in one domain of scientific study and a process in another domain; in particular, one should not push the analogy too far. In the analogical approach, the relationship between evolution and language is essentially opportunistic – an opportunity for linguists to utilize some already developed theoretical constructs.

An example of the analogical approach appears to be the use of the biological metaphor in creole studies, e.g. Whinnom (1971) and Mufwene (1996a, to appear). Whinnom suggests that the biological concept of hybridization can be applied to language contact ‘provided that the analogies are properly applied’ (Whinnom 1971:91). Mufwene compares languages to species and the factors that determine a language’s survival or extinction as ecological factors, but states:

I do not want to suggest that language evolution is in all, or most, respects like species evolution . . . There are, however, some similarities between the concepts of *language* and *species*, which I find informative and would like to use cautiously to shed light on the process of language evolution. (Mufwene to appear, fn. 1)

It appears that Lass’s adoption of an important concept in recent evolutionary theory, exaptation (Lass 1990), and its application to historical linguistics, is another example of the analogical approach to the relation between biological evolution and language change (see §5.3 for further discussion). Lass writes: ‘while claiming that the notion of exaptation seems useful in establishing a name and descriptive framework for a class of historical events, I remain fully aware (even insistent) that languages are not biological systems in any deep sense’ (Lass 1990:96). He adds, for instance, that ‘There is as far as I am aware no storage or coding mechanism for linguistic transmission equivalent to DNA’ (*ibid.*).

However, Lass makes it clear in the following paragraph that he is not taking an analogical approach. It is not that languages ARE biological systems. It is that languages and biological systems are instances of a more general phenomenon: ‘rather than extending a notion from biology to linguistics, I am suggesting that the two domains . . . have certain behaviors in common by virtue of evolving’ (Lass 1990:96). In fact, Lass is taking a third, generalized approach. This approach proposes a generalized theory of evolutionary processes, which applies to the evolution of species and their traits in biology, to language change in linguistics, and to other domains as well: ‘I am convinced . . . that there is such a thing as a theory of “historically evolved systems”’ (Lass 1997:316). In the generalized approach, there is a profound relationship

between biological evolution and language change, which is worth exploring in greater detail. The two are not identical by any means. But they both display salient properties that demonstrate that they are instantiations of the same generalized theory that crosses disciplinary boundaries.

Even in the generalized approach, the evolutionary biologists are again in the forefront. Lass cites Dawkins in support of this approach. Dawkins' popular book *The selfish gene* (Dawkins 1976), besides making specific proposals about the nature of biological evolution, suggests that his model can be applied to cultural evolution as well. He proposes the notion of a MEME to represent a sociocultural unit that can evolve via differential replication. David Hull, a biologist who became a philosopher of science but not without continuing to make contributions to systematics and evolutionary theory, develops a generalized theory of selection which subsumes both biological and conceptual evolution (Hull 1988, especially chapters 11–12). In this chapter, I will argue that Hull's model can be applied to language change, and allows us to construct the foundations of a theory of language change and the major mechanisms that bring it about.

If the generalized approach is to be taken seriously, then Lass's claim – that there is no equivalent of DNA in linguistic evolution – raises a difficult question. If Lass's claim is true, then are linguistic and biological evolution really instances of the same thing? Does DNA play an essential role in the theory of evolution developed by biologists – developed in far greater detail than the theory of language change in linguistics? If so, then the role in evolutionary theory that is played by DNA in biological systems must have a counterpart in linguistic systems. Lass argues against the literal approach, that is, that there is a genetic basis to the phenomena of linguistic evolution: language change does not occur through biological genetic mutation and selection. But that does not necessarily mean that there is no functional equivalent to DNA in linguistic evolution. Hull writes, 'People reject selection models in conceptual change out of hand because they have a simplistic understanding of biological evolution' (Hull 1988:402). The trick is making the right instantiation of the theoretical constructs of the generalized theory.

In this chapter, I will argue that there is an equivalent to DNA in linguistic evolution, and that it is the utterance. Both the existence of an equivalent to DNA and the entity I am proposing as the equivalent will at first strike the reader as surprising, perhaps even bizarre. But an essential role in the generalized theory of selection is assigned to a function most typically centered on DNA in biological evolution. I will argue that this function is most typically centered on the production and comprehension of utterances in language change. It should be clear from the wording of the preceding sentence that the DNA–utterance analogy is going to be rather indirect and not the one that Lass rightly rejects. In fact, what will emerge from the application of Hull's theory to language change is a theory that is thoroughly based on what happens to language in use, from the origin of an innovation to its adoption as a convention of the speech community, and which unifies internal and external causes

of language change. The remainder of this book will explore some of the consequences of this theory. But first we must explicate the two most fundamental notions in the generalized theory, the population theory of species and the generalized theory of selection.

2.2 Populations and phylogenies

One of the major advances of the so-called evolutionary synthesis is the replacement of the essentialist theory of species by the population theory of species (Dobzhansky 1937, Mayr 1942, cited in Hull 1988:102; see also Mayr 1982). The population theory of species is sometimes traced back to Darwin, but Darwin was not entirely clear in his definition of species (Mayr 1982:265–69; Hull 1988:96, 213 fn. 2; see Mayr 1982:272 for other precursors). The population theory is also inextricably tied up with the question of systematics, that is, the taxonomic classification of organisms into varieties, species, genera, families and higher taxa. Here the contrast is between classification and phylogeny; we must discuss this question as well.

In the ESSENTIALIST view of a species, each species has immutable essential structural properties that identify it (Mayr 1982:256). That is, the essentialist view is that a species instantiates an abstract type. The essentialist view ran into problems due to various sorts of structural variation among species, including high degrees of structural variation among individuals in a population and also among different life-stages in an individual in a population (for example, a caterpillar and the butterfly it turns into, or a species that changes sex over its lifetime; Hull 1988:430). The essentialist view also ran into problems with populations which could not be distinguished by structural features but were distinct reproductive communities (Mayr 1982:271; see below). But the greatest problem for the essentialist view of a species is that a species evolves, and in so evolving, can lose 'essential' structural properties. Identifying this problem is one of the major contributions of Darwin to evolutionary biology.

The POPULATION theory of species is completely different from the essentialist theory (Mayr 1982:272). A species consists of a population of interbreeding individuals who are REPRODUCTIVELY ISOLATED from other populations. This property – interbreeding, and lack thereof between species – is the 'essential' property the individuals have in common. There is no essential species type. Individuals can vary in enormous ways in physical structure (and behavior), but as long as they form a population in the evolutionary sense, they are members of the same species. Conversely, individuals may be structurally extremely similar, but if they come from two distinct reproductively isolated populations, they are members of different species. This is a radically different view of the species as a conceptual category. The category definition is based on a specific set of individuals, and category membership is defined in terms of how the individuals interact with each other, not by any specific traits associated with all and only the individuals in the category.

Not every individual need breed with every other individual in order for the set of individuals to form a population; only reproductive isolation is necessary: 'extensive interbreeding with the population system is not an essential property of biological species; non-interbreeding with other population systems is' (Grant 1981:91). Although reproductive isolation is treated in theory as a sharp dividing line, it is not entirely so in fact (Hull 1988:102-3). There are cases in which populations which were separated and then brought into contact again developed a stable hybrid region in between the two distinct populations. Hull gives the example of the hooded crow and the carrion crow in Europe: separated by glaciers which then receded, the species populations remain distinct, but there is a stable band of hybrids in a zone not exceeding 75 to 100 kilometers in width (Hull 1988:103). Hence there is interbreeding where there is contact, but there is little gene flow between the two populations. Conversely, reproductively isolated populations of plants can merge: 'estimates of the proportion of plant species in general that are of hybrid origin run as high as 30 or 40 percent' (Hull 1988:103; cf. Grant 1981:203).

In the essentialist view of species, a species is a type, defined by a set of properties, that is not located in space or time but in an abstract domain of biological traits. In the population view, a species is a spatiotemporal individual, not an eternal essence. The population of organisms constituting a species is circumscribed by the region in time and space collectively occupied by the individual members of the species. The beginning of a species is defined by its branching off in a speciation process, and its end by either its extinction or its fission into two or more new species in speciation: 'Just as the name "Gargantua" [an individual gorilla] denotes a particular organism from conception to death, "*Gorilla gorilla*" denotes a particular segment of the phylogenetic tree' (Hull 1988:215).

A species, like an individual organism, is a historical entity in the population view. In the essentialist view, a species is not a spatiotemporal individual: it is a type or kind, whose instantiations may be particular individuals, but the kind is not spatiotemporally bounded itself. But species are not types; they cannot be types. The essentialist view treats a historical entity as if it were a type. In the population view, only entities as abstract as species (in general), and certain theoretically defined subpopulations of a species such as demes (in general) or geographical races (in general) are kinds. Any particular species, deme or geographical race is a spatiotemporal individual. In other words, a population is a fundamentally historical entity. This point is a very important one in understanding both the relationship between populations and classification and between populations and selection (see §2.4.1).

If the population theory of species is distinct from the essentialist theory of species, then one would expect to find cases where there are mismatches in the world between species defined in terms of reproductively isolated populations and species defined in terms of essential structural properties. In fact, this is the case (see e.g. Hull 1988:104). *SIBLING SPECIES* are two reproductively isolated species whose structural descriptions overlap to such an extent that on an

essentialist definition, they would be the same species. For example, five different species of the flower *Gilia* in the Mojave Desert are so similar that they were once classified as a single species, but the five species are highly intersterile (Grant 1981:61-2). *POLYTYPIC SPECIES*, on the other hand, are species that are structurally so heterogeneous that an essentialist would be hard put to categorize them as a single species, yet they form an interbreeding population (in terms of gene flow; Mayr 1982:287-92).

A population may split into two or more parts, often through geographical isolation. The term *VARIETY* is generally used for a subpart of a species population. Varieties may cease to interbreed. In fact the two populations may diverge in structure and behavior such that they could no longer interbreed even if brought together again. At this point one would say that the original species has split into two daughter species. (I follow Hull 1988, and Hennigian systematics in general, by assuming the old species no longer exists after such a split, thereby avoiding the pseudoproblem of deciding which of the daughter species is 'really' the continuation of the parent species.) 'Varieties are merely incipient species. Not all varieties become species, but all species at one time were varieties' (Hull 1988:96).

If one constructs a historical account of the splitting (and also merging) of populations of organisms, the result would be a *PHYLOGENY*. A phylogenetic classification is intended to reflect the history of the organisms being classified. A phylogenetic classification is not the same as a taxonomic classification, the familiar classification of species into genera, families and so on. A taxonomic classification is based on similarities and differences among traits. It is basically founded on an essentialist view of species. A phylogenetic classification is historical. Yet the two have often been confused, even by biological systematists (taxonomists). There are two reasons for this confusion. The first is the apparent similarity between a phylogenetic tree and a taxonomic tree: 'The relationship between a branching phylogenetic tree and the successive subdivisions of a hierarchical classification could not seem more patent. Yet, it is not' (Hull 1988:98).

The second is due to a practical problem in determining phylogenies. It is not always practically possible to discover patterns of gene flow in populations. In particular, it is impossible if the species is extinct and one must rely on fossil evidence. Instead, one examines traits, and uses the differences in traits among populations to hypothesize a historical scenario of the successive splitting (and merging) of populations. Since one is using traits to project a phylogeny, it is tempting to use a taxonomic classification to project the phylogeny. But a proper phylogeny requires the differentiation of traits based on their history. If two taxa share a trait, it could be a retained trait from the parent population (a *SYMPLESIOMORPHY*), or it could be a shared innovation of the two taxa (a *SYNAPOMORPHY*). Only a shared innovated trait can justify grouping the two taxa together phylogenetically. A shared retained trait simply indicates that the two taxa may be grouped together phylogenetically with other taxa at a greater time depth. And of course the shared traits may be shared accidentally, that is,