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Introduction Contents

On Language, Change, and Language Change – Or, Of History, Linguistics, and Historical Linguistics

RICHARD D. JANDA AND BRIAN D. JOSEPH

*Fellow-citizens, we **can not** escape history.*

Abraham Lincoln, "[2nd] Annual Message of the President of the U.S. to the Two Houses of Congress; December 1, 1862" original emphasis, reprinted in Richardson (1897: 142)

History is more or less bunk.[1]

Henry Ford as interviewed by Charles N. Wheeler; *Chicago Daily Tribune* 75.125 (May 25, 1916: 10) (repeated under oath during Ford's libel suit against the *Tribune* before a court in Mount Clemens, Michigan (July, 1919))

In this introduction to the entire present volume – a collection of chapters by scholars with expertise in subareas of historical linguistics that together serve to define the field – we seek to accomplish three goals. First, we present and explicate what we believe to be a particularly revealing and useful perspective on the nature of language, the nature of change, and the nature of language change; in so doing, we necessarily cover some key issues in a rather abbreviated fashion, mainly identifying them so that they may together serve as a frame encompassing the various subsequent chapters. Second, we introduce the book itself, since we feel that in many respects this volume is unique in the field of linguistic diachrony. Third and finally, we seize the opportunity provided by the still relatively recent turn of both the century and the millennium to step back for a moment, as it were, and use the image of historical linguistics that emerges from the representative set of papers in this handbook for the purpose of reflecting on what the present and future trajectory of work in our field may – and can – be.

Thus, in the first part of this introduction, we do not hesitate to address extremely general, even philosophical, issues concerning language, change, and language change – whereas, in its second part, we focus on more concrete matters pertaining to the volume at hand, and, in its third part, we present a modest, minimal synthesis that aims to assess what are likely to be the most promising avenues and strategies for investigation as research on linguistic change continues to move forward to (the study of) the past. As we pursue these three goals, we intentionally do not at any point give chapter-by-chapter summaries. Rather, we weave in references to chapters as we discuss major issues in the field, with references to the authors here represented given in SMALL CAPITALS when they occur.

The particular thematic organization of our discussion, however, does not alter the fact that the major sections into which this book is divided follow fairly traditional – and thus for the most part familiar – lines of division: the twenty-five chapters that follow are grouped into sections in such a way as to fall into three main parts. First, in part II, the major methodologies employed in studying language change are presented, with emphasis on the tried-and-true triad of the comparative method, internal reconstruction, and (the determination of) genetic relatedness. Second, in parts III through VI, discussions of change in different domains and subdomains of grammar are to be found: these respectively cover phonology, morphology/lexicon, syntax, and pragmatics/semantics, in that order. In each case, the topics are approached from two or more different – and sometimes even opposing – perspectives. Third, in part VII, various causes of change, both internal and external – and cognitive as well as physiological – share the spotlight. In all of these sections, the long tradition of scholarship in historical linguistics in general is amply represented, but a final indication of the dimensions of the scholarly tradition in these areas can be found in this volume's composite bibliography, which collects all the references from all the chapters and this introduction into a single – and massive – whole.

1 Part the First: Intersections of Language and History in this Handbook

1.1 *On language – viewed synchronically as well as diachronically*

1.1.1 *The nature of an entity largely determines how it can change*

[A] language . . . is a grammatical system existing . . . in the brains of a group of individuals . . . [;] it exists perfectly only in the collectivity . . . , external to the individual.

Mongin-Ferdinand de Saussure (1916: 30–1), trans. Roy Harris (1983: 13–14)

[A] LANGUAGE . . . is . . . a set of sentences . . . [–] all constructed from a finite alphabet of phonemes . . . [– which] may not be meaningful, in any independent sense of the word, . . . or . . . ever have been used by speakers of the language.

Avram Noam Chomsky, "Logical structures in language," *American Documentation* 8.4 (1957: 284)

Linguistic theory is concerned primarily with an ideal speaker-hearer, in a completely homogeneous speech-community, who knows its language perfectly.

Avram Noam Chomsky, *Aspects of the Theory of Syntax* (1965: 3)

The range of possible changes in an entity is inextricably linked with the nature of that entity. This is a truism, but that status does not make such an observation any less significant – or any less true. On a more abstract level, it is directly supported by the differential predictions concerning linguistic diachrony that follow from the above-cited characterizations of language (in general) associated with de Saussure (1916) versus Chomsky (1957, 1965). On the Saussurean view that *langue* is essentially the union of different speakers' linguistic systems, an innovation such as one speaker's addition of an item to some lexical field (e.g., color terminology) may count as (an instance of) significant language change, since any alteration in the number of oppositions within some domain necessarily modifies the latter's overall structure. But no such conclusion follows from the Chomskyan focus on a language as a set of sentences generated by an idealized competence essentially representing an intersection defined over the individual grammars within a community of speakers.

As a more concrete example, consider the diachronic consequences of Lieber's (1992) synchronic attempt at *Deconstructing Morphology*, where it is argued that, in an approach to grammar with a sufficiently generalized conception of syntax (and the lexicon), there is in essence no need whatsoever for a distinct domain of morphology. On such a view, it clearly is difficult – if not impossible – to treat diachronic morphology as an independent area of linguistic change.[2] An idea of how drastic the implications of this approach would be for studies of change in particular languages can be quickly gained by picking out one or two written grammars and comparing the relative size of the sections devoted to morphology versus syntax (and phonology). For example, nearly two-thirds (138 pp.) of the main text in Press's (1986) *Grammar of Modern Breton* is devoted to morphology, as opposed to only 14 percent (30 pp.) for syntax and 21 percent for phonology (44 pp.). Nor is such "morphocentricity" (cf. also Joseph and Janda 1988) limited to "Standard Average European" languages or to what might be thought of as more descriptive works. Thus, for example, in Rice's (1989) highly theoretically informed *Grammar of Slave* (an Athabaskan language of Canada), the relative proportions are roughly the same: 63 percent (781 pp.) for morphology versus only 27 percent (338 pp.) for syntax and 10 percent (128 pp.) for phonology.[3]

While Lieber's morphological nihilism is admittedly an extreme position, it is by no means an isolated one. After all, morphology is so recurrently partitioned out of existence by syntacticians and phonologists alike that it has even been called "the Poland of grammar" (cf.[4] Janda and Kathman 1992: 153, echoed by Spencer and Zwicky 1998: 1). On the other hand, while phonology and syntax themselves – along with phonetics, semantics, and the lexicon – seem to be in no danger of disappearing from accounts of linguistic structure, there is constant variation and mutation (not to mention internecine competition) within and among the major approaches to these domains. Hence, even if there were unanimity among historical linguists concerning the mechanisms and causes of language change, most (if not all) diachronic descriptions of particular phenomena would still remain in a state of continuous linguistic change, as it were, due to the never-ending revisions of synchronic theories and hypotheses.[5]

The present volume attempts to make a virtue of necessity by promoting such manifestations of diversity and (friendly) competition. Subject only to practical limitations of space, time, and authorial independence, we have – for selected individual aspects of language change – tried to match each chapter that depends on a particular synchronic perspective with one or more opposing chapters whose approach is informed by a specific alternative take on linguistic theory and analysis. For example, chapter 14, which is imbued with DAVID LIGHTFOOT's commitment to approaching syntactic change from a formal starting-point, can be juxtaposed with chapter 17, which reflects MARIANNE MITHUN's exploration of functional explanation in both synchronic and diachronic syntax. This handbook thus follows an inclusive strategy that omits no traditional subfield of historical linguistics (as opposed, say, to the exclusions which would result from accepting the diachronic consequences of Lieber's whittled-down approach to synchronic grammar).

1.1.2 *Pruning back the view that languages change like living organisms*

However, in contrast to works like Pedersen's (1924) book-length account of what was achieved mainly by Indo-Europeanists during the nineteenth century, or like much of James Anderson's (1991) encyclopedia-article overview of linguistic diachrony, the present volume is most assuredly not a history of historical linguistics – and it is especially not a history of general linguistics.[6] As a result, the various contributors to this book (apart from this introduction) make virtually no mention of certain positions concerning the nature of language and language change which were once quite common but have now been largely discredited, though not completely abandoned. Perhaps the most prominent such position involves approaches which find it productive to treat languages as organisms.

In the view of Bopp (1827, here quoted from 1836: 1), for example, languages must be seen "as organic natural bodies that form themselves according to definite laws, develop, carrying in themselves an internal life-principle, and gradually die off" (translation after Morpurgo Davies 1987: 84; see also the discussion and references there – plus, more generally, Morpurgo Davies 1998: 83–97 et passim).[7] In this, Bopp followed the treatment of Sanskrit and other things Indic by Friedrich von Schlegel (1808/1977), whose own positive use of "organic" (German *organisch*) – roughly meaning "innately integrated but able to develop" (as opposed to "adventitious and merely 'mechanical' [*mechanisch*; cf. pp. 51–52]") – was due less to his admiration (from afar) for comparative anatomy than it was to his familiarity with German Romantics (see Timpanaro 1972) like Herder (cf., e.g., 1877–1913: vol. 1, 150–2) and the natural philosopher von Schelling (1798, 1800). Going even further, August Schleicher (1873: 6–7) advocated treating linguistics as literally a branch of biology parallel to botany and zoology (for discussion, see Koerner 1978a, 1989; Tort 1980; Wells 1987; Collinge 1994a; Desmet 1996: 48–81 et passim; Morpurgo Davies 1998: 196–201 et passim; and their references on Schleicher):

> Languages are natural organisms which, without being determinable by human will, came into being, grew and developed according to definite laws, and now, in turn, age and die off; they, too, characteristically possess that series of manifestations which tends to be understood under the rubric "life". Glottics, the science of language, is therefore a natural science; in total and in general, its method is the same as that of the other natural sciences.

Yet one immediately wonders how such pioneering figures of historical linguistics could overlook the ineluctable fact that, as already pointed out by Gaston Paris (1868) in an early critique (p. 242):

> [a]ll of these words (*organism, be born, grow . . . , age,* and *die*) are applicable only to individual animal life . . . [. E]ven if it is legitimate to employ metaphors of this sort in linguistics, it is necessary to guard against being duped by them. The development of language does not have its causes in language itself, but rather in the physiological and psychological generalizations of human nature. . . . Anyone who fails to keep in mind this fundamental distinction falls into obvious confusions.

De Saussure (1916: 17, here quoted from 1983: 3–4) reacted to the organicism of Bopp and Schleicher in a rather similar vein: "[T]he right conclusion was all the more likely to elude the[se] . . . comparativists because they looked upon the development of languages much as a naturalist might look upon the growth of two plants." But Bonfante (1946: 295) expressed matters even more trenchantly: "Languages are historical creations, not vegetables."

While we are here constrained to extreme brevity (but see the above references), present-day diachronicians can draw from the organicism of many nineteenth-century linguists an important moral regarding cross-disciplinary analogies (and envy). It is certainly the case that, during K. W. F. von Schlegel's and Bopp's studies in Paris (starting respectively in 1802 and 1812) and during the period of their early writings on language (respectively *c.*1808ff and 1816ff), such natural sciences as biology, paleontology, and geology were quite well established and abounded with lawlike generalizations, whereas such social sciences as psychology and sociology either had not yet been founded or were still in their infancy. Von Schlegel's and Bopp's formative experiences at this time were thus set against a general backdrop which included the wide renown and respect accorded to, for example, Cuvier's *principe de corrélation des formes* (formulated in 1800 and usually translated as "principle of the correlation of parts"; cf., e.g., Rudwick 1972: 104, and 1997: passim), which stressed the interdependence of all parts of an organism and thus functioned so as both to guide and to constrain reconstructions of prehistoric creatures. Hence it is not surprising that, lacking recourse to any comparably scientific theory of brain, mind, personality, community, or the like, such linguists as von Schlegel, Bopp, and later Schleicher were irresistibly tempted to adopt an organismal (or organismic) approach when they found lawlike correspondences across languages (or across stages of one language) and began to engage in historical reconstruction.[8]

This trend can be seen as following from a variation on a corollary of Stent's (1978: 96–7) assertion that a scientific discovery will be premature in effect unless it is "appreciated in its day." In this context, for something to lack appreciation does not mean that it was "unnoticed . . . or even . . . not considered important," but instead that scientists "did not seem to be able to do much with it or build on it," so that the discovery "had virtually no effect on the general discourse" of its discipline, since its implications could not "be connected by a series of simple logical steps to canonical . . . knowledge." (It was in this sense, e.g., that Collingwood (1946/1993: 71) described Vico's 1725 *Nuova scienza* (*"New Science"*) as being "too far ahead of his time to have very much immediate influence.") In the case at hand, the relevant corollary is that scholars tend to interpret and publicize their discoveries in ways which allow connections with the general discourse and canonical knowledge of their discipline. More particularly, however, scholars in a very new field – one where canons of discourse and knowledge still have not solidified or perhaps even arisen yet – are tempted to adopt the discourse and canons of more established disciplines, and it is this step that nineteenth-century organicist diachronicians of language like von Schlegel, Bopp, and Schleicher seem to have taken. Seen in this light, their actions appear understandable and even reasonable.

What remains rather astonishing, though, is the fact that, even after the (more) scientific grounding of psychology and sociology later in the nineteenth century, a surprising number of linguists maintained an organicist approach to language. As documented in painstaking detail by Desmet (1996), a "naturalist linguistics" was pursued in France during the period from approximately 1867 to 1922 by a substantial body of scholars associated with the École d'anthropologie and the Société d'anthropologie de Paris, publishing especially in the *Bulletins* and *Mémoires* of the latter, in the *Revue d'anthropologie* or *L'homme*, and in the *Revue de linguistique et de philologie comparée* (*RdLPC*), a journal which they founded and dominated. Thus, at the same time as the Société de linguistique de Paris continued to enforce its ban on discussions concerning the origin(s) of language(s), a cornucopia of lectures, articles, and even books on issues connected with the birth and death of language(s) as viewed from an organicist perspective (along with issues related to language vis-à-vis race) flowed from the pens of such now little-known scholars as Chavée, Hovelacque, de la Calle, Zaborowski, Girard de Rialle, Lefèvre, Regnaud, Adam, and Vinson (the last of whom had 237 publications in the *RdLPC* alone; cf. Desmet 1996).

Still, while this movement itself died out in France *c.*1922 (aging and weakening along with its major proponents),[9] one can still document occasional instances of explicitly organicist attitudes toward language and language change within the scholarly literature of the last decade of the twentieth century and on into the first decade of the twenty-first. Yet this is an era when the increasing solidity and number of accepted cognitive- and social-psychological principles leave no room for a Bopp-like appeal to biology as the only available locus for formulating lawlike generalizations concerning linguistic structure, variation, and change. Still, for example, Mufwene (1996) has suggested that, in pidgin and creole studies, there are advantages to viewing the biological equivalent of a language as being not an individual organism, but an entire species – which, expanding on Bonfante's (1946) above-mentioned aphorism, we may interpret as implying that, rather than being a vegetable, each language is an agglomeration of vegetable patches!

More provocative have been various organicist-sounding works by Lass, beginning especially with his earlier (1987: 155) abandonment of the "psychologistic/individualist position . . . that change is explicable . . . in terms of . . . individual grammars." Instead, Lass (1987: 156–7) claims that "languages . . . are objects whose primary mode of existence is in time . . . [– h]istorical products . . . which ought to be viewed as potentially having extended (trans-individual, trans-generational) 'lives of their own'." More recently, Lass (1997: 376–7) has reiterated and expanded this glottozoic claim, suggesting that we "construe language as . . . a kind of object . . . which exists (for the historian's purposes) neither in any individual (as such) . . . nor in the collectivity, but rather as an area in an abstract, vastly complex, multi-dimensional phase-space . . . [a]nd having (in all modules and at all structural levels) something like the three kinds of viral nucleotide sequences."

This sort of approach has already been compellingly and eloquently countered by Milroy's (1999: 188) response to Lass's (1997: 309 et passim) characterization of languages as making use of the detritus from older systems via "bricolage," whereby bits and pieces left lying around get recycled into new things. After first asking how we can "make sense of all this without . . . an appeal to speakers," Milroy further queries: "If there is bricolage, who is the bricoleur? Does the language do the bricolage independently of those who use it? If so, how?" Our own answer to Milroy's rhetorical questions echoes former Confederate General George Pickett's late-nineteenth-century riposte – "I think the Union Army had something to do with it" (cf. Reardon 1997a: 122, 237n.2, 1997b; Pickett 1908: 569) – to incessant inquiries concerning who or what had been responsible for the negative outcome of "Pickett's Charge" at the battle of Gettysburg (July 1–3, 1863) during the American Civil War.[10] That is, unlike

Lass (1980: 64ff, 1981: 268ff, 1997: passim), who comes perilously close (cf. especially p. xviii) to suggesting that – as Dressler (1985b: 271) critically puts it – "[i]t is not . . . individual speakers who change grammar, but grammar changes itself," our view on the identity of the parties most reponsible for linguistic change is, rather: we think speakers have something to do with it (see Joseph 1992; Janda 1994a).

And this conclusion leads us to the above-mentioned moral for students of language change which, to repeat, is provided by the history of linguistics, even though considerations of space dictate the virtually total further exclusion from this volume of that topic. Namely, given that human speakers (and signers) are the only known organisms which/who come into question as plausible agents of change in languages, it is incumbent on historical linguists to avoid the trap of reacting to their potential disillusionment with current research findings in psychology and sociology by giving up entirely on psychology and sociology – and, along with them, on speakers – and so turning too wholeheartedly to the "better understood" field of biology. It is the latter move, after all, which has lured scholars like Lass (1997) into treating languages as organisms, or at least pseudo-organisms. Learning a lesson from what can now be recognized as needless wrong turns in the work of K. W. F. von Schlegel, Bopp, Schleicher, and later *linguistes naturalistes*, we can conclude that it is better for diachronic linguistics if we stand for an embarrassingly long time with our hands stretched out to psychology and sociology than it is for us to embrace the siren of biological organicism.[11]

It is thus no accident that the present volume apportions either entire chapters, or at least substantial portions of them, to various aspects of psycholinguistics (including language acquisition and the psychophysics of speech perception) – see the respective chapters by JOHN OHALA (22) and JEAN AITCHISON (25) – and to central topics in sociolinguistics (like social stratification, attitudes or evaluations, and contact) – as in the respective chapters by GREGORY R. GUY (8), SARAH GREY THOMASON (23), and WALTER WOLFRAM AND NATALIE SCHILLING-ESTES (24).

1.2 On change – both linguistic and otherwise

All things move, and nothing remains still . . . ; you cannot step twice into the same stream.

Heraclitus (*c.*540 BC – *c.*480 BC[12]), quoted by "Socrates" in Plato's *Cratylus* (*c.*385 BC: 402A, trans. Harold N. Fowler (1926: 66–7))

Plus ça change, plus c'est la même chose.
"The more that changes, the more it's the same thing" (often less literally as *"The more things change, the more they stay the same"* or *"The more things change, the less things change"*).

Alphonse Karr, *Les Guêpes* ("The Wasps") (January, 1849), reprinted (1891: 305) in vol. 6 of the collected series

As the title of this introductory essay indicates, we believe that it is crucial for historical linguists to devote some attention to working toward an understanding of change overall, and thus to wrestling conceptually with the time dimension that accompanies all activity in this world.[13] We therefore begin with some general thoughts about time and change, as well as the epistemology and methodology of historical research.

1.2.1 Lesser and greater ravages of time

Only this . . . is denied even to God . . . [:] / the power to make [undone] what has been done.

Agathon (*c.*400 BC), quoted in Aristotle's *Nicomachean Ethics*, VI. 2.6 (p. 1139b, l. 10) (*c.*330 BC), trans. H. Harris Rackham (1934: 330–1)[14]

As the sun's year rolls around again and again, the ring on the finger becomes thin beneath by wearing; the fall of dripping water hollows the stone; the bent iron ploughshare secretly grows smaller in the fields, and we see the paved stone streets worn away by the feet of the multitude. . . . All these things, then, we see grow less, since they are rubbed away.

(Titus) Lucretius Carus, *Dē rērum nātūrā librī sex* (*"Six Books on the Nature of Things"*), I. 311–19 (*c.*60 BC), transl. after Cyril Bailey (1947: I, 190–3, II, 643–50)

Imagine that you are a geologist and that you want to study an event[15] such as the ongoing erosion – by wind and water – of an exposed sandstone hillside (recently denuded of its grass cover by fire) over the course of several decades. How should you go about this? More particularly, consider which option you would select if you were forced to choose between two polar-opposite possibilities. On the one hand, you are offered the opportunity to obtain a relatively continuous filmed record of the hillside and the forces affecting it, in the form either of a real-time videotape or of time-lapse photography (advancing at a rate of, say, one frame per minute). Alternatively, you will be limited to only two snapshots of the hillside, one taken at the beginning and one taken at the end of the relevant decades-long period – that is, when the originally smooth and sloping surface was first exposed to the elements, and then again after it had been worn down to corrugated flatness.

Few indeed, we venture to say, are those who would willingly choose the essentially static, before-versus-after view afforded by the latter alternative, with just two stages documented – given that, after all, it is so much less informative and revealing, that it omits the details showing the course of change, and that it leaves the mechanisms of the transition between initial stage and final stage to be reconstructed inferentially. The point here is not that such reconstructions are impossible to carry out. Indeed, if they are all that is available to a scholar, then she or he will tend to be content with them and to do with them what she or he can. Still, if options with more detailed information are available, such as time-lapse photography (e.g., with 60 frames per hour) or even continuous videotaping (which later can be either excerpted or else viewed at high speed), then these will of course tend to be preferred. The second, interstitial-reconstruction alternative simply provides less of the information that is relevant for understanding the transition between two states whose spatiotemporal connectedness is beyond dispute even though they lie far apart chronologically.

Yet, before we turn from our brief encounter with research on geological change back to a focus on investigations of alterations in language(s), it is worth emphasizing that the relevant moral lesson provided by geology for historical linguists goes far beyond the fact that geologists indeed view diachronic data which fill in the gaps between the beginning and the endpoint of a change as being highly desirable in principle. Rather, in cases like ongoing studies of the behavior of Mount Etna, it is clear that geologists regularly take the practical step of putting their money where their mouth – of a volcano – is. As recently as 2001, newspapers were reporting that the Sicilian peak was producing spectacular lava flows moving up to 100 meters an hour – and this information comes largely from the "huge array of monitoring techniques" recently discussed by Rymer et al. (1998): for example, measurements of seismicity, ground deformation, and microgravity, or results derived from electromagnetic, magnetic, and gas geochemistry, and the use of remote sensing. The authors conclude (p. 335) that a full understanding of Etna's volcanism over time will require "the more comprehensive acquisition and real-time analysis of continuous data sets over extended periods."

Furthermore, the above-mentioned time-lapse photography of flowers, plants, and trees, which is so familiar to (present and former) schoolchildren from nature films, sometimes turns out to be a crucial tool in the discovery of botanical secrets. Milius (2000: 413), for instance, describes the 26-year-old mystery of a New Zealand mistletoe whose "hot-pink buds . . . open upside down . . . [,] stay[ing] connected at their tips but split[ting] apart . . . at the stem end" – the agency of particular birds (and bees) in twisting open these buds from the top became clear only through the use of "surveillance videos." In short, actual research practice in the natural sciences makes it abundantly clear that scholars of virtually all disciplines have much to gain from studying the intermediate stages of changes, not just their before and after.

In historical linguistics, a revealing pair of terms has been adopted by a number of scholars in order to do justice to this crucial difference between (i) the juxtaposition of two temporally distinct states, regardless of the number of events intervening between them, and (ii) the transitional course of one event as it happened. As the most constant advocate of this distinction, Andersen (1989: 12–13) has stated:

> [L]inguists have tended to take little interest in the actual diachronic developments in which a language tradition is preserved and renewed as it is passed on from speaker to speaker – which should be the historical linguist's primary object of inquiry. Instead . . . [,] they have focused . . . on *diachronic correspondences*, calling these metalingual relations "changes" . . . and speaking of them as of objects changing into other objects, bizarre as it may seem. . . . In other words, the word "change" has commonly been employed . . . not to describe anything going on in the object of inquiry – language in diachrony – but rather to sum up a reified version of the linguist's observations. . . . In order to describe effectively the reality of diachronic developments, . . . the term *"innovation"* [can be used] to refer to any element of usage (or grammar) which differs from previous usage (or grammars). The notion of innovation makes it possible to break down any diachronic development ("change") into its smallest appreciable constituent steps. [emphasis added]

In addition, however, some socio- and historical linguists (of varying persuasions) who employ the above notions find it useful to make a further distinction between an *innovation* – as the act of an individual speaker, regardless of whether or not it later catches on in a speech community – and a *change*, strictly defined as an innovation that has been widely adopted by members of such a community. Milroy (1992: 219–26), refining earlier discussion in Milroy and Milroy (1985), distinguishes between *speaker innovation* and *linguistic change*, while Shapiro (1991: 11–13, 1995: 105n.1), imposing a specific interpretation on the more general definition in Andersen (1989: 11–13), similarly reserves the term *change* "for an innovation that has ceased to be an individual trait and . . . [so has] become a social fact" (1995: 105n.1).

It is worth emphasizing that more than terminology is at stake here, because differing interpretations of the word *change* have sometimes led historical linguists to talk past one another. On the one hand, many works on grammaticalization surveyed here by BERND HEINE (chapter 18) focus on the beginning and endpoints of developments which stretch over so many centuries that their authors are virtually compelled to neglect numerous (sometimes even all) intermediate stages and hence to treat myriad static diachronic correspondences – in a rather direct manner – as outright changes.[16] Many formalist treatments of diachronic syntax discussed by LIGHTFOOT (chapter 14), on the other hand, limit their accounts of language change primarily to an individual speaker's innovations (especially those of a child). Yet the collective view of the variationist works discussed by GUY (chapter 8) is that expressed by Labov (1994: 310–11), who speaks of "change in language . . . [only] when other speakers adopt . . . [a] new feature . . . [, so that] the change and . . . [its] first diffusion . . . occur at the same time." There is thus much to be said for recognizing the above-mentioned three-way distinction: namely, *diachronic correspondence* (juxtaposing two potentially non-adjacent times) versus *innovation* (initiated by an individual person at one particular time) versus *change* (requiring adoption, over time, by all – or at least much – of a group).[17]

Applying these distinctions to our above geological example, we can say that studying a diachronic correspondence like the relation between the starting-point and the endpoint of a hillside's erosion could rarely, if ever, provide as much insight into that long-term phenomenon as detailed research on the actual series of innovations which make up the overall change-process of erosion itself.

However, in doing historical linguistics, we are generally closer to being in the position of a geologist who has only two before-versus-after snapshots – or, perhaps more fittingly, only a pair of hand-drawn sketches based on two such photographs. Nearly all historians, in fact, confront (to varying degrees) this kind of yawning chasm amidst fragments of documentary evidence,[18] a predicament which led the American scholar Charles Beard to say that, in doing history, "We hold a damn dim candle over a damn dark abyss" (cf. Smith 1989: 1247). In our own field, too, Labov (1994: 11) has noted that "[h]istorical linguistics can . . . be thought of as the art of making the best use of bad data," though we would prefer to characterize the data in question as "imperfect." That is, until recently, the devices available for making and storing historical records have been such as virtually to guarantee that the information preserved will of necessity be fragmentary or otherwise incomplete, and so possibly misleading, etc. – whereas "bad" implies mistaken, faulty, or false.[19] Still, Labov's point is well taken, and there sometimes are bona fide, or rather mala fide, hoaxes (e.g., this seems to apply to the so-called Praenestine fibula; see Gordon 1975; Guarducci 1984), where the bad data are of an evil sort. Indeed, as both MARK HALE and SUSAN PINTZUK stress in their chapters (7 and 15, respectively), there are many cases where the only way to study a change involves consulting fragments of documentary evidence such as texts, recordings, and the like[20] (and see sections 1.2.3.4 and 1.2.3.5 on "imperfections" in paleontological data).

Nor should we forget the fact that the overwhelmingly preponderant direction of spread for linguistic changes is generally believed to flow from colloquial speech to more formal speech and thence to documentary writing, despite occasional instances of the reverse. (As for the latter, there are, e.g., spelling pronunciations like *of*[t]*en* and sporadically attested backformations like *misle* 'to mislead,' variously rhyming with *fizzle* or *(re)prisal*, based on a reinterpretation of (visually presented) simple past or past participial *misled* as *mísle-(e)d* rather than *mis-léd*.) Consequently, most research on language changes which date back before the era of sound recordings is actually focused on the penetration into writing of already-occurred changes, rather than on their ultimate origin in spoken language.[21] And, even then, the texts (in the general sense) which are at issue are all subject to the vagaries of attestation, to the need for interpretation (e.g., of the relation between spelling and pronunciation, which is one focus of philology), and to problems regarding dating of composition, manuscript transmission, and scribal traditions, etc.[22] Caution is thus always in order – for several reasons, as can easily be shown by a few brief examples.

1.2.1.1 *Historical evidence is like the sea: constant but ever-changing*

For one thing, not all (forms or sentences found in) texts are of equal status, particularly where normalized editions or collections of excerpts are concerned. Instructive in this regard is a scholarly exchange – cf. Lightfoot (1979, 1980), Lieber (1979), and Russom (1982) – concerning the absence versus presence in Old English of so-called "indirect passives": sentences of the type *I was recently given a book about cats*, in which a logical (grammatical relational) indirect object surfaces as the subject of a passive verb. Lightfoot (1979) started off this debate by claiming that Old English had only a non-transformational (lexical) passive, and thus that the Modern English transformational (syntactic) passive represents an innovation, basing this assertion on the apparent absence from pre-Modern English of indirect passives (which he viewed as necessarily non-lexical and hence syntactic).[23] Lieber (1979) then countered this claim by adducing four apparent instances of indirect passives from the Old English period. Russom (1982) settled the matter, however, by showing that these four examples all evaporate when subjected to closer examination. One case, for example, involves the passive of a verb that did not normally govern a surface indirect object (but instead two accusative objects), while two cases are actually alternative versions of the same example – cited elliptically in two different ways in Lieber's source – which clearly involves (in its fullest form) an underlying animate direct object realized as a passive subject (or theme) on the surface, as in *The slave was given (to) the master*. The fourth and final case likewise shows an animate passive subject as theme, but it significantly also contains a true (underlying and superficial) indirect object that is inflectionally marked as such (by *-e*) via a conventional scribal sign (a macron over the final consonant) that is visible in the best editions of the text but missing from many secondary sources that cite the example, including the only one consulted by Lieber. Here, Russom's careful assessment of the evidence from a philological standpoint (one taking original text, scribal practices, and overall context into account) proved crucial to an accurate assessment of the linguistic claim being made – and not only with respect to the synchronic status of an Old English construction, but also regarding an alleged change (versus the actual lack thereof) in the diachrony of English passives.

1.2.1.2 *Accidental gaps in the historical record*

Moreover, despite all the philological care in the world, even something as seemingly fixed as date of first attestation is not always a reliable indication of age. For instance, the word *éor* is attested very late in the Ancient Greek tradition, occurring only in glosses from the fifth century AD attributed to the lexicographer Hesychius, but it clearly must be an "old" word, inherited from Proto-Indo-European, since it seems to refer to female kin of some sort and thus appears to be the Greek continuation of PIE *swés(o)r 'sister,' altered by the action of perfectly regular sound changes.[24] The complete absence of this word from the substantial documentary record of Greek prior to the fifth century AD, which covers thousands and thousands of pages of text, is thus simply an accidental gap in attestation. Further, oral transmission clearly can preserve archaic forms, as the evidence of the *Rig Veda* in Sanskrit shows, even though there is no (easy) way to assign a "first attestation" to an orally transmitted text.[25]

1.2.1.3 *Delays in attestation – for example, of taboo words*

A similar issue arises with lexical items that have special affective or emotive value, such as the subset of taboo forms often called "curse words" – that is, expletives (fillers) of a particular sort. To take a comparatively mild example, the earliest citations in the *Oxford English Dictionary* (s.v.) for the English noun *shit*, attested since *c*.1000, reflect a purely referential use, with the relevant sense being 'diarrh(o)ea, especially in cattle.' The usage of this form as a "contemptuous epithet applied to a person" is documented only since 1508, while its extremely frequent contemporary (modern) use as an expletive (with the euphemistically deformed variant *Shoot!*) is not recorded in the *OED* at all. However, the word in question has clear cognate forms within Germanic (e.g., *Scheiss(e)* in German), and it arguably derives from an Indo-European prototype, given the formal and semantic parallels in related languages (e.g., Hittite *sakkar*, Greek *skô:r* 'dung'). Moreover, there appears to be a panchronic and thoroughly human proclivity to employ lexical items with such meanings for affective purposes.[26] We therefore contend that the burden of proof ought to be on anyone who claims that its expletive use is only a recent phase in the more than 5,000-year history of the word at issue in this paragraph.[27]

1.2.1.4 *High-prestige data can come from once low-prestige sources*

Furthermore, even when some specific set of documents – or, with luck, an entire textual genre – characteristic of a particular linguistic period happens to be preserved in nearly or (*mirabile dictu*) completely pristine form,[28] we do well to remind ourselves of the apparently ubiquitous bias favoring the creation and preservation of religious, legal, commercial, and literary texts over written representations of informal speech. Now, it is in the very nature of holy scriptures, stabilizing laws, binding contracts, and monumental epics to promote the iconic equating of fixation in writing with fixity of language, and of intended invariance over time with imposed linguistic invariance.

As Rulon Wells (1973: 425–6) once eloquently put it:

> [T]here was never a time in biology when the study of fossils was more highly esteemed than the study of living plants and animals . . . [, whereas] it was only after centuries of debate that the study of living languages and literatures (written or oral) came to be considered not inferior to the study of Latin and Greek. And the debate was, in effect, ended sooner for literature than for language: the "progressive" view prevailed, very broadly speaking . . . [,] for literature already in the Enlightenment, but for language not until romanticism . . . In biology, per contra, it was generally recognized that if, e.g., one classified fossil molluscs exclusively according to properties of their shells, this basis of classification, used for lack of anything else, was forced upon us by the circumstance . . . that only their hard shells, and not their soft inner vital parts, . . . [were] preserved . . . [. But, eventually, t]his view [was] attained in the nineteenth century . . . [:] that we lacked information about such vital parts of the classical languages as their . . . intonation, the details of their pronunciation, and the full extent of differences of dialect, social class, and style within them.

In the twentieth century, on the other hand, it was well into the 1960s and even the 1970s before William D. Labov's findings concerning the greater consistency and even systematicity of informal speech-styles firmly impressed themselves on the minds of linguists. We have in mind such quantitative results as those of Labov (1989a: 13–14, 17–18) concerning speakers of Philadelphia English. Even though the spontaneous speech of a representative sample of these speakers was characterized by 99–100 percent consistency (with 250 clear tokens versus 1 ambiguous case) in realizing the lexical – that is, phonemic – contrast between low, lax /æ/ in *sad* versus raised, centralized /æh/ (phonetically [e^{ə}]) in *bad*, *glad*, and *mad*), there was only 73–7 percent consistency (depending on the evaluation of difficult-to-interpret tokens) in the realization of this pattern within the more formal style involved in reading word-lists aloud. And even elicitation-style (i.e., focused interrogation of the sort that asks questions like "What do you do/say when such-and-such happens?") was only 90–6 percent consistent for /æ/ versus /æh/. Simultaneously, that is, writing tends to favor both conservatism and hypercorrection.

In short, there is little we can do to change the circumstance that the texts which most often tend to be written and preserved are those which least reflect everyday speech.[29] But we can at least admit our awareness of this situation, and concede that it obliges us to use extreme caution in generalizing from formal documents. After all, in the words of Bailey et al. (1989: 299): "[T]he history of . . . language is the history of vernaculars rather than standard languages. Present-day vernaculars evolved from earlier ones that differed remarkably from present-day textbook[-varieties] . . . These earlier vernaculars, rather than the standard, clearly must be . . . the focus of research into the history of . . . [languages]." In fact, this view had already been just as forcefully expressed at the beginning of the twentieth century by Gauchat (1905: 176), who referred to "spoken dialects" as "living representatives" which can provide evidence regarding "the phases which the literary languages have passed through in the course of time . . . [; t]he vernaculars . . . can serve as our guides in helping us to reach a better understanding of academic [varieties of] languages."[30]

1.2.1.5 *The first shall be trash, and the trash shall be first*

To this pithy encapsulation of the diachronic linguistic facts, we would only add that modern-day archeology and paleontology are replete with suggestive parallels likewise involving the subsequent historiographical valorization of phenomena whose worthlessness or even repulsiveness could only seem obvious both to cohorts in the past (human or otherwise) and to laypeople in the present. To take a specific and extreme example: probably the most revealing and reliable information regarding the diet and activities of the prehistoric Egyptians living at Wadi Kubbaniya (near modern Aswan) *c*.18,000 years ago comes from the analysis of "charred infant feces, so identified by their size . . . , [which had been] swept into . . . [camp]fire[s]" (cf. the summary in Fagan 1995: 92–3, 264, plus the fuller account in Hillman 1989). Similarly, the controversial question of whether members of the dinosaur family *Tyrannosauridae* (now extinct for tens of millions of years) were principally predators or scavengers is now beginning to be resolved on the basis of *Tyrannosaurus rex* coprolites (see, e.g., Chin et al. 1998). This is because "histological examination of bone in coprolites can give the approximate stage of life of the consumed animal" and thus show whether *Tyrannosauri reges* tended to prey on the youngest and oldest (hence most vulnerable) members of herds or instead to scavenge on carrion of all ages, gregarious or not (cf. the more accessible discussion in Erickson 1999: 49).[31] In short, as Rathje (1978: 374) has put it so well (in the context of justifying studies of present-day waste products along with ancient ones; cf. also Rathje 1974): "All archeologists study garbage; the Garbage Project's raw data are just a little fresher than most." Similarly, Rathje (1977: 37) draws special attention to a dictum of "[a]rcheology pioneer Emil Haury . . . [:] 'If you want to know what is really going on in a community, look at its garbage.' "[32]

Among the situations in historical linguistics to which findings like the above are strikingly similar, we here mention three. First, there is the fact that the most revealing evidence concerning the history of Romance languages comes not from Classical Latin texts, but from Vulgar Latin like that found in the graffiti of Pompeii (volcanically fixed in 79 AD) and from the later list of stigmatized forms excoriated in the so-called "Appendix of Probius" (late fourth century); cf., for example, Elcock and Green (1975: 35–8, 40–6). What some upstanding Pompeiians thought of the graffiti in question is revealed by a contemporary addendum (written in classical meter) which Elcock and Green render as "I wonder, o wall, that you have not fallen in ruins, / since you bear the noisome scrawl of so many writers." A second such case concerns the short non-literary Latin texts, mostly from *c.*100 AD, found on small pieces of wood (*c.*10 cm by 10 cm) that had been used for everyday records and messages at the Roman fort of Vindolanda (now near Chesterholm, Northumberland) in northern England; see the discussion and references in Grant (1990: 129–33, 234–5). Precisely because of their non-Classical spelling and grammar, these texts by humble soldiers and their families have recently been described as priceless – yet, shortly after they were written, many of the messages "were evidently deposited in a rubbish dump," while "others were found in drainage areas, suggesting that they had been flushed away" (p. 132).[33]

Our third and final example of this type shows particularly clearly how seemingly throwaway texts can provide crucial evidence regarding the dating of specific linguistic changes. This instance comes from Old High German (OHG) and concerns rough drafts (*Vorakte*) from the eighth to ninth centuries which happened to be preserved in the northeastern Swiss monastery of St Gall – even though (most of) the filed official documents (*Urkunden*) based on these drafts were also preserved and so might have been expected to allow the discarding of the latter. As documented in detail by Sonderegger (1961: 253, 267–8, 1970: 34–9), the fortuitously preserved rough versions of many OHG legal documents written in St Gall *c.*800 AD are several decades ahead of the officially filed final versions in consistently designating the primary umlaut (i.e., to short *e*) of OHG *a*. In an example pair from 778, for instance, the draft form *(H)isanherio* – a man's name – was changed to *Isanhario* for the final version, and a pairing from 815 similarly matches the name spelling *Uurmheri* in a draft with the rewritten final form *Wurmhari*. Due to the serendipitous preservation of the St Gall rough drafts, then, a more accurate initial-stage chronology for the much-discussed process of umlaut as it occurred in (Alemannic) OHG could be arrived at (cf. Janda 1998a) without that process meeting an otherwise certain fate of being assigned far too late a date. But we are rarely so lucky.

1.2.1.6 *Broken threads in the histories of languages*

In sum, then: no matter how carefully we deal with documentary evidence from the past, we will always be left with lacunae in coverage, with a record that remains imperfect and so confronts us with major chasms in our understanding that must somehow be bridged. And "chasm(s)" is sometimes a charitable characterization of the impediments that bedevil the pursuits of diachronic linguists. Surprisingly often, the discontinuities posed by apparent gaps are compounded many times over when it turns out that what we actually face is not an interruption of a single linguistic tradition, but the end of one line of language transmission and the beginning or recommencement of a related but distinct line. Precisely such a situation obtains in the case of English – one sufficiently well known to receive mention in a popularizing work like the imposing encyclopedia compiled by Crystal (1995: 29):

> Most of the Old English corpus is written in the Wessex dialect . . . because it was th[e speech of the West Saxon] . . . kingdom . . . [,] the leading political and cultural force at the end of the ninth century. However, it is one of the ironies of English linguistic history that modern Standard English is descended not from West Saxon but from Mercian, . . . the [ancestor of the Southeast Midland] dialect spoken . . . in . . . [and] around London when that city became powerful in the Middle Ages.

That is, it is more or less impossible to carry out a direct tracing of West Saxon linguistic trends from late Old English into early Middle English, since Wessex speech is so sparsely attested after the Norman Conquest, and it is simultaneously impossible to pursue the direct antecedents for the early Middle English form of Southeast Midland speech back into the late Old English period, due to the dearth of Mercian texts in that earlier era.[34] In terms of the eroding-hillside analogy used above in the beginning of section 1.2.1, not only do cases like the one just mentioned limit analysts to dealing with (drawings of) just two photographs; they also force scholars to work with before-and-after photographs of different (albeit similar and neighboring) hillsides. Let us mention just one more related hurdle: Lass (1994: 4n.2) mentions a curious paradox of temporal misalignment which Dieter Kastovsky (pers. comm.) had once pointed out to him – the fact that, even in the normalized and hence homogeneous-seeming treatments of Old English typically found in historical grammars, "the phonology usually referred to in the[se] handbooks is that of the ninth to tenth centuries, but the morphology and syntax is that of the tenth to the eleventh." As if it were not already bad enough that seeking historical explanations for linguistic phenomena sometimes seems like looking for the Loch Ness monster, the many discontinuities involved should make us wary that alleged images of the monster may actually show not only the front part of one creature and the tail of another, but even the head of one creature, the neck of another – and so on. Exorcising such multiple demons may be a holy endeavor, but endeavoring to study language change is unavoidably a holey exercise (though undeniably of wholly consuming interest to its practitioners). Kroeber (1935: 548) said it perhaps best of all: "More useful is the definition of a historian as one who 'knows how to fill the lacunae.'"[35]

1.2.1.7 *Historical linguistics versus presently imperfect records of the past*

There is little doubt, then, that one fundamental issue in historical linguistics concerns how best to deal with the inevitable gaps and discontinuities that exist in our knowledge of attested language varieties over time. This book as a collective whole is largely an attempt to answer this key question as it pertains to language and related cultural phenomena.

One (partial) reponse is that – to put matters bluntly – in order to deal with gaps, we speculate about the unknown (i.e., about intermediate stages) based on the known. While we typically use loftier language to characterize this activity, describing the enlightened guesses in our speculations with more neutral names like "sober hypotheses that can be empirically tested," the point remains the same. In this respect, one of the relatively established aspects of language that can be exploited for historical study is our knowledge of the present,[36] where we normally have access to far more data than could ever possibly become available for any previously attested stage (at least before the age of audio and video recording), no matter how voluminous an earlier corpus may be.

We focus on this application of the present to the past in the following section. Still, it is important to note first that some linguists have suggested that there can be too many data available for some stage of a language, and that such a situation can get in the way of a clear understanding of what is going on. Thus, for example, in the view of Klein (1999: 88–9): "L[ass (1997)] makes the important paradoxical point that, despite our interest in taking into account as much data as possible in applying the comparative method, too much data can sometimes be a hindrance in that it may muddle the picture by making it harder to know what forms to take as input to the method." Stronger statements than this are hard to find in print, but one of us was once told by a former historian colleague at the University of Chicago: "Study the present as history in progress? Don't do that, or you'll drown in the data!" As regards

current and future progress in increasingly skilled applications of the comparative method (see chapter 1 by RANKIN, chapter 2 by S. P. HARRISON, and HALE's chapter 7), we agree with the view that some careful sifting of available data is needed. But, with regard to the question of understanding how languages change, it is clearly the case that, the more enriched our view is of what holds for any given language state, the better and therefore the more enriched will be our view of the historical developments which led to that state or which emerged from that state (remember again the eroded hillside washed and blown away above, from section 1.2.1).

One angle on utilizing the present for the illumination of the past is linguistic typology, as emphasized nearly half a century ago by Roman Jakobson (1958: 528–9): "A conflict between the reconstructed state of a language and the general laws which typology reveals makes the reconstruction questionable . . . A realistic approach to a reconstructive technique is a retrospective road from state to state and a structural scrutiny of each of these states with respect to the typological evidence." In this way, knowledge gained from a survey of the various features that synchronically characterize the range of the notion "possible human language" can be used as a means to gain insights into possible synchronic stages in the past. For instance, suppose it turns out to be a valid (linguistic-universal) generalization, as Jakobson (1958: 528) also claimed, that "as a rule, languages possessing the pairs voiced–voiceless . . . [and] aspirate-nonaspirate . . . have also a phoneme /*h*/" – that is, that there are no languages with aspirated stops that do not also have [h].[37] Suppose, further, that one is faced with the task of accounting for the transition from a language state with [pʰ tʰ kʰ] and [h] to one with [f θ x] but no [h].[38] It would seem reasonable to posit an initial stage with [f θ x h], prior to the stage with [f θ x] but no [h], rather than positing (contrary to the above-mentioned alleged universal) first the loss of [h], with the subsequent survival for some period of the aspirated stops. We would in this way be using information gleaned from the present to guide hypotheses about putative language states in the past. Crucially, our hypotheses in such cases are only as valid as the strength and certainty of our typological information and putative language universals,[39] but the methodological practice of using typology as a heuristic and a guideline for hypotheses regarding the past is what it is instructive to draw attention to here.[40]

Typology (or at least typologists) can be said to come in two flavors, however. One approach views typological gaps as constituting an interim report suggesting but not demonstrating the systematic absence of some phenomenon (or, conversely, the presence of some negative constraint). On this view, any qualitatively unique linguistic element or structure newly proposed for some language(s) is viewed with suspicion – since it has the defect of lacking independent motivation – but it is not treated as *a priori* impossible. Another approach to typology, though, is tempted either to reject unique phenomena, almost out of hand (e.g., as being the result of observational or analytical error), or to reanalyze each of them as a marked variant of an existing (more robustly motivated) phenomenon. This latter perspective might make more sense if more of the world's thousands of languages and dialects had been thoroughly, cogently, accessibly described, but our present state of knowledge about current linguistic diversity around the globe is seriously incomplete. As a result, many typological slots cannot be regarded as anything more than provisionally unfilled – especially since, from time to time, apparently unique elements and structures turn out to be more common than was originally thought. Thus, for example, Ladefoged and Maddieson (1996: 18–19, plus references there) discuss sounds produced by "moving the tongue forward to contact the upper lip" – for example, the "series of linguo-labial segments . . . [found] in a group of [Austronesian] languages from the islands of Espíritu Santo and Malekula in Vanuatu" (cf. the sequence of photographs, Ladefoged and Maddieson 1996: 19, showing the production of such a sound in Vao), and they also mention similar sounds elsewhere in the world.

Given the surprising frequency of such discoveries, a less absolutist approach to language typology seems preferable, and we would wager to say that this perspective is indeed the predominant one in current synchronic typologizing. Nevertheless, in mentioning above that typology often plays a role in historical linguistic reconstruction, we have already implicitly indicated that typology has a diachronic dimension, as well. Intriguingly, though, many historical linguists have been quite absolutist in their invocations of typology – to the point where, for example, Watkins (1976: 306) could complain that the "typological syntax" of Lehmann (1974) and others had led to "a theory which elevate[d] . . . some of Greenberg's [(1966)] extremely interesting quasi-universals to the dubious status of an intellectual straitjacket . . . into which the facts of various Indo-European languages . . . [had to] be fitted, willy-nilly, rightly or wrongly."

As it turns out, projections of absolutist synchronic typology onto a diachronic axis are often discussed by historical linguists in connection with (or even as constituting) the so-called "uniformitarian principle" (or "hypothesis"). This notion has been variously defined, as can be seen by comparing the version given in Labov (1972a: 275) – "the forces operating to produce linguistic change today are of the same kind and order of magnitude as those which operated in the past" – with either of the two versions later provided in Hock (1991b: 630), the second of which states that "[t]he general processes and principles which can be noticed in observable history are applicable in all stages of language history." In devoting the next section entirely to the nexus of issues centering on uniformitarianism, we have been guided by two main considerations. On the one hand, this (sort of) principle continues to figure prominently in contemporary discussions of language change. On the other hand, the "principle" itself is also revealed by closer inspection not only to be entirely derivable from other (irreducible) principles but also to be bound up with a number of lingering controversies, for some of which it seems that at least one of the contending parties is not fully informed about the relevant opposing views – hence the second part of the following section title. For readers who have either just acquired or always felt an antipathy toward the (nine-syllable length of the) term *uniformitarianism*, we should immediately mention that our eventual

conclusion will be that the relevant concept is better expressed under an alternative rubric like "informational maximalism."

1.2.2 *Uniformitarianism(s) versus uninformèd tarryin' -isms*

> *All sequences of events based on human activity can be viewed as natural – that is, as causally determined developments in which every stage must be understood with reference to the combinations and tensions of the preceding stage. In this sense . . . [,] one does not need to distinguish between nature and history, since what we call "history", if seen purely as a course of events, takes its place as part of the natural interrelationships of world happenings and their causal order.*
>
> Georg Simmel, "Vom Wesen der Kultur," *Österreichische Rundschau* 15 (1908), reprint in Simmel (1957: 86); trans. Roberta Ash (1971: 227)

> *[T]hose who, maintaining the historicity of all things, would resolve all knowledge into historical knowledge . . . argu[e:] . . . Might not a . . . revolutionary extension sweep into the historian's net the entire world of nature? In other words, are not natural processes really historical processes, and is not the being of nature an historical being?*
>
> Robin George Collingwood, *The Idea of History* (1946), re-edited (1993: 210)

While one is admittedly not likely to run into the term *uniformitarianism* outside of historical linguistics and other disciplines which deal with change(s) over time, the central concept behind this apparent sesquipedalianism is actually quite hard to avoid and/or ignore. For example, if a diachronician of any sort tries to escape from his or her subject by planning a vacation visit among the miles of snowy-white gypsum dunes in White Sands National Monument near Alamogordo, New Mexico, he or she may pick up Houk and Collier's (1994) guide to the dunes and there read (on p. 18):

> Ancient sand dunes are the building blocks of many of the earth's sedimentary rocks . . . Geologists have studied these rocks all around the globe . . . [,] peer[ing] back into the past . . . But the best instrument for studying the past is a sound understanding of the processes operating in the present . . . White Sands . . . offer[s] . . . geologists a perfect opportunity to study sand in the process of being deposited.

In fact, even non-geographical attempts to escape the long reach of uniformitarianism are ultimately doomed to failure. That is, any historically minded scholar who enjoys hiding temporarily in detective novels as a form of escape literature probably will eventually read some of G. K. Chesterton's Father Brown stories – among which is "The Strange Crime of John Boulnois" (published first in 1914), whose title character writes on "Catastrophism" and so is a presumptive opponent of uniformitarianism. (Boulnois, an "Oxford man," has challenged "alleged weak points in Darwinian evolution" via his counter-proposals involving "a comparatively stationary universe visited occasionally by convulsions of change" – which anticipates our later discussion, in sections 1.2.3.4 and 1.2.3.5, of "punctuated equilibrium" – though that is not his crime; cf. Chesterton 1929: 292–304.) In short, if uniformitarianism gives the impression of being uniformly present in disciplines which possess a diachronic component, or even just some kind of historical relevance, that is probably an accurate impression.

Virtually all scholars engaged in historical pursuits agree that uniformitarianism, at a minimum, has something to do with the relevance of the present for the study of the past. Several factors provide the crucial support for this conclusion and hence justify using considerations connected with the present as a means to elucidate the past. One such factor is sheer practicality: that is, the present (i.e., non-relic-like elements of the present – ones which lack unmistakable traces of a different past existence) are normally more directly accessible than is the past (i.e., those aspects of a former past identifiable from traces carried over into the present), and so we are able to study the present in ways that are unavailable for the study of the past: by reinterviewing someone, for instance. A more logic-oriented factor, though – and certainly a more compelling one – has to do with what can be called *independent motivation*. That is, since present-day entities and processes, being investigable in great detail before our very eyes, can be established with relative certainty, they are also available to be exploited for the purpose of proposing descriptions and explanations for phenomena – linguistic or otherwise – which occurred before our lifetimes, or even before the time of the earliest records kept by humans.

Lurking behind the scenes here, as the foundational core of this discussion, is the principle of parsimony (a.k.a. economy), which – despite its frequent association with a particular Franciscan theologian and philosopher who lived *c.*1285–1349 (his identity is "revealed" below) – was actually first invoked by Aristotle (384–322 BC) in his *Posterior Analytics*, his *Physics*, and his *Of the Heavens* (each time in a slightly different phrasing) For example, in chapter 25 of book 1 from the first of these (written *c.*350 BC), Aristotle states (in our adaptation of a 1960 translation by Hugh Tredennick) that:

> it may be assumed, given the same conditions, that that form of demonstration is superior to the rest which depends on fewer postulates, hypotheses, or premises – for, supposing that all of the latter are equally well known, knowledge will be more quickly attained when there are fewer of them, and this result is to be preferred.

This methodological principle of Aristotle's was well known to the most prominent figures of medieval scholasticism. It was thus regularly quoted and discussed in works – written mainly in the period from *c.*1225 to *c.*1325 – by authors like Robert Grosseteste, (St) Bonaventure, (St) Thomas Aquinas,

Henry of Ghent, Duns Scotus, and Peter Aureol, who also favored certain paraphrases of their own, such as (here translated from the Latin) "It is useless to explain by several things what can be explained by one"; cf. Maurer (1978: 405). But the concept at issue is in fact not now typically referred to either in this or in Aristotle's phrasing. Instead, it is most often encountered in a formulation widely known from the philosophical and scientific literature as "Ockham's razor," a name that arose in the mid-seventeenth century because parsimony as an entity-shaving device had become closely associated with a late scholastic writer, English-born William of Ockham (the above-mentioned Franciscan theologian and philosopher), who invoked it with particular frequency.[41] Still, the precise phrasing of the principle which most linguists and other scholars associate with Ockham was not in fact ever used (literally) by him. Rather, it appears to be post-medieval and was first attested in the seventeenth century, later becoming famous when it was prominently mentioned by Leibniz: "Entities are not to be multiplied without necessity" – that is, "without independent motivation." The closest that Ockham ever came to writing this was in his statement(s) that "a plurality never is to be posited without necessity" (in the Latin form *"pluralitas numquam est ponenda sine necessitate"*; cf. again Maurer 1978: 405). At any rate, it can indeed be demonstrated that what has been called Ockham's razor in fact holds Aristotle's blade.

Now, in the case of language change, working backwards from a knowledge of the present is clearly (equivalent to) a way of "depending" on "fewer postulates" (since it does not rely on entities postulated for the past without any other motivation), and it also just as clearly does not needlessly multiply entities (within a particular account), since constructs that are needed independently for explaining the present are pressed into service as parts of an explanation for the past. The methodological step of working backwards from the present – advocated, for instance, by Labov (1972aff) (as already noted above) – is thus licensed by both Aristotle's and Ockham's versions of the parsimony principle.

Another key factor that must be summoned into play here, though, is the assumption that the laws of nature are the same at all times and in all places. This crucial assumption – though sometimes treated as in essence a principle, too – is really nothing more than the result of another application of Ockham's razor (with Aristotle's blade), and thus likewise follows from the principle of parsimony. In a paradoxical sense, however, this concept is often treated as axiomatic – for the reason that, without some such orienting concept as an underpinning for investigations of the past, there would be no principled way to establish meaningful comparisons between different time(period)s, since the "ground rules" (so to speak) would then be free to differ from era to era. Moreover, it then would presumably be very difficult to determine (whether anyone could know) what the temporal locus is of the point(s) in time where a transition from one set of natural laws to another distinct set occurs, since such a difference could set in even from one moment to the next. One surely cannot – for obvious reasons – legitimately propose generalization of the following type: at sea level, water now always boils at 100°C, because it has done so ever since the exact moment on the morning of May 13, 2,000,000,000 BC, when Mickey's little hand was on the nine and his big hand was on the twelve – though the relevant boiling temperature had earlier always been 200°C. (On the general subject of time, especially as it relates to language change, see section 1.3 below.)

Much more can and should be said about "uniformitarianism" in its various avatars – and not just because (as befits a principle that frequently comes up in the course of historical linguists' musings on language change) both the history of the term itself and the ways in which it came to be applied in studies of language change prove to be enlightening. Rather, there really are major points of dispute latent in the differing definitions and interpretations that have been offered for this concept, with significant consequences relating, for example, to what can and cannot be achieved by reconstruction. We address a number of these issues in the section that follows (though we will have to reserve more extensive discussion for some other, later occasion).

1.2.2.1 *"Multiple meanings of* uniformity *and Lyell's creative confusion"*

While scholars are sometimes tempted to inveigh against certain (in their opinion) perverse ways in which other people – including scholars – use particular terms, it is usually best if they try to resist this temptation. In rare cases, though, it seems that some such policing of terminology would actually have been well advised, since it would apparently have staved off a certain amount of confusion and spared a great deal of otherwise wasted time and effort. Such a yearning to manage scientific terminology is perhaps most justified in the case of labels whose morphological transparency suggests that they have equally obvious semantics – a situation which readily invites misinterpretation of technical usage, especially when forms are borrowed from another field. All these factors seem to have been at work in linguists' misappropriation of the geological (and biological) term "uniformitarianism," and so we devote most of this subsection to keeping the relevant strands apart – in doing which we follow the model from geology established by Gould (1987), and so take our title from that of the corresponding subsection (pp. 117–26) of his monograph.

A scholar encountering *uniformitarianism* for the first time would surely recognize the base stem *uniform-(ity)*, and so ask: "But uniformity of what?" – only to answer, perhaps in the next breath, "Why, uniformity of law, certainly!": that is, the above-mentioned parsimony-derived principle that natural laws are constant across space and time. Yet probably another consideration would soon come to mind, one involving the slightly extended (and likewise previously mentioned) parsimony-derived assumption that such uniformity of law allows one to view the present as a key to the past: any process now observable thereby becomes available to be invoked as part of a plausible explanation for past events – this principle is that of "uniformity of process through time." This and the previous interpretation are both aspects of uniformitarianism that make eminent sense; indeed, their validity has already been argued for above. Moreover, these notions are in keeping with two specific cases already discussed here previously. One of these concerned the assumption that, given what we know about variation in modern languages, there cannot really have been as little diversity in spoken Gothic as the relatively variation-free documentary record suggests (cf. n. 28); the other case involved the application to reconstruction of synchronically based observations concerning linguistic typology (recall section 1.2.1.7).

Things start to go wrong, though, when historical linguists and/or other diachronicians view principles like these as having been first introduced into the scientific arena by nineteenth-century British (and, later, American and German) geologists led especially by Sir Charles Lyell. Quite on the contrary: as we document below, numerous historians of geology and biology over the past forty years have emphasized that explicit appeals to such uniformity of law were already common practice among Lyell's geological and biological contemporaries and predecessors (a number of whom he did not portray in a positive light). Moreover, Lyell's own innovative uniformities – namely, uniformity of rate (a.k.a. uniformity of effect) and uniformity of state (a.k.a. uniformity of configuration) – have not held up well at all.

Lyell (1830–3: passim) claimed in particular that geological change is "slow, steady, and gradual" (and not cataclysmic or paroxysmal) – cf. Gould (1987: 120) – because such floods, earthquakes, and volcanic eruptions as do occur are strictly local catastrophes. While this turns out to be true most of the time, it is by no means true all of the time, and Lyell's insistence that "the earth has been fundamentally the same since its formation" (argued by Gould 1975/1977 to have been the type of uniformity "closest to Lyell's heart") was abandoned even by its author before the end of his life, essentially because it had been empirically falsified by the documented phenomena of complete extinction and speciational evolution which had been championed by his protégé, Charles Darwin.

That the original sense of "uniformitarianism" involved Lyell's uniformity of rate is clear from the context within which Whewell (1832: 126) coined this long term, since Whewell suggested that the question of "uniform . . . intensity . . . [would] probably for some time divide the geological world into two sects, . . . the *Uniformitarians* and the *Catastrophists*" (original emphasis). The crucial missing element here is that there really were two kinds of catastrophists: what can be called "scientific catastrophists," like Whewell and the French paleontologist Cuvier, and what can be called "religious catastrophists," like Buckland (1836). Lyell wrote as if he were refuting all catastrophists, but in fact he was refuting only religious catastrophism. Yet, within geology, religious catastrophism no longer needed refutation at the time of Lyell's writing; cf. Gould (1975/1977: 149):

> By 1830, no serious catastrophists believed that cataclysms had a supernatural cause or that the earth was [only] 6,000 years old. Yet . . . these notions were held by many laymen . . . and . . . some quasi-scientific theologians. A scientific geology required their defeat, [for which scientific] catastrophists . . . praised Lyell because he brought a geologic consensus so forcefully to the public.

In short, as pointed out by Gould's (1987: 118–19) extensive and eloquent study of Lyell as a "Historian of Time's Cycle" (expanding on the start already made in Gould 1965):

> Lyell united under the common rubric of uniformity two different kinds of claims – a set of methodological statements about proper scientific procedure, and a group of substantive beliefs about how the world really works. The methodological principles were universally acclaimed by scientists, and embraced warmly by all geologists; the substantive claims were controversial, and, in some cases, accepted by few other geologists . . . [. In short,] Lyell . . . pulled a fast one – perhaps the neatest trick of rhetoric, measured by subsequent success, in the entire history of science. He labelled . . . different meanings as "uniformity" and argued that since all working scientists must embrace the methodological principles, the substantive claims must be true as well.

But, in so doing, Lyell (1830–3) achieved more than just an ephemeral accomplishment, more than a temporary victory. Rather, his strategy worked so well that he earned himself a lasting place in the history of geology *on his own terms* – an extremely rare and truly stunning coup. Thus, as Gould (1975/1977: 142) goes on to emphasize:

> [m]ost geologists would tell you that their science represents the total triumph of Lyell's uniformity over unscientific catastrophism. Lyell . . . won the victory for his name [and term], but modern geology is really an even mixture of two scientific schools – . . . original . . . uniformitarianism and . . . scientific catastrophism. . . . We accept . . . [the] two uniformities [(of law and process)], but so did the catastrophists. Lyell's third uniformity [(of rate/effect)], appropriately derigidified, is his great substantive contribution; his fourth (and most important) uniformity [(that of state or configuration)] has been graciously forgotten.

With so many senses of "uniformitarianism" struggling with one another in the geological trenches, it is not really surprising that historical linguists should show a correspondingly high degree of variation in their understanding and use of the term in question. The great frequency with which one encounters the rate-oriented interpretation of the concept appears to show, on the one hand, how strong an influence was exercised by a concentrated set of publications by Labov during the decade 1971–81 and, on the other hand, exactly how little attention is sometimes paid by readers in certain fields to the titles of books.

As regards the former point, it is useful to juxtapose with each other the primary statements made about uniformitarianism in the first two publications of the series Labov (1972a, 1974/1978, 1981). Repeating from earlier the remarks of Labov (1972a: 275) in *Sociolinguistic Patterns*, we can note that the definition there speaks of a principle such that "the forces operating to produce linguistic change today are of the same kind and [the same] order of magnitude as those which operated in the past." This is quite similar to – but

also (in that it mentions magnitude) slightly stronger than – Labov's (1974/1978: 281) definition in "On the use of the present to explain the past." In the latter work, there is a statement to the effect that, in "apply[ing] principles derived from . . . sociolinguistic studies of change in progress . . . [to the study of language change in the past], we necessarily rely upon the uniformitarian principle – that . . . the forces which operated to produce the historical record are the same as those which can be seen operating today." And a similar statement is found in the equally influential Labov (1981) ("Resolving the Neogrammarian controversy").

Though noticeable attention was paid both to the definitions and to the discussions provided by Labov on the subject of uniformitarianism in the set of publications just mentioned, the most salient fact about general reactions to Christy's (1983) short (xiv + 139-page) book on roughly the same topic in its historical dimension was that much of his audience seems to have ignored the circumscribed focus stated explicitly in Christy's title. At least among diachronic (as well as synchronic) *linguists*, that is, there apparently have been many readers who have assumed that Christy's monograph on *Uniformitarianism in Linguistics* was – and still is – essentially a comprehensive treatment of uniformitarianism in every relevant field, including geology and biology. Yet Christy's (1983) study, a revision of his Princeton University Ph.D. dissertation from 1982, actually has (reflecting its origins) an extremely narrow scope. The two nearly exclusive foci of Christy (1983) are, namely: (i) the geology of the nineteenth century and bordering decades as the idiosyncratic uniformitarian Lyell, his contemporaries, and his later hagiographers saw it, and him(self), and (ii) the paths by which the general concept of uniformitarianism first found its way from geology and (to a lesser extent) also biology into linguistics and then became established in the latter field, especially among the Neogrammarians. Because of its temporally truncated, excessively personalized (Lyellian), and thus myopic view of geology (lacking even glancing mention of numerous relevant studies on uniformitarianism which were available before 1982), the quite brief monograph in question has had the unfortunate effect of allowing diachronicians of language in particular to deprive themselves of access to works presenting a much truer picture of a major concept in their own and neighboring fields.

Admittedly, the background issues here – which involve at least partly the union as well as definitely the entire intersection between and among linguistics, geology, and biology – are quite complicated: to stay abreast of developments in three fields both diachronically (in terms of prior and ongoing historiography) and synchronically (in terms of current theory and practice) is probably beyond the capacity of any one individual. Nor do we wish to downplay Christy's (1983) achievement in combing numerous mainly nineteenth-century sources in order to establish which specific scholarly and personal connections were most probably responsible for allowing uniformitarian ideas to percolate so rapidly from geology (and biology) into linguistics. Yet certain other comparisons are difficult to avoid. For example, Wells (1973: 424) – to whom Christy

(1983) refers – dissects several inconsistencies inherent in Lyell's remarks on uniformitarianism, reviews the related geological and other literature, and concludes that, in essence, "Lyell himself was not an out-and-out uniformitarian." Christy (1983), however, mentions three earlier works – by Hooykaas (1959, 1970) and Gould (1965) – which explicitly and cogently argue that Lyell's only novel uniformities were not methodological and solid, but theoretical and seriously flawed, and yet Christy fails to discuss these findings (also repeated in other roughly contemporary works), but instead touts Lyell's theoretical proposals (one of which Lyell ultimately abandoned) as being what sets him above and apart from his predecessors.

It is true that Christy (1983) gives a definition for uniformitarianism that is arguably more productive than those (quoted above) provided by Labov (1972a, 1974/1978), since Christy avoids any phrasing of an excessively, unnecessarily restrictive sort which would basically prohibit the positing of entities or processes for the past which are not observable today. Instead, for Christy (1983: ix), the principle in question has more to do with the fact that "knowledge of processes that operated in the past can be inferred by observing ongoing processes in the present." This is essentially the "independent motivation" variety of uniformitarianism discussed near the start of the previous section: what is observed in the present can be proposed for the past, but what is not observed in the present cannot simply be banished, *ipso facto*, from the realm of the possible for the past. Labov (1994), however, keeps pace with shifts of thought in geology (thus citing Gould 1980 on Bretz 1923; cf. also Baker and Nummedal 1978; Baker 1981), adopts this geological consensus which had come to the fore since his last (1972a) book, and therefore thoroughly revises his earlier views by redefining uniformitarianism in Christy's terms. For Labov (1994: 21), that is, the relevant principle states that proposals regarding the past are to be seen as independently motivated if they invoke processes known from the present. Yet, although Christy's (1992) paper was presented at a 1989 conference that not only followed Christy's (1983) book by six years but also was attended by some of the authors whose past and present research runs counter to his conclusions about the notions of uniformitarianism – and catastrophism – held in geology before, during, and after the time of Lyell, there is no mention in Christy (1992) of these scholars' insights, even as claims.

The essence of this situation can perhaps best be expressed by means of a geological/geographical metaphor, and so we contend that the upshot of the above considerations for diachronicians (and synchronicians) of language is roughly as follows. In brief, taking Christy's (1983) *Uniformitarianism in Linguistics* as one's main or even sole source of information on the nature of uniformitarianism in geology (especially pre- and post-Lyell, but even *apud* Lyell) would be like mistakenly believing that a suspension bridge which linked the two rims of the Grand Canyon would constitute the entire US state of Arizona. Arizona indeed bills itself as "The Grand Canyon State," and the Canyon itself is of such monumental depth and breadth that any bridge over it (we hasten to add that there is no such bridge at present, nor do we favor the building of one) would truly be a marvel of engineering. Yet, relative to the entirety of both the Kaibab and the Coconino Plateaus, which it separates, the Grand Canyon is not large; compared to the whole rest of Arizona, the Canyon is anything but grand. Just as obviously, then, one short monograph on how an idea was transmitted from those who promoted it in earlier nineteenth-century geology to those who perceived, received, and reconceived it in later nineteenth-century linguistics does not even sufficiently exhaust the relevance of nineteenth-century geology for linguistics (whether historical, historiographical, or otherwise), let alone pre- and post-nineteenth century geology, and nineteenth-century geology as it existed apart from propaganda and hagiography.

1.2.2.2 *On living with catastrophes – and toward informational maximalism*

In this regard, one striking note of geological continuity – or at least resonance – that has potentially great relevance for diachronic (as well as synchronic) linguistics is provided by the way in which the non-religious catastrophism which had prevailed before Lyell (1830–3), even though driven underground by the latter's gradualistic uniformitarianism, today has a contemporary parallel in modern "neo- (or: new) catastrophism." Because it refutes uniformity of rate (or effect), this trend has been particularly stressed (as already indicated above) by Labov (1994: 21–3), who refers to the above-mentioned Gould (1980) and Bretz (1923) precisely for their discussions of how the so-called channeled scablands of Eastern Washington were carved out by repeated instances of "a single flood of glacial meltwater" which had "violent effects" when "vast volumes of water [were] suddenly released." It is examples like this which have sounded the death knell for versions of uniformitarianism that refuse to countenance proposals involving processes which are posited for the past but which have never been observed in the present (or during recorded history). As we have previously mentioned in connection with a number of issues, this older viewpoint – with its "if we don't see it now, then it never happened before" perspective – is now generally seen by geologists as being excessively restrictive on theoretical as well as on empirical grounds; cf., for example, Baker (1998).[42]

As regards the empirical evidence in question, the proponents of the new catastrophism have so far collected a host of dramatic examples that have, by and large, been found convincing. (The catastrophes proposed in connection with certain extinctions, however, have been more controversial: cf., e.g., Alvarez et al. 1980, Raup 1986, and Alvarez 1997 on asteroids as the possible nemesis of dinosaurs.) We will here cite only two general types of what could be called "neo-examples of paleo-catastrophes," but all of the relevant cases are quite dramatic. The first such case involves comparing recorded versus unrecorded events in the behavior of volcanoes. On the one hand, some notable instances of volcanic activity have been witnessed and recorded – and thus can be considered to be part of a "present" that is available to anyone invoking strict uniformitarianism as a guide to the past. This was the case, for example, with the Mt St Helens eruption in Washington state during 1980, and with the Tambora and Krakatoa eruptions in Indonesia during 1815 and 1883, respectively. Yet, as stressed by, for example, Decker and Decker (1998: 514; see also *Encyclopedia Britannica Online* 1994–2000) in a recent discussion of "Volcanism" exemplified partly with reference to the western United States, it is clear that "civilizations have never been tested by a cataclysm on the scale of the eruption at Yellowstone about 2,000,000 years ago; that eruption involved nearly 3,000 cubic kilometres of explosively boiling magma." In short, the two observed eruptions in question ejected far less magma (from Krakatoa only some 18 cubic kilometres; from Tambora still just some 50–100 cubic kilometres) than did the prehistoric volcanic activity at issue – whereby it must of course be noted that the ancient eruption has been totally inferred from the geological record precisely because it was not witnessed.

Furthermore, according to the widely accepted "Big Bang" theory of the origin of the universe (cf., e.g., Weinberg 1977), certain events took place in the first few seconds or even picoseconds (billionths of a second) that have clearly not taken place in exactly that way at any time since, even though the unique events of this cataclysmic origin apparently do conform to natural laws as currently understood. Phenomena of this and the previous (volcanic) sort represent the kind of evidence which is now routinely adduced as showing the cogency of the neo-catastrophist conclusion that, in the concise but eloquent phrasing of Gould (1980: 201): "uniformity of law [across time and space] does not preclude natural catastrophes, particularly on the local scale . . . [;] some invariant laws operate to produce infrequent episodes of sudden, profound change."

Moreover, the intervals between recurrences of even non-catastrophic but lawful phenomena can be so extended that the recurrent events in question have not yet occurred before the eyes of modern-day scientists. Therefore, glibly saying that the "present is the key to the past" does not excuse us from defining precisely what we mean by "present." Clearly, not all phenomena occur at all times (just as they do not occur in all places – and certainly not simultaneously in all places!). Rather, in stating that the present is the key to the past, we intend "the present" to signify "the period during which scientifically accurate and explicit records have been kept." Still, once we concede that this is what we mean, we thereby also admit that the relevant period is of comparatively brief duration – regardless of whether it is thought to have started during the lifetime of the Renaissance physician (and alchemist) Paracelsus (1493–1541) or of the Sanskrit grammarian Pāṇini (*c.*500 BC) or even of some Paleolithic painter drawing animal shapes on a cave wall (*c.* 14,000 BC) near what is now Altamira, Spain. That is, no matter how we calculate the length of time "during which scientifically accurate and explicit records have been kept," we effectively are forced to concede that neither in language nor in geology have all possible types and magnitudes of phenomena necessarily occurred before our eyes.

Gould (1998: 211) has made this very point in a particularly succinct and apposite way (cf. also Wells 1973: 424) by writing that:

> [to] regard nature's laws as invariant in space and time . . . [is] to articulat[e . . .] a general assumption and rule of reasoning in science . . . [, but it is] false . . . [to] extend such a claim to current phenomena (rather than universal laws) . . . [; then,] we surely go too far. The present range of observed causes and phenomena need not exhaust the realm of past . . . [ones].

Yet, by constraining themselves to use only the present in order to explain the past, some linguists have done exactly what Gould cautions against. In particular, instead of assuming that whatever occurs now is independently motivated and is thus available to be invoked in order to explain the past, even an old hand at historical linguistics like Lass (1978) instead once chose to adopt a struthious viewpoint – that of an ostrich – which in effect really does say that, "if we can't see something now, then it couldn't have existed then." This kind of claim (which suggests that nothing can be postulated that has not yet been seen) may seem to be so extreme that no right-minded diachronician could ever have even implied it, but cf. Lass (1978: 274): "If we adopt a 'uniformitarian' view of language history . . . , then what we can reconstruct is . . . limited by our empirical knowledge of things that occur in present-day languages." And Lass (1978: 277) is even more adamant: "If we reject the binding force of uniformitarian principles on the content of history, then we reject all interesting history" (for a less extreme view of uniformitarianism, however, see Lass 1997). The approach taken by Lass (1978) and certain like-minded scholars admittedly is quite wonderfully constrained, but this virtue does not compensate for its inconsistency with modern science – which, after all, has deposited promissory notes for many kinds of initially unobservable (and many still unobserved) constructs. There simply is no absolute basis for forbidding all hypotheses regarding unobserved elements in either a spatial or a temporal dimension.

Digging so deeply below the surface, in either linguistic or geological bedrock, is not very common among diachronicians of language, but our doing so here serves to show that an accurate summary of most discussions of uniformitarianism by historical linguists over the past two decades is quite reminiscent of a line from a short story by H. H. Munro ("Saki") (1924): "A little inaccuracy sometimes saves tons of explanation." Perhaps this strategy lies behind Lass's (1980) apparent exaggerations in favor of positing for the past only presently observable phenomena. Perhaps, too, it explains why Lyell has gone into so many older histories of geology (and biology), and even into newer introductory textbooks, right up to the present day, as an essentially error-free warrior-hero of science who vanquished ignorance and conquered religiously inspired anti-scientific prejudice – with not a word about his exaggerations of uniformitarianism or his creationist beliefs. These virtual hagiographies, in turn, clearly dominate the view of geology presented in the most-quoted monograph on uniformitarianism in linguistics, Christy (1983) – whose almost exclusive focus on Lyell's own self-servingly (if unintentionally) misleading blend of substantive and methodological uniformitarianism has not served to enlighten linguists either about language change or about pre- and post-Lyellian geology. For example, there were histories of geology (and biology) available long before 1982–3 whose discussions of the relevant issues would have helped avoid the canonization of Lyell (and the turning of the catastrophist Cuvier into a veritable scapegoat); cf. Davies (1969: 218):

> Lyell and his disciples were mistaken in their belief that earth-movements have acted incessantly and with the same intensity throughout geological time, and their opponents, with their theory of catastrophes alternating with periods of calm, came closer to the modern conception of Earth-history as a series of orogenies [cases of mountain formation] separated by periods of quiescence . . . [. T]he sole mistake of the catastrophists was to regard the earth-storms as sudden cataclysms occupying a period to be measured in days rather than in the millions of years demanded by modern geology.

(This passage once again anticipates our discussion of punctuated equilibrium in sections 1.2.3.4 and 1.2.3.5 below.) Here, we would only add that a more positive picture of Cuvier (though by no means a whitewash) emerges in such works as Coleman (1964), Outram (1984), and Rudwick (1997).

Admittedly, we may not be typical in our enthusiastic reaction to accounts of geological (and biological) controversies like those in Davies (1969), Rudwick (1972), Mayr (1982: 375–81, 875, 881–2n.9), and Gould (1987). Still, we personally find these to be nearly as gripping as detective stories, and we urge linguists – particularly all students of language change – to read such works, and also to read collections of original geological classics like those in Albritton (1975), rather than consulting only sanitized summaries written at one or two removes. It is apparently only in this way that certain misleading ideas about uniformitarianism can be avoided. First, there are a number of writers on linguistic topics from the mid-nineteenth-century and before whose verifiably uniformitarian leanings tend to be neglected;[43] for discussion, see especially Aarsleff (1982), Naumann et al. (1992), and Janda (2001: §8). Second, neither Lyell nor his close precedessor Hutton (1788, 1795) nor the latter's devoted apologist Playfair (1802) can by any means be considered the originator of the concept of uniformitarianism; crucial in this regard is Aarsleff's (1979: 316) observation that:

> [i]t is characteristic of the history of ideas . . . , [and especially] of its weakness, that it does not find th[e] . . . principle [of uniformitarianism] until the word had been created . . . around 1840. But there is an analogue in the early seventeenth century in the discussion and controversy that followed Galileo's writings on Jupiter's moons, on the surface of the moon, etc. Indeed, the rejection of the hierarchical Aristotelian universe (with its fixed spheres, etc.) marks the assertion of a uniformitarian view of nature.

Given that Aarsleff is thanked by Christy (1983: vi) for "invaluable advice" during the writing of that book, and that another of his works is cited by the latter author, it is puzzling that Aarsleff's earlier (1979) comments about pre-Lyellians who advocated what was basically uniformitarianism long before that term was coined (by Whewell in 1832, it will be recalled) are not mentioned anywhere by Christy (1983). At any rate, we believe that it is crucial to emphasize that the list of pre-Lyellian uniformitarians (in either theory or practice) is extremely long, that it reaches back to the early 1600s and is more or less continuous through to Lyell's time (and afterward), and that it is much more international (in the sense of pan-European) than one might expect.[44] In addition, Sober (1988), has recently emphasized the centrality of uniformitarian ideas in the scientific work of Newton (1687) and the philosophical work of Hume (1748): "Newton's idea[s] implement . . . an Ockhamite principle of parsimony" (Sober 1988: 52–3), while "Hume gave prominent place to an idea he called the Principle of the Uniformity of Nature . . . [– i.e., across] space and time" (Sober 1988: 41). Since these facts were known even during Lyell's lifetime (and since it is also evident that Lyell was strongly influenced by Newton), we find it almost incomprehensible that Lyell and Hutton so regularly receive credit as, so to speak, the father and the grandfather of uniformitarianism. Probably the main reason for this is that, as we have already emphasized repeatedly, Lyell (1830–3) blended together at least four kinds of uniformities, and so this may have made his proposals seem unique – although, as we have seen, this is ultimately not to his credit (a point which we take up immediately below).

A third point worth repeating here is that a truly large number of mid-to-late-twentieth-century geologists (and biologists) have emphasized that Lyellian uniformitarianism is *not*, despite that author's best (albeit probably unconscious; cf. Gould 1987: 119) efforts, an indivisible monolith of a notion that inextricably combines uniformities of law, process, rate, and state. Gould (1987: 118) himself "single[s] out the work of Hooykaas (1959), Rudwick (1972), and Porter (1976)" as having first pointed out the cracks in the alleged unity of Lyell's uniformitarianism, but Gould (1965) had also come to the same principal conclusion.[45]

Closing the circle by returning to the subject of Aristotle's blade in Ockham's razor and using them to cut away an unnecessary entity, we can summarize both this and the previous subsection by saying that (in a strict sense) linguistics, geology, biology, and other fields with a historical component do not really have a uniformitarian *principle*. Instead, they have only a uniformitarian *theorem* – at least as revealingly expressed, we think, by a name like *informational maximalism*, which we discuss below. This unprincipled conclusion, so to speak, follows because the only two valid aspects of uniformity – uniformity of law and uniformity of process (which have misleadingly come to be associated more with Lyell than with his predecessors, who developed them) – are in fact both straightforwardly derivable from the familiar principle of parsimony (or simplicity). The other two principal senses of uniformity – uniformity of rate (or effect) and uniformity of state (or configuration) – both of which are non-methodological and hence subject to empirical (dis)confirmation – are both demonstrably false in the general case, though we must concede that gradualness is not infrequently found in particular cases (yet once again, cf. the subsequent discussion of punctuated equilibrium in sections 1.2.3.4 and 1.2.3.5).

It is a good sign for historical linguistics that the majority of discussions which specifically treat uniformitarianism tend to focus primarily on uniformity of process (introduced above as an independent-motivation-related criterion) and only secondarily on uniformity of law (introduced above as a more directly parsimony-related criterion whereby two sets of laws – each for a different time – are clearly inferior to one set of law holding for all time(s). In such works, uniformity of rate tends to receive little, if any, (tertiary) attention, while uniformity of state is hardly heeded at all. Thus, for example, Collinge's (1994b: 1561) remarks on the historiography of historical linguistics single out uniformitarianism as a "desirable . . . controlling subtheory" for Neogrammarians like Osthoff and Brugmann (1878), who reasoned that (in our adjustment of Collinge's translation) "the psychological and physiological nature of [hu]man[s] as speaker[s] must have been essentially identical at all epochs" (here, intriguingly, we seem to be on the border between the uniformities of law and of process).

In dealing here with the nexus of issues usually discussed together under the Lyellian rubric of *uniformitarianism*, we have so far avoided proposing any new names for specific senses falling under that umbrella term – though we have suggested that the "*u* . . . word" itself be dropped, partly because it does not represent a basic principle, anyway, but just a theorem derivable from the principle of parsimony (i.e., Ockham's razor and Aristotle's blade). We should mention, however, that some scholars have dispreferred *uniformitarianism* on such grounds as the fact that this term would also apply to a universe which showed uniformity because every event was controled by the intervention of divine whim (cf., e.g., Mayr 1972/1976: 286). On the other hand, there are also difficulties with the related proposal to give *uniformitarianism* the alternative name *actualism* on the grounds that the principle's main force is that the present is the key to the past. As has already been discussed above and elsewhere (cf. Janda 2001: §8), the main reason for mentioning the present in connection with the study of the past is that the present is the time about which we normally can gain the most information. But this is not a necessity; an unfortunate conjunction of industrial accidents, environmental problems, political turmoil, and arbitrary, dictatorial governments could cause it to happen that, at some point in time, more information was available (and could be gathered) about language use at a recent past time than about speech in the present.[46] Hence the term *actualism*, we would claim, actually suppresses the crucial fact that the present is important to the study of the past, not simply because it is the present, but because it is the time at and for which the greatest amount – and the greatest variety – of information is normally available.

To a great extent, then, what we should really strive for, in diachronic pursuits such as historical linguistics, is what could be called *"informational maximalism"* – that is, the utilization of all reasonable means to extend our knowledge of what might have been going on in the past, even though it is not directly observable. Normally, this will involve a heavy concentration on the immediate present, but it is in fact more realistic just to say that we wish to gain a maximum of information from a maximum of potential sources: different times and different places – and, in the case of language, also different regional and social dialects, different contexts, different styles, different topics, and so on and so forth. We can recall here the hypothetical situation discussed at the start of section 1.2.1 above, where we listed two alternatives involving very different collections of information about the same event: on the one hand, a few still-life photographs of an eroding hillside; on the other hand, a series of time-lapse photographs of the same "event." What time-lapse pictures do, of course, is to maxim(al)ize the available information in comparison with just a few random snapshots, and we would suggest that it is the sworn duty of every kind of historian – of language, of natural events, or of (non-linguistic) human acts – to exploit any ethical means available in order to reach such an information-maxim(al)izing goal. (We should consider renaming this approach, however – and thus think about calling it *"informational maximality"* – if we want to avoid any negativity that might tend to accompany words ending in *-ism*.)

Now, uniformitarianism in some of the senses discussed here – most profitably following Gould (1987) and similar-minded others – can be a remarkably powerful and beneficial tool in this pursuit of maxim(al)izing information. For example, it sometimes brings a vigorous breath of fresh air into diachronic investigations when a researcher suddenly says, as Glassie (1968: viii) did about historical studies of folklore, "We . . . have talked too much in the past tense . . . [;] our methods have been too few, our fields of investigation too limited." And issues centering on issues of uniformitarianism – both pro and con – have recently invigorated debates among historians of family life as to whether and when families in earlier times lived their lives in ways (e.g., regarding child-rearing) that were basically different from the practices of our own time.[47] Indeed, discussions concerning how studies of earlier times by present-day scholars should best be carried out – and how students can most effectively be instructed about the past, even if they do not later intend to become diachronicians of any kind – quite commonly center on uniformitarianism-related issues.[48] But there are certain other senses of *uniformitarianism* that can turn this principle into a straitjacket which hinders the formulation of reasonable hypotheses about the past and about the why and how of change. Let us therefore now cease any and all uninformèd tarryin' in *-isms*, and thus turn back now to a (re)consideration of the basic object under scrutiny here – change itself – all the while attempting to maxim(al)ize the amount of relevant information about it which we can efficiently assemble and concisely present.

1.2.3 Change revisited

> *The description of a language is not achieved through taking apart all the elements of its delicate machinery any more than a watch would be usefully and exhaustively described through the linear display on a green cloth of all its springs and cogwheels. It is necessary to show how all the elements of both the language and the watch cooperate when at work. Anatomy, unless studied with a view to accounting for physiology, would amount to some sort of "necrology" or corpse-lore of little use or interest to anybody except perhaps professional embalmers. So far we have had, in . . . linguistics, a little too much anatomy and not enough physiology, and the rigor after which some of us are striving too often resembles rigor mortis. But no analogy is fully satisfactory. . . . In the case of languages, observation will show, not only how they function today, but also how the ever changing and conflicting needs of their users are permanently at work silently shaping, out of the language of today, the language of tomorrow.*
>
> André Martinet, "The unity of linguistics," *Word* 10.2–3 (1954: 125)

> *What model will ever catch process? . . . [A] history that claims . . . realism must surely catch process – not just change, but the changing, too.*
>
> Greg Dening, *Mr. Bligh's Bad Language: Passion, Power and Theatre on the "Bounty"* (1992: 6)

Most if not all works on language change which are known to us take the concept of change essentially for granted. Their reasons for doing so may well have something to do with the difficulty of precisely and accurately characterizing the relevant notion. Take, for instance, one philosopher's definition – that of Bertrand Russell (1903: 469 [§442]):

> Change is the difference, in respect of truth or falsehood, between . . . [(1)] a proposition . . . [P] concerning an entity . . . [X] and a time T and . . . [(2)] a proposition . . . [P′] concerning the same entity . . . [X] and another time T′, provided that the two propositions . . . [P and P′] differ only by the fact that T occurs in the one where T′ occurs in the other. . . .

For Russell, that is, an entity X can be said to have changed between times T and T′ if some proposition concerning X is true at T but false at T′, or vice versa. Significantly, this much-discussed definition does not require the two relevant times T and T′ to be chronologically adjacent, and so it apparently permits use of the term *change* with reference to diachronic correspondences between states which are temporally quite distant from each other: say (to take a linguistic example), between reconstructed Proto-Indo-European (PIE) *c.*5,000 BC versus present-day Modern English in AD 2000.[49] But Russell's (1903) account of change was soon directly challenged (along with much previous philosophizing about time in general; see here section 1.3 below) by J. M. E. McTaggart's arguments to the effect that, since change crucially involves time, but "nothing that exists can be temporal," then "time is unreal," and so change does not exist, either (1908: 457).[50] In response to McTaggart's provocative claims, a defense and clarification of Russell's approach to change (though not to time in general) was later provided by C. D. Broad (1938). Broad more explicitly narrowed the sense of his definiendum in ways which strike the present editors/authors as more conducive to explaining change(s) in language – as long as we take "change" here to collapse the distinction made between *(individual) innovation* and *(group-wide) change* in section 1.2.1 above. That is, Broad's account is more centrally focused on spatiotemporal and causal connectedness – and hence on differences which, for language, could arise within a single speaker's lifetime (1938: 297):

> There are certain series of successive events . . . such that the members of any one such series are intimately interconnected by . . . [particular] spatial, causal, and other relations, which do not interconnect members of any two such series. Each such series is counted as the history of a different thing. Now successive members of one such series may differ in respect of a certain quality; e.g., one term may have the determinable quality Q in the determinate form q_1 . . . [, while] . . . a later term may have Q in the form q_2. The statement "The thing . . . [X] changes from q_1 to q_2" is completely analyzable into a statement of the . . . kind . . . "There is a certain series of successive events so interrelated that it counts as the history of a certain thing [X] . . . ; e_1 and e_2 are two successive adjoined phases in this series . . . [,] and e_1 has Q in the form q_1 . . . [, while] e_2 has Q in the form q_2".

In the time since Broad wrote the foregoing, the already considerable philosophical literature on change has grown truly massive (but cf. the ancient-to-modern historical surveys given in brief by Čapek 1967 and Turetzky 1998 or at length by Strobach 1998). Still, we assume that an updated general account of the above sort (as most cogently explicated by Mellor 1998: 70–2, 85–96, 98–100, 115–17 et passim; Strobach 1998; and their most recent references) will be adequate for the purposes of this introduction (and in fact as a background for all the chapters in this volume, just as each author implicitly assumes). Hence the main remaining issue to be addressed here concerns what can be viewed as the difference between change(s) in a token versus change(s) in a type. This distinction is particularly relevant for historical linguists, as is evident from the amount of discussion devoted to its ramifications in the following subsections. But the same difference often arises in everyday life.

For instance, if someone begins a conversation or discourse by saying, *"That dog has changed a lot since I last visited your breeding farm,"* this ambiguous start might be continued either with *"– it's full-grown now"* (revealing that a dog in a specific sense is being discussed) or with *"– the spots have been bred right out of it"* (revealing that a breed of dog in a generic sense is at issue). In this case, saying that one particular dog has changed involves a report on a comparison made across two different temporal states of a single concrete entity, but saying that a breed of dogs has changed requires a comparison made across a series of different entities (associated with two at least partly distinct times) which are still taken to (help) constitute earlier and later states of one abstract entity. Latent here, of course, is the question of species as realities versus abstractions – an extremely vexed complex of issues in biology beyond our ability to do justice to here (but see Wilson 1990).[51]

Hence, after this broad but rapid pass through the general issues involved in defining both change and what changes, we now return to specific issues of linguistic change.

1.2.3.1 *Processes of change versus accidental gaps in the historical record*

With regard to the phenomenon of change itself, we would argue that anyone who wants to understand the mechanisms by which change takes place – in language or indeed in any happenstance or activity or event – must (i) find two well-attested different states which are as close together in time as possible and (ii) learn as much about each one as is humanly possible, since this provides the best basis for determining the nature of the transition between them.[52] Most of the time in historical linguistics, however, we have one stage about which we know little and another stage about which we know even less. In such (myriad) cases, one may well ask whether the study of language change is a reasonable or even a possible endeavor. Of course, we can try to make a virtue of necessity, and so rejoice in the fact that extremely limited bases of comparison of this sort – with two fragmentarily attested stages – prevent us from being overwhelmed by data (recall the discussion in section 1.2.1.7 above). But the extensive filling-in which this approach unavoidably entails can lead diachronic linguists to reconstruct direct continuities in places where the actual history of a language may well have included many abandoned offbranchings, or even a succession of extremely similar dead ends. As that inimitable giant of Romance historical linguistics, the late Yakov Malkiel, once put it (1967: 149):

> [N]ot only does the actual progress of research fail to follow a straight line, but the development of language itself . . . reveals, on microsopic inspection, a number of . . . sharp curves and breaks . . . [,] an angularity which, as a rule, only in long-distance perspective yields to the soothing image of straight, beautifully drawn lines.

Bynon (1977: 6), on the other hand, has talked of "an optimal time-lapse" of some "four or five centuries" between the two linguistic states being examined. She reasons that this "is most favorable for the systematic study of change . . . [:] the differences between successive language states are then sufficiently large to allow the statement in the form of rules of completed changes . . . [,] yet continuity is not at stake – one is clearly still dealing with 'the same language'." (Or is one? See both above and below for further discussion of this notion of "sameness.") Related to this is Bloomfield's (1933: 347) assertion that "the process of linguistic change has never been directly observed; . . . such observation, with our present facilities, is inconceivable."[53]

Still, as Labov has forcefully argued, with regard to what he first documented on Martha's Vineyard and has repeatedly seen confirmed since (see chapter 8 by GUY): "the mixed pattern of uneven phonetic conditioning . . . [with] shifting frequencies of usage in various age levels, areas, and social groups . . . is the process of linguistic change in the simplest form which deserves the name" (1963: 293). In short, overall processes of linguistic change are not unobservable. Indeed, it was already the case in the early 1960s that the particular changes involving diphthong centralization by English-speakers on Martha's Vineyard (e.g., in *knife* and *house*) had been documented first-hand (via several kinds of recordings: audiotapes, spectrograms, tables or graphs, phonetic transcriptions, and the like). Yet even Labov's work on these data was based on inferences about change extrapolated by means of a comparison of Martha's Vineyard in the early 1960s with records from some thirty years earlier – that is, by looking at two chronologically close stages (for related discussion, see also chapter 24 by WOLFRAM AND SCHILLING-ESTES).

We thus learn about change from comparisons of various sorts. One approach performs "vertical"[54] comparison – that between different stages of a language – and so relies on the interpretation of documentation linked with some earlier stage(s), whether in a written form requiring more intensive philological analysis[55] or in some other form requiring less intensive analysis (e.g., wax recordings, tapes, movies, etc.);[56] from these sources, we extract inferences about change by looking at what is different between the two stages. But we can also perform "horizontal" comparison – that between related languages[57] – and so make inferences about change that rest on two crucial assumptions. These are, first, that all related languages must ultimately have arisen from a common earlier source (see chapter 4 by LYLE CAMPBELL) and, second, that finding mismatches in comparable items between the two languages implies that at least one change – and possibly more – must have taken place.[58] In either way, we can learn something about language change; in both cases, comparison is necessarily involved.

Actually, these, observations point to a further complication, since it is far from obvious that the same object is really being compared in any intended vertical comparison between two of its different stages – this is the previously mentioned problem of type change versus token change. For one thing, a notion such as "English," even if it is temporally limited as, say, "twentieth-century English," and geographically further localized as, say, "twentieth-century North American English," is always (though see nn. 35, 36) something of a convenient fiction, a construct which allows us to proceed with analysis by suggesting cross-temporal uniformity but then, when minutely scrunitized, quickly breaks down. For another thing, even if we agree that we can talk in terms of "English of the twentieth century in North America" and compare it with "English of the eighteenth century in North America," will there really be something(s) to compare meaningfully?[59]

For example, further arguments are given in the following subsection (see also chapters 7, 21, and 14 by HALE, BENJAMIN W. FORTSON IV, and LIGHTFOOT, respectively) that it is valid to view the transmission of language over time as necessarily discontinuous, since the twin facts of birth and death of individual speakers require some version of the object "Language"/"language X" to be recreated anew within each individual as she or he helps define a new generation. But, in that case, seeking the continuity that is needed for cross-temporal comparisons may often or even always be in vain. Rather, we must recognize the social fact that, as the members of each identifiable generation recreate language for their own use, language is continuously being integrated into a society that is not uniform in terms of age but still takes in new members seamlessly from new entries into it (i.e., new individuals). Thus, the social dimension of language must be a crucial ingredient in any attempt to provide some sense of the continuity that exists, overall, throughout the history of a language.

To take yet another tack, though: Is "English" as instantiated in one individual necessarily the same as "English" as instantiated in another? If not, will a valid cross-temporal comparison ever be possible? The question of asking whether "English" as an entity covers Old English, Middle English, and Modern English is thus akin to the issue of considering whether the "New York Yankees" is/are an entity that covers both the 1927 instantiation and the 1998 instantiation of that team, even though all that is the same is the "corporate" being – the "Yankees" as an abstraction. On a more personal level, given that most of the cells in a person's body are completely replaced within a certain number of years (seven, according to one tradition of folk wisdom), is there any real sense in which we can consider ourselves to be "the same" individual at different stages of our life? It was a negative response to this kind of query that apparently induced the Ancient Greek philosopher Heraclitus to make his famous statement that "you can't step twice into the same river" (cf. section 1.2 above), but the basic question here at issue was not just asked but also answered more than a century ago by the physical scientist and writer John Tyndall (1897: 50–1):

> Consider . . . personal identity . . . in relation to . . . molecular form . . . [:] the whole body . . . wastes . . . , so that after a certain number of years it is entirely renewed. How is the sense of personal identity maintained across this flight of molecules? . . . Constancy of form in the grouping of the molecules, and not constancy of the molecules themselves, is the correlative of this constancy of perception. Life is a *wave* which in no two consecutive moments of its existence are composed of the same particles [original emphasis].

This same phenomenon is, if anything, even more characteristic of the way in which speakers view their languages as maintaining diachronic coherence and essential identity in the face of constant variation and change. In fact, one historical (and general) linguist, as brought out in the next subsection, has even gone so far as to claim that "[l]inguistic change does not exist," and he seems to be right – if not in every sense, then (as the following discussion shows) in at least one sense of *change*.

1.2.3.2 *(Potential) type immortality via a discontinuous series of mortal tokens*

Most linguists, we think, would agree that an individual person's language is more than the totality of sentences that he or she has ever uttered – or will ever actually utter – since an infinity of possible sentences always remains unsaid. It therefore makes sense to identify a person's idiolect with the neurologically instantiated cognitive system(s) allowing him or her: (i) to use and understand language, spoken or signed, and (ii) thereby to follow or flout the group- and community-norms of his or her surroundings.[60] In this sense, the birth of a new linguistic pattern correlates with the moment of its initial cognitive adoption, not with its first application in speech. Even more linguists, we are confident, would agree that speakers are mortal – from which it follows that cognitively realized linguistic systems exist, on average (depending on the conditions of life at any given place), for less than 100 years, with many enduring less (often, alas, much less) than the biblical three score years and ten: 70 years. But particular (sets of) speech-patterns used by older speakers can exceed these temporal limits of human mortality, because communities are continually replenished by the births of younger speakers willing and able to replicate some version of such patterns.

Yet, in terms of Tyndall's (1897) above-mentioned distinction, this chain of generations is interlinked not by "constancy of the [same neural] molecules [of grammar] themselves," but by "constancy of form in the grouping of . . . [different] molecules" – or even more abstract entities – of grammar. For example, in cases where historical linguists tend to say that *a* "becomes" *a′* (commonly abbreviated as $a > a'$), it really is not completely accurate to substitute a description in which *a* "is replaced" by *a′*;[61] rather, it is most revealing to characterize such cases by saying that, after a time when (only) *a* is used, *a′* is introduced and varies with *a* – until *a* no longer is used, but only *a′*. Given this, there follow certain conclusions as to the nature of language and change; it was Coseriu (1982: 148) who pursued these latent implications to their most drastic but most rigorously logical extreme, contrasting *dúnamis* (Classical Greek for 'power, ability, faculty' – thus here, 'system of procedures') with *érgon* (Classical Greek for 'work, deed,' thus here, 'product'):

> The actual problem of linguistic change viewed from the standpoint of . . . language as a creative activity can best be understood . . . if we start from the assumption that linguistic change "does not exist" . . . [. T]here are three ways in which what has been called "linguistic change" does not exist: first, it does not exist as a modification in an "object" conceived of as being continuous, as a process of change in external phenomena (as, for example, $a > e$); second, it usually does not exist for the speakers of a language, who normally are convinced – so far as their own activity is concerned – that they are continuing a linguistic tradition without change . . . [, and] third, it often does not exist in the language . . . as a system of procedures, but rather only in language . . . as a product of already given procedures of . . . language, which as such do not become different.

Coseriu's third point appears to be the least controversial: regardless of whether use of a novel speech-pattern is characteristic of an entire community or of only one individual, an insightful analysis will recognize (as argued above) that the origin of such a pattern almost always lies earlier in time than the moment(s) of its first utterance.[62] For example, one of the authors (Janda) recalls that, when he first heard someone pronounce the past tense of *speedread* with ablaut in only its second element (as [spídrɛ̀d], his reaction was to wince. This was because he suddenly realized, for the first time, that his own analysis of this verb involved a quasi-serial structure which would require him to say double-ablauted *spedread* ([spɛ́drɛ̀d]), even though he had never heard this (innovative?) past-tense form before and in fact did not have any occasion to utter it himself until much later.

Coseriu's second sense in which linguistic change is non-existent has been challenged by proponents of the view that some (especially older) speakers do become aware of the directionality and change inherent in linguistic variation (cf., e.g., Andersen 1989, with whom we tend to agree), but nearly the identical conclusion had earlier been reached by writers like Bynon (1977: 1, 6):[63]

> [S]peakers for whom a . . . language serves as a means of communication are in general quite unaware of its historical dimension. . . . [B]ecause it is embedded in variation patterns current within the community, the process of language change lies for the most part outside of the individual speaker's awareness; pre-occupied with the social significance of alternative forms, . . . [most speakers are] largely unaware of their correlation with time . . . [. Yet] the present state [of a language] is the only one which can provide . . . full information on all . . . phenomena, including . . . change.

This issue is far from being moot, in part because Labov (1972aff) has demonstrated that middle-aged adults often play a crucial early role in ongoing changes, due to their being incomparably more sensitive to the social ways of their community than are young children, and in part (as well as relatedly) because Labov and other variationists have taken the central ingredient of linguistic change to be an alteration of sociolinguistic norms. Obviously, too, if we grant the validity of Coseriu's (1982) first point, then innovations in a speaker's idiolectal grammar during his or her lifetime are left as the only possible kind of change in language: if such phenomena are rejected (as changes), then there is no escape from the conclusion that linguistic change does not exist. Yet it is such innovations in an individual's grammar over his or her post-acquisitional lifetime that most generative diachronicians have found least revealing (or, at any rate, least deserving of their attention). Let us thus turn to the issue on which, despite persistent criticisms from adherents of other approaches to diachrony, there seems to be the most agreement between Coseriu and earlier as well as more recent generativist historical linguists: the discontinuous transmission of language over time (the following discussion of which is expanded from Janda 2001: §3).

It is actually by no means unexpected that discontinuities of diachronic transmission should characterize a phenomenon like language, which shows such relatively abstract patterning and is realized (whether in speech or in signing) by elements that, individually, are highly ephemeral. This is because even an entity with a more concrete nature and greater temporal staying power cannot survive for long on an absolute timescale unless it is recategorized as representing a more abstract type instantiated by a temporal succession of discontinuous physical tokens (for a musical parallel, cf. Hopkins 1980: 615–17 on French composer Maurice Ravel's techniques for expressing the temporal extension of musical "objects" via strategies of movement as well as stasis). The point at issue can be illustrated with reference to a set of nineteenth-century train-car pictures employed – for other purposes, but with equal force – by the Swedish archeologist Oscar Montelius (1899: 260–3), who used the drawings here labeled figures I.1–4 (= figures 73–6 in his article) to exemplify his "typological" method for deriving a chronology of artifacts.[64] For example,

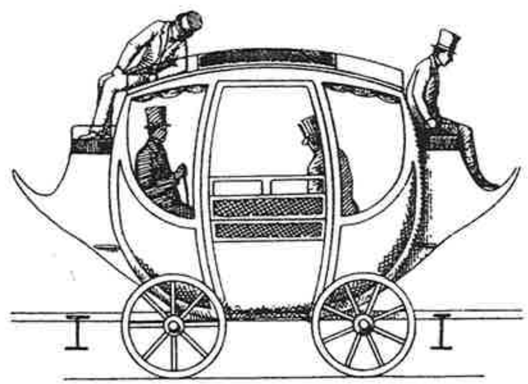

Figure I.1 Montelius's figure 73: British, 1825: the first train-car for passenger transport

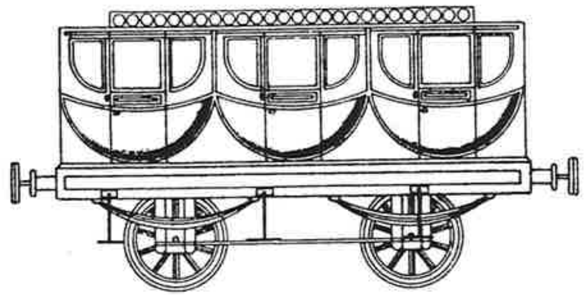

Figure I.2 Montelius's figure 74: Austrian, 1840

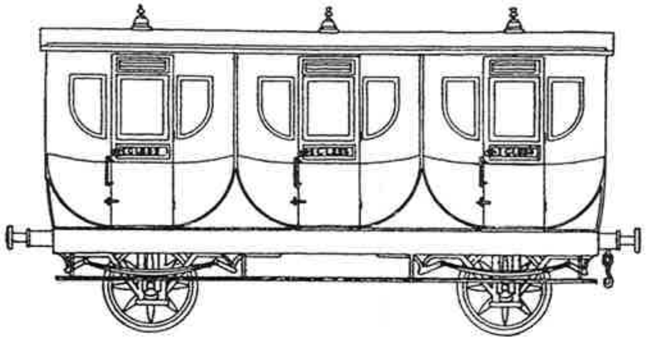

Figure I.3 Montelius's figure 75: one of the first train-cars ordered for the Swedish state railways (made in Germany shortly after the mid-1850s): for first-class passengers

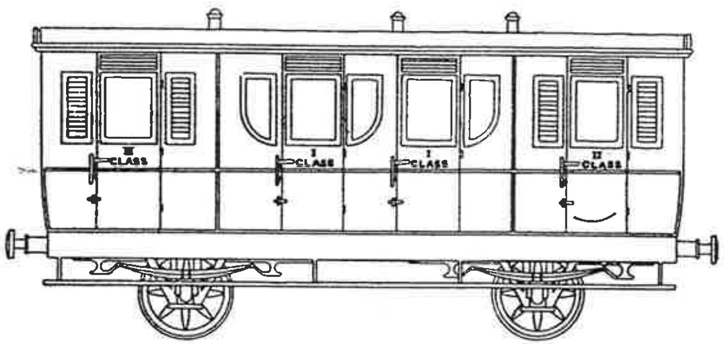

Figure I.4 Montelius's figure 76: another of the first train-cars ordered for the Swedish state railways (made in Germany shortly after the mid-1850s): for first- and second-class passengers

given a set of objects whose respective properties are, schematically, (i) A, (ii) AB, and (iii) BC, this method would analyze these objects as having developed in that order – viz., (i)–(ii)–(iii) – that is, from lesser to greater overall complexity, and with formally intermediate items being medial in time. Now, such an approach is known to face certain problems of temporal ambiguity when it attempts to order prehistoric artifacts whose chronology is as yet unknown on other grounds.[65] But the development of European railroads is a historical development whose exact chronology is not in any doubt.[66] Hence there is nothing to prevent us from hijacking Montelius' train-cars, so to speak, and focusing on the fact that a series of four distinct, discontinuous physical objects can here be viewed as four tokens that are

relatively constant in themselves yet, together, successively instantiate one overall type which is undergoing change (recall section 1.2.3 above on change, tokens, and types).

The type/token distinction is thus indeed crucial as regards discussions of change. That is, we might say (without any reflection) that the European train-car "changed in shape" from rounded to squarish between 1825 and *c.*1857, and we might even figuratively say that the carriage-like British train-car of 1825 "ultimately turned into" the squarish Swedish train-car of *c.*1857 – in both cases describing a type in terms of its earlier versus later tokens at the extremes of a timespan. But (unless railway parts underwent direct physical recycling in the 1800s) we cannot truthfully say that any particular English train-car of 1825, as a concrete object, "literally changed into" a train-car of 1840 (in Austria or anywhere else) much less that it "physically became" a Swedish train-car of *c.*1857. In sum, then, individual (tokens of) train-cars are not immortal, so to speak: they eventually disappear from railway traffic and must be replaced. Yet precisely the continuing construction of new (tokens of) train-cars, even with slightly different properties, allows the (type of the) train-car to survive longer than any one of its particular manifestations ever lasts on the job.

Hence, on this concrete, token-based interpretation, the train-car of an earlier era does not change into, but is instead replaced by, the train-car of a later era, and so a Coseriu of the rails could legitimately claim that, in at least one sense, "train-car change does not exist" – perhaps only to receive the answer that, in another sense, individual (tokens of) train-cars do in fact undergo some physical change over their working lifetimes. But a Labov of the locomotives could then point out that even a figurative, type-oriented approach – one which allows a train-car of one era to be described as changing into a train-car of another era – obscures the fact that, at any given time, there are likely to be several vintages of train-cars in use. For example, the working life of a train-car from 1840 might well have been so lengthy that such an entity could share the rails with a train-car built in *c.*1857, and perhaps even be pulled by the same engine. Even when relativized to a type, then, train-car change, too, surely can sometimes happen through variation due to overlap, not via periodic abrupt replacement of entire vintages of train-cars.[67]

This kind of observation is worth emphasizing, because the present chronological sequence discussed by Montelius (1899) vis-à-vis archeology and here compared to linguistic change involves a persistent property – the curved, stagecoach-like windows flanking the central door(s) on every post-1825 train-car – of the sort sometimes said to require a "historical explanation," as if such a retention could arise, or be repeated, in some way other than synchronically. The implication here is that the older window-style of train-cars built earlier must somehow have been held over into later train-cars by a quasi-physical inertial force. But this ignores the crucial fact of discontinuity. Newly produced train-cars cannot come to have old-style windows unless they were actively – that is, synchronically – designed and built with copies of these; the only place where the motionless sort of inertia can keep old windows is on old train-cars. We can avoid the "historical explanation" trap and its invalid inertial reasoning, though, by recalling the above-mentioned variationist fact that at least some train-cars of an older vintage are likely to have been still in use (or at least vividly remembered) when new train-cars were planned – and in fact probably served as a model and motivating factor for the design of the latter. Since, at every moment, any given state represents either an identical continuation or else a changed version of some earlier state, and since both continuity and change can be viewed as aspects of history, it follows that everything in the universe must in some sense have a "historical explanation," and so this concept simultaneously explains everything and nothing; cf., for example, Janda (1984: 103n.3).[68] It is much more useful, therefore, to consider psychological and sociocultural factors (such as conformity and accommodation) in seeking explanations for the long-term retention of some property across a type's many successive, discontinuous tokens, whether these be train-cars or linguistic systems (i.e., grammars).

Still, in switching our focus away from how design features of conveyances for transporting humans are diachronically transmitted, and back to how human speech-patterns are passed along through time, there is one last (but far from least) parallelism to be noted. Namely, there can be certain periods during which virtually every newly constructed token of a type – either linguistic or rail-related – seems to resemble its predecessor model(s) so closely that no systematic (i.e., type-representative) trend of change in form is evident across such a chain of two or more members (although the latter will of course be physically distinguishable with reference to their non-systematic characteristics).

In the case of train-cars, this practically goes without saying, since it is normally much more profitable in manufacturing to build multiple exemplars of a successful product over several years (by making nearly exact copies of an only slightly varying prototype) than it is to construct one qualitatively unique (type of) ware after another. Thus, although the four train-cars discussed here following Montelius (1899) do indeed represent (regardless of the temporal overlap that they may later have shown) a chronologically accurate series when they are sequenced according to their date of construction and earliest use (first 1825, then 1840, and finally, twice, the mid-1850s), they do not actually form an unbroken chain – since, between any adjacent pair of these, there intervened many other tokens more nearly identical to the earlier model of the two. For instance, the manufacture of the 1825 train-car was followed, over the next several years, by the building of many similar conveyances that did not systematically differ from it. Besides, given that the use of assembly lines and of interchangeable parts was not common until after about 1855, repeated manufacturing of "the same train-car" tended to involve taking the most recently built train-car as a model for creating its successor more than it did the cookie-cutter-like turning out of identical train-cars literally from the same mold(s).[69]

1.2.3.3 *Child-changed or not, language is always transmitted discontinuously*

But, just as it is not a mere possibility but a verifiable fact that, during some temporal spans, the physical features of train-cars were passed along discontinuously – from earlier to later tokens of that type – without *systematic* change, so do we also know that there continue to be times when the discontinuous transmission of a linguistic system's more abstract features too can take place without any *systematic* change – as opposed to idiosyncratic innovation(s). This kind of amazingly exact grammatical cloning (in the non-technical sense of the word)[70] is documented for cases of language transmission from an older to a younger generation like those reported by Labov (1994: 579), who mentions "children as young as three years old" who have near-identical matches with their parents for patterns of quantitative variation like English *-t/-d* deletion (cf. also Roberts and Labov 1995; Roberts 1997). These findings may seem innocuous on the surface (e.g., they surprise few non-linguists), but they have profound implications for synchronic as well as diachronic linguistics.

Most crucially, the fact that language can be discontinuously transmitted from parents to children without systematic change confirms what we asserted above: the main reason to assume discontinuous language transmission is that human life is bounded by natality and mortality. That is, the force obliging us to accept discontinuity is the (delayed) one–two punch of birth and death, not some misguided reasoning whereby the existence of linguistic change and a dearth of imaginable explanations for it somehow foster the desperate belief that only imperfect language acquisition can explain substantial linguistic changes over time. After all, language acquisition as part of discontinuous transmission need not involve systematic change, and (as stressed in the last section) socially motivated (group-oriented) change can be associated with an individual's adulthood – for example, when a lower-middle-class speaker in New York City brings to his most formal styles an off-the-scale frequency for a prestige variant (like "undropped" /r/ in syllable codas; cf., e.g., Labov 1972a: 160 et passim). This is, one might say, the linguistic equivalent of a train-car manufacturer's adding various new external panels, grillwork, and coats of paint to a train already in service for several years after the latter has been moved onto a route passing through up-scale neighborhoods.

Given our insistence on the reality of discontinuity, in language as well as in life (both being bounded by death), it is incumbent upon us to offer at least a sketch of a model suggesting how language is passed along over time, and where the primary locus (or loci) of change is (or are) likely to be, vis-à-vis the different stages of life and the various possible sorts of transmission. We discuss this topic at some length below, but first address a further implication of the fact that discontinuous linguistic transmission is not automatically associated with systematic change, especially during language acquisition in childhood. Namely, if the acquisitional accomplishment of overcoming the challenge of discontinuous transmission by achieving close copies of older speakers' linguistic patterns can be repeated across a large number of generations before there is any major systematic change, then this situation might be considered a linguistic equivalent of the scenario known among evolutionary biologists as "punctuated equilibrium" (and mentioned here above in n. 17).

1.2.3.4 *Peripatric speciation of biologists' "punctuated equilibrium" among linguists*

Though briefly discussed as an attested possibility by Haldane (1932: 22, 102) and anticipated above the species level by the "quantum evolution" of Simpson (1944: 206), the concept variously referred to as *punctuated equilibrium, punctuated equilibria,* or *punctuationism* gained prominence in current evolutionary biology due to the recent writings of two contemporary paleontologists. First (but as yet without new terms) came a short, low-profile journal article by Eldredge (1971), and then a long paper by Eldredge and Gould (1972) in the proceedings of a high-profile symposium. The perspective outlined in those works has been updated periodically by their authors: for example, in Gould and Eldredge (1977, 1993), Gould (1982, 1989, 1997), and Eldredge (1989, 1995, 1999), with the longest dedicated treatment being Eldredge's (1985) book *Time Frames,* which is entirely devoted to – and hence subtitled – *The Rethinking of Darwinian Evolution and the Theory of Punctuated Equilibria* (but see now also – passim – Gould's 2002 *The Structure of Evolutionary Theory,* especially pp. 745–1024). In the nearly three decades since its full-blown emergence, punctuationism has provoked critical reactions of varying severity and cogency,[71] and these, in turn, have elicited very pointed responses from Eldredge and/or Gould. Others as well have contributed defenses and elaborations; as representatives of either or both of the latter, cf. Stanley (1975 ,1979, 1981), Vrba (1980, plus Vrba and Gould 1986), Williamson (1981, 1985), Sober (1984/1993: 355–68), Cheetham (1986), Jackson and Cheetham (1990, 1994, 1999), and Schwartz (1999: 321–30, 354–7, 377–9), among others. In short, the topic of punctuated equilibrium has now achieved such a broad distribution across both the specialist and the generalist literatures on evolutionary biology and other disciplines that it could not do otherwise than eventually enter the consciousness of linguistic diachronicians. Still, as we discuss in this and the next section, the results of linguists' dealings with punctuational matters include a heavy mixture of the vague, the misinterpreted, and the misleading, though we are convinced that a heuristic look at biological punctuationism suggests several largely corrective but nonetheless genuine insights – mainly of a sociolinguistic nature – which are of great value for the study of language change.

At issue in this general debate are a number of related punctuationist claims; a convenient statement summarizing the biological core of these is provided by Eldredge (1999):

> [T]he bulk of most species' histories are marked by stability (... little or no accumulation of anatomical change) ... [. Thus,] most ... change in evolution, assumed to be under the control of natural selection, occurs ... in conjunction with the actual process of speciation, which for the most part occurs through ... geographic variation and isolation. (p. 22)

> [S]peciation – the derivation of two or more descendant species from an ancestral species ... [–] is commonly regarded as requiring, on average, from several hundred to several thousand years to complete. To an experimental biologist, the process is hopelessly slow ... [. But, to] a paleontologist, ... speciation seems almost blindingly quick, especially when contrasted with much longer periods (millions of years, often) ... [during which] species appear to persist relatively unchanged. (pp. 37–8)

Yet one aspect of punctuated equilibrium must be evaluated as most central, while some apparent aspects turn out to be peripheral or even misleading. For example, in the estimation of Gould (1982):

> Of the two claims of punctuated equilibrium – geologically rapid origins and subsequent stasis – the first has received the most attention, but ... [it must be] repeated[ly] emphasized that ... the second ... [i]s most important. We ... [may], and not facetiously, take ... as our motto: stasis is data ... [. I.e., s]tasis can be studied directly ... [, and t]he (potential) validation of punctuated equilibrium will rely primarily upon the documentation of stasis. (p. 86)[72]

> Punctuated equilibrium is a specific claim about speciation and its deployment in geological time; it should not be used as a synonym for any theory of rapid evolutionary change at any scale. (p. 84)

Despite such caveats, however, certain historical linguists and other students of non-biological evolutionary change have been unable to resist the temptation to draw parallels between biological punctuationism and diachronic phenomena in their own fields, particularly on the basis of facts like the following sociolinguistic realities summarized by Labov (1994: 24):

> [C]atastrophic events ... play ... a major role in the history of all languages, primarily in the form of population dislocations: migrations, invasions, conquests ... Other abrupt political changes ... le[a]d to alterations in the normative structure of the speech community. ... [S]ignificant external effects are of this catastrophic type, while all gradual effects are internal, stuctural reactions set off by these rare disruptions. ... The external history of most languages shows the uneven path of development that corresponds well to the sporadic character of sound change [sporadic, that is, in its unpredictability of occurrence, despite the regularity of its outcome]. ... It remains to be seen whether the two types of uneven development can be fitted together, or whether language and social change are both erratic and independently motivated.

After all, this coincidence involving linguistic and politico-demographic catastrophes is extremely reminiscent of the paleontological finding expressed by Eldredge (1985: 168) as follows: "nearly every burst of evolutionary activity represents a rebound following a devastating episode of extinction," whereby the "truly severe extinctions took out up to 90 percent of all species then on . . . earth." (Further discussion of extinction rates and even apparently cyclic mass-extinction patterns can be found, e.g., in Lawton and May 1995 and the extensive references there, as well as in more generally oriented works like Raup 1986.)

It is thus not really surprising that, in light of its suggestive name and its seeming applicability well beyond biology, the concept of punctuated equilibrium has exercised an influence stretching deep into other fields like psychology, anthropology, sociology, political science, economics, philosophy (cf. the range of papers in Somit and Peterson 1992 on *The Punctuated Equilibrium Debate in the Natural and Social Sciences*, to which *"and in the Humanities"* should have been appended), and, most recently, historical linguistics. However, radically (and radially) extending punctuationism outside biology has led to such far-reaching reinterpretations that these quasi-mutations among peripheral populations have ended up paralleling the very evolutionary mechanism that underlies punctuated equilibrium itself. This is, namely, *peripatric speciation*, one subtype of the larger category of *allopatric* (née *geographic*) speciation,[73] whose importance was first pointed out by Mayr (1942, 1954, 1963: 481–515 et passim) in work often seen as building on the sort of findings reported by Dobzhansky (1937) and particularly on Wright's (1931, 1932) earlier research concerning genetic drift (i.e., distributional asymmetries arising in small populations), most of it later summarized in Provine (1986). As we have already indicated, certain works on historical linguistics exemplify precisely this phenomenon whereby conceptual speciation of "punctuated equilibrium" has occurred on the periphery (or, more accurately, the exterior) of biology: thus, for instance, the publisher's blurb (on p. i) for Dixon (1997) describes that book as "offer[ing] . . . a new approach to language change, the punctuated equilibrium model." Similarly, Lass (1997: 304) takes it to be obvious that, "not dissimilar to the picture of 'punctuated equilibrium' . . . in biology, . . . languages . . . vary all the time, but they change in bursts."

Forming the background for these issues is Darwin's (1859: 341–2) contention, in *The Origin of Species*, that apparent gaps in the evolutionary development of species are simply accidental lacunae resulting from the non-preservation of intermediate forms in the fossil record:[74]

> The geological record is extremely imperfect . . . [;] this fact will to a large extent explain why we do not find interminable variants . . . connecting together all the extinct and existing forms of life by the finest graduated steps. He who rejects these views on the nature of the geological record . . . will rightly reject my whole theory.

Disagreeing with this claim, however, Eldredge and Gould (1972) took as their point of departure the view that evolutionary gaps are not apparent, but real, so that abrupt transitions in the fossil record at a given site or region must be taken at face value. On this view, evolution – at the level of *species*[75] – does not occur via infinitesimal changes continuously accumulating at a constant rate, but via occasional, relatively short bursts of comparatively rapid speciation which can be seen as starkly setting off (or punctuating) the considerably long intervening periods of non-speciational stasis (i.e., periods of provisional equilibrium). Crucial here is the focus both on the geologically sudden appearance and on the subsequent persistence of entire species – particularly on the permanent replacement of one species by another from within the same phylum (i.e., either species selection or, alternatively, species sorting; cf. Stanley 1975, 1979; Gould 1985, 1990; Eldredge 1995: 119ff) – rather than on the gradual transformation of a complete species or complete phylum ("phyletic gradualism") or on transitions between individual organisms. This fits well with the arguments provided by Ghiselin (1974, 1987, 1989) and Hull (1976, 1978, 1999), among others, in favor of treating species themselves as "individuals" (i.e., as collectivities functioning as higher-level units) which are smaller than phyla but larger than organisms (and populations). For more detailed discussion of species and species formation, see Mayr (1963: 14, or 1957), on the much earlier literature, and Endler (1977) or White (1978), plus Jameson (1977) or Barigozzi (1982), on the more recent literature. Rather closer to the present are the treatments of species and speciation given in Ereshefsky (1992) or Claridge et al. (1997), Wilson (1990), Giddings et al. (1989), Otte and Endler (1989), Kimbel and Martin (1993), Lambert and Hamish (1995), and, most recently, Howard and Berlocher (1998), Maguran and May (1999), or Wheeler and Meier (2000).

Bringing to the punctuation-versus-stasis distinction a primary focus on species-as-individuals, rather than on organisms-as-individuals, is what allows Eldredge, Gould, Stanley, Vrba, and others to avoid contradiction in maintaining both (i) that transitions between species are abrupt and (ii) that this fact need not be attributed to so-called "macro-mutations" in organisms (for background, see Dietrich 1992). Hence punctuationists can adopt a non-Darwinian (because literal) reading of the fossil record without abandoning Darwin's adherence to Linnaeus' dictum (cf. von Linné 1753: §77) that nature does not make (evolutionary) leaps: *Natura non facit saltus* [sic].[76] The apparent dilemma here can be resolved by making use of Mayr's above-mentioned notion of allopatric – especially peripatric – speciation. That is, a series of heritable mutations in individual organisms must indeed be responsible for speciation, but this occurs in some other (Greek *allo-*) place than in the ancestral core "homeland," or "fatherland" (Greek *pátra*), of the species – usually taking place, instead, around (Greek *peri*) the edges of its range.

Beyond its suggestive parallelism with the linguistic finding that communicative isolation promotes increasing divergence between dialects, Mayr's (1942, 1954, 1963/1979) achievement in linking together geographical isolation and speciation is noteworthy because it actually represents quite a departure from Darwin's (1859: 51–2) practice in treating:

> the term *species* . . . as one [that is] arbitrarily given for the sake of convenience to a set of individuals closely resembling each other . . . [and so] does not essentially differ from the term *variety*, . . . given to less distinct and more fluctuating forms . . . [, which], again, in comparison with mere individual differences, is also applied arbitrarily, and for mere convenience sake.

In short, Darwin's denial of species as systematic entities existing in nature made it impossible for him to address speciation insightfully – so that, as Mayr (1963: 13) puts it:

> [As for that] . . . great evolutionary classic . . . *On the Origin of Species* . . . [, i]t is not . . . widely recognized that Darwin failed to solve the problem indicated by the title of his work. Although he demonstrated the modification of species in the time dimension, he never seriously attempted a rigorous analysis of the problem of the multiplication of species, the splitting of one species into two.

In fact, as Sober (1993: 143) has trenchantly phrased such matters (cf. also Stanley 1981: 14):

> Perhaps a less elegant but more apposite title for Darwin's book would have been *On the Unreality of Species as Shown by Natural Selection* . . . [– yet, i]f species are [not] . . . real, how could a theory . . . explain their origin? . . . [Indeed,] Darwin thought . . . that there . . . [is] no uniquely correct way to sort organisms into species . . . [;] species are unreal . . . [– but not . . .] higher taxa, such as genera, families, orders, and kingdoms. . . . Darwin [(1859: 420)] thought that th[e] . . . phylogenetic branching process provides *the* objective basis for taxonomy . . . : "all true classification is genealogical; . . . community of descent is the hidden bond which naturalists have been unconsciously seeking, . . . [not] the mere putting together and separating objects more or less alike."

Although Sober (1993) and Mayr (1963, plus previously as well as subsequently: e.g., 1942, 1997) both judge Darwin (1859) as having erred in downplaying the evolutionary role of biological species, it is intriguing that Darwin's approach – essentially the view that "it's branches all the way down" – is basically identical to the perspective which diachronic (and synchronic) linguists have tended to adopt. That is, given the well-known difficulties (primarily of a sociolingustic nature) connected with attempts to define any language as a collection of structurally similar or mutually intelligible dialects, many linguists have viewed *dialect* as the more tractable term, since the joint genetic pedigree of related dialects remains much easier to determine than speakers' possible recategorization of cognate dialects as different languages. It is this viewpoint which yields book titles referring to, for example, "the Italic dialects" (as in Conway et al.'s 1933 three volumes with that same name) or to "the Germanic dialects" (as in Baskett 1920, *Parts of the Body in the Later Germanic Dialects*). At the same time, most historical linguists have avoided the error made by Darwin when he overlooked the importance of isolation for

speciation – and dialect differentiation. On the other hand, paleontologists as a whole have been far ahead of historical linguists when it comes to recognizing the non-recoverability (hence the necessarily incomplete reconstructibility) of certain ancestral entities. And this biological insight, too, is intimately tied up with Mayr's emphasis on the role of peripheral isolates in (peripatric) speciation.

In evolutionary terms, that is, a selectionally shaped mutational development on a species' periphery – whose crucial outcome is reproductive isolation – usually occurs with such rapidity, and among so few organisms, that it essentially never survives into the fossil record. (Recall – from n. 17 – Engelmann and Wiley's (1977: 3) statement that they "do not know of any paleontologist who would claim to recognize an individual ancestor . . . in the fossil record.") What fossils tend to show, rather, is an abrupt replacement such that the sort of organisms remaining in the "ancestral homeland(s)" – so also Dawkins (1986: 238–9) – suddenly yield to those of an originally peripheral variety, whereby this kind of situation arises when ecological changes or other external events promote the return of a once small and ancestor-like (but now large and crucially mutated) allo-/peri-patric population.[77] In this regard, considerable confusion has been caused by biologists and other scholars who have de-emphasized not only Eldredge, Gould et al.'s organism/species distinction, but also their description of punctuations as being quasi-instantaneous in geological time. Given the existence of obvious linguistic parallels to the scenario just sketched (e.g., when a construction that arose and spread slowly within the colloquial speech of a socially peripheral group later enters the formal register of written records with relative rapidity[78]), it is quite unfortunate that disequilibrating punctuations have been misinterpreted as occurring virtually instantaneously in absolute time.

In a (geo)paleontological context, though, a "short" burst of "rapid" speciation is virtually never reducible to a duration any more "punctual" than 10,000 years, and only rarely and serendipitously limited to 10,000–20,000 years in length (cf. Gould 2000: 339–45).[79] This is because, as Stebbins (1982: 16) puts it, often even "60,000 years is so short relative to geological periods that it cannot be measured by geologists or paleontologists . . . [; hence t]he origin of a new kind of animal in 100,000 years or less is regarded by paleontologists as 'sudden' or 'instantaneous'." Thus, for example, the sharp-toned criticisms of punctuationism intended by Dawkins (1986: 230ff, 241–8, 1996: 105, 2000: 195–7) to tie Eldredge, Gould et al. to macro-mutations within individual organisms are simply irrelevant to those authors' actual focus on species-as-individuals. That is, the speciation which eventually occurs via geologically rapid replacement in an ancestral homeland, while far from being either continuous or infinitesimal, still has a gradual (stepwise) component. This is because it requires no saltational macro-mutations of the sort that could produce a human-like or even an insect-like eye in a single leap, as it were, but instead involves a very large number of intermediate generations which simply happen to pass by too quickly, too peripherally, and among too few individuals to appear in the fossil record.

The drastic compressions to which the vagaries of (non-)preservation can subject the objects that are produced (and/or reproduced) over lengthy time-spans are brought home to us, as linguists living and working shortly after the year 2000, by historian Felipe Fernández-Armesto's (1995: 11) suspicions about how little will ultimately remain of our own experiences and memorabilia from the last millennium, in that the author mentions his:

> vision of some galactic museum of the distant future in which diet Coke cans will share with coats of chain mail a single small vitrine marked "Planet Earth, 1000–2000, Christian Era" . . . [. M]aterial from every period and every part of the world . . . over the last thousand years . . : will be seen . . . as evidence of the same quaint, remote culture . . . [: both] bankers' plastic and Benin bronzes. The distinctions apparent to us . . . [today], as we look back on the history of our thousand years . . . , will be obliterated by the perspective of long time and vast distance. Chronology will fuse like crystals in a crucible, and our assumptions about the relative importance of events will be clouded or clarified by a terrible length of hindsight.

Given that distortions of this sort (compression fractures, so to speak) are inevitable whenever the very closest comparanda across fossil records of any kind, linguistic or otherwise, are separated by millennia (in linguistic evolution) or even – to coin a useful term – millionennia (in biological evolution), how can we be so confident about our diachronic-linguistic activity in attempting to reconstruct details and overall structures of earlier language-states – as well as major changes in these – on the basis of arguably scanty textual evidence? Probably the best that we can do is to confess explicitly that any seemingly direct pairing of an apparent etymon with a reflex from which it is separated by hundreds or even thousands of years surely reflects, not an actual innovation, but a diachronic correspondence (recall section 1.2.1 above): that is, it is virtually certain that numerous intermediate steps were involved, even if it is now possible only to speculate about them. For example, the abrupt appearance in documents of a linguistic innovation at a considerably advanced stage of generalization (say, the distinctive palatalization of all consonants before any formerly – but not necessarily still – front vowel) does not force historical phonologists to posit a single macro-change leaping from no change to a maximum effect. After all, it can rarely be ruled out that such a general pattern may have evolved via stepwise extension from an originally much more limited set of inputs and contexts (more detailed discussion along these lines can be found in Janda and Joseph 2001 on sound change and in Janda 2001 on both phonological and morphosyntactic change) – that is, via a linguistic expansion process all of whose non-final stages may have been realized only in informal speech, without any reflection in the formal register of writing (cf. again n. 21).

In short, as an activity based heavily on studying fragmentary, fossil-like documents that are subject to similar vagaries of preservation and destruction, the study of language change, too, can be said to have its "geological" time as well as its peripheral isolates – and this fact justifies micro-mutational alternatives to the previously mentioned objectionable macro-mutations which, in biology, critics like Dawkins have attempted to link unfavorably with punctuated equilibrium. Still, while Dawkins may have aimed at punctuationism (as a whole) and missed, his critical arrow can find at least one mark within the community of historical linguists. In particular, the straw man that Dawkins (1986: 223–4) intentionally sets up in seeking to show that Eldredge, Gould, et al. have not overturned orthodox Darwinian gradualism is strikingly reminiscent of certain writings on grammaticalization theory.[80] Dawkins's straw man is an imaginary proponent of the view that, since "[t]he children of Israel, according to the [biblical] *Exodus* story, took 40 years to migrate across the Sinai desert to the Promised Land . . . [–] a distance of some 200 miles . . . [– t]heir average speed was therefore approximately 24 yards per day, or 1 yard per hour."

Of course, this can hardly be an exact figure, since one must factor in the lack of travel at night (hence Dawkins revises his wilderness speed-figure to 3 yards per hour). Yet, as Dawkins (1986: 223) goes on to observe:

> [h]owever we do the calculation, we are dealing with an absurdly slow average speed, much slower than the proverbially slow snail's pace (an incredible 55 yards per hour is the speed of the world-record snail according to the *Guinness Book of Records*). But of course nobody really believes that the average speed was continuously and uniformly maintained. Obviously the Israelites traveled in fits and starts, perhaps camping for long periods in one spot before moving on.

Now, Dawkins's point in setting up this dummy view is to demonstrate the lack of novelty of the punctuationist ("fits and starts") approach. Next, he continues (still on p. 223):

> suppose that eloquent young historians burst upon the scene. Biblical history so far, they tell us, has been dominated by the "gradualistic" school of thought . . . [, which] literally believe[s] that the Israelites . . . folded their tents every morning, crawled 24 yards in an east-northeasterly direction, and then pitched camp again. The only alternative to "gradualism", we are told, is the dynamic new "punctuationist" school of history . . . [, a]ccording to the radical[s of which] . . . the Israelites spent most of their time in "stasis", not moving at all but camped, often for years at a time, in one place. Then they would move on, rather fast, to a new encampment, where they again stayed for several years. Their progress towards the Promised Land, instead of being gradual and continuous . . . [involved] long periods of stasis punctuated by brief periods of rapid movement. Moreover, the . . . bursts of movement were not always in the direction of the Promised Land.

While we obviously think that a gradual and continuous version of the *Exodus* migration would be exactly as far-fetched as Dawkins makes it sound, essentially this sort of scenario appears to be accepted by most grammaticalizationists for such phenomena as potentially millennia-long changes from (i) stressed full word to (ii) prosodically weak clitic to (iii) unstressed suffix to

(iv) zero. For instance, Greenberg (1991) traced the development of the Aramaic definite suffix *-a* "over a period of approximately 3000 years" (p. 302). Greenberg himself masterfully divided the overall change involved into a sequence of individual and discrete changes, but the fact remains that many – if not most – grammaticalizationists assert the reality and even the conceptually necessary status of grammaticalization as a virtually indivisible continuum. Still, given the vast timespans over which grammaticalization is often said to occur, as well as the existence of counter-grammaticalizational phenomena – for examples and discussion, see especially Janda (2001: 269 et passim), along with the other papers in Campbell (2001b) – we view it as virtually certain that much of what is now called "grammaticalization" actually displays punctuational tendencies ("fits and starts"). We see no more reason to think that all "morphemes grammaticalize" irreversibly, continuously, gradually, and at a constant rate, across thousands of individuals and hundreds of years – as in Haspelmath's (1998: 344) "gradual unidirectional change . . . turn[ing] . . . lexical items into grammatical items" – than we do to assume that the Israelites of Exodus moved northeasterly toward the Promised Land at a fixed rate of 24 yards per day while traveling through the wilderness. Indeed, it is believing in either of these tall tales that is likely to entrap the gullible in a wilderness of gratuitous assumptions.

In short, then, Dawkins (1986) surely was wrong to assume that no serious scholar in any historical discipline focusing on how fossil-like records reflect speciation-like phenomena over millennia could ever find glacial gradualism (much less seamless continuity) to be worthy of serious consideration as a possible major tempo and mode of change. Rather, the advocates of a yards-per-day account of the Exodus migration, intended by Dawkins as straw-filled caricatures, actually have flesh-and-blood counterparts among grammaticalizationists within diachronic linguistics. Indeed, given the failure of many historical linguists to address the above-mentioned distinction between diachronic correspondences and actual innovations (again recall section 1.2.1 above), it can fairly be said that what Dawkins takes to be the obvious and non-newsworthy core of punctuationism – that is, predominantly gradual real-time transitions between (mostly unpreserved) individual organisms versus periodically abrupt geological-time leaps between preserved fossils bearing on the species level – remains (and most likely will long continue to be) a bone of contention among students of language change.

Admittedly, issues of gradualism/continuity versus punctuationism are ripe for misunderstanding outside of linguistics, as well – both in biology and in other fields. We have already remarked, for example, on Dawkins's tendency to underreport Eldredge, Gould et al.'s focus on entire species, rather than individual organisms, in discussions of punctuated equilibrium. Still, the greatest distortions of the latter concept have occurred on the periphery of biology: that is, in non-physical disciplines which have nonetheless tried to adopt biological metaphors – including, as adumbrated above, linguistics, especially in its diachronic aspect.

1.2.3.5 *Parallels between biological and linguistic evolution: some fruitful, some not*

The irony here, as noted at the start of the previous section, is that the metamorphosing/mutation of punctuated equilibrium in peripheral fields – into variant notions far removed from its original sense in biology – iconically mirrors the very notion of peripatric speciation which provides the foundation for punctuationism. For example, Lightfoot (1999a: 18, 84, 228, 231–2), in devoting considerable discussion to linguistic instantiations, or at least purported analogues, of punctuated equilibrium, omits mention of the species-level focus of Eldredge, Gould, et al., even though his characterization of individual speakers' grammatical reanalyses as "catastrophic changes" (in the technical sense) runs directly counter to the supra-individual, quasi-social emphasis in published explications by biological punctuationists themselves.[81] Indeed, both punctuationists and their critics agree on the crucial role played by migration in accounting for the non-gradual transitions in the fossil record, and, as already discussed above in n. 17, migration is clearly a contact- and group-related social factor – hence arguably a form of spread; cf., for example, Dawkins (1986: 240–1; original emphasis):

> [I]f . . . the "transition" from ancestral . . . to descendant species appears to be abrupt . . . [, the reason may be] simply that, when we look at a series of fossils from any one place, we are probably not looking at an *evolutionary* . . . [but] a *migrational* event, the arrival of a new species from another geographical area . . . [. T]he fossil record . . . is *particularly* imperfect just when it gets interesting, . . . when evolutionary change is taking place . . . [. T]his is partly because evolution usually occurred in a different place from where we find most of our fossils . . . [,] and partly because, even if we were fortunate enough to dig in one of the small outlying areas where most evolutionary change went on, that evolutionary change (though still gradual) occupie[d] . . . such a short time that we . . . [would] need an extra rich fossil record in order to track it.

Paleontology, then – diachronic biology, so to speak – provides essentially no *direct* evidence (as opposed to inferential considerations – so-called "how else?" arguments –) regarding the crucial role of innovating/innovative *individual* organisms in evolutionary change. But is there some way in which synchronic biological studies of rapidly reproducing organisms can perhaps compensate for this lacuna? Again, in principle, yes; in practice, however, no.

It is not difficult to compile a solid list with documented cases of rapid contemporary evolution. We have in mind here more than just instances like Goodfriend and Gould's (1996) demonstration that evolution of shell-ribbing in the Bahamian snail *Cerion rubicundum* occurred via a geologically punctuational "ten-to-twenty-thousand-year transition by hybridization," or Lenski and Travisano's (1994) meticulous recording of increases in average cell-size over 2000 generations of replications (slightly different in each case, despite maximally identical experimental conditions) by each of 12 different populations of the human-gut bacterium *E(scherichia) coli*. Much more convincing to the general public, rather, is the better-known example (cf. Weiner 1995; Grant and Grant 1999, and references there) involving persistent changes – as a response to rapid climatic alterations – in the size and strength of the bills of Darwin's finches on the Galápagos Islands. No less deserving of close attention, though, is the research of Reznick et al. (1977), who traced changes in Trinidadian guppies' maturity rates (and in other reproduction-related behaviors known to be highly heritable) over eleven years, for females, and as little as four years, for males. Losos et al. (1997), on the other hand, were able to document an adaptation of Bahamian lizards' average leg-length (ecologically conditioned according to whether the dominant local flora consisted mainly of trees and other vegetation with thick perching places or of bushes having narrow twigs) over only 20 years. (For further discussion of such studies, see Gould 2000: esp. 334–41ff)

Yet, as Gould (2000: 335) summarizes concisely:

> [B]iologists have documented a veritable glut of . . . rapid and . . . measurable [modern] evolution on timescales of years and decades . . . [, in spite of t]he urban legend . . . that evolution is too slow to document in palpable human lifetimes. . . . [Yet, although t]he . . . truth has affirmed innumerable cases of measurable evolution at this minimal scale – [still,] to be visible at all over so short a span, [such] evolution must be far too rapid (and transient) to serve as the basis for major transformations in geological time . . . – or, "if you can see it all, it's too fast to matter in the long run!".

That is, even if the fast-track evolution among individual creatures which can be currently observed is assumed also to have been characteristic among the prehistoric organisms now preserved only in fossils (even if what we see is what prehistory got, so to speak), the associated rates of change are not slow enough to explain the glacial pace of broad trends in the fossil record. Indeed, says Gould (2000: 344):

> [t]hese measured changes over years and decades are too fast . . . to build the history of life by simple cumulation . . . [. E.g., Reznick et al.'s (1977)] guppy rates range from 3,700 to 45,000 darwins (a . . . metric for evolution, expressed as a change in units of standard deviation – . . . [in particular, as a] measure of variation around the mean value of a trait in a population – per million years). By contrast, rates for major trends in the fossil record generally range from 0.1 to 1.0 darwin[s – so that] . . . the estimated rates . . . for guppies . . . are . . . four to seven orders of magnitude greater than . . . [for] fossil[s] (that is, ten thousand to ten million times faster).

Far from being disappointing, however, this finding actually provides a number of reasons for students of language change – and not just biologists – to be especially content. For one thing, the above-mentioned examples of rapidly trending but not lasting directions of variation present linguists with

a crucial caveat to remember in their diachronic studies. Namely, some variation is stable (occasionally for surprisingly long periods of time – a point that we stress below in section 1.2.3.8, in connection with the age-grading example of a youngster's *Mommy* yielding to an adolescent's *Mom*, and see NICHOLS's chapter 5 regarding other kinds of stability in language over time), so that variants which one encounters for the first time – and thus takes to be innovatory harbingers of future developments – may well be neither recent in origin nor likely to win out in the future. We emphasize this point because of our own experience as speakers of English. After living for an appreciable period of time (into our twenties) without any feeling that much linguistic change was occurring (recall Bynon's 1977: 1, 6 previously quoted suggestion that most speakers are unaware of real changes in language precisely because they are so preoccupied with the social significance of alternative forms that they overlook their correlation with time), we later (especially in our thirties, and increasingly in our forties) became convinced that many diverse trends had just started and were surely proceeding rapidly toward their endpoint, maybe even to be completed during our lifetimes. Yet caution directs us to concede that perhaps very little of the variation which is currently known will survive for very long (even if it outlives us), much less undergo strengthening and expansion across most or all varieties of our native language. Gould (2000: 345) draws a remarkably similar conclusion regarding the rapid but ephemeral biological-evolutionary phenomena here summarized further above, incidentally (but intentionally) implying that their reversibility is largely responsible for the equilibrium (= stasis) part of the punctuational two-step (on this point, cf. also Eldredge 1995: 69–78):

> Most cases like the Trinidadian guppies and Bahamian lizards represent . . . momentary blips and fillips that "flesh out" the rich history of lineages in stasis, not the atoms of substantial and steadily accumulated evolutionary trends. Stasis is a dynamic phenomenon. Small local populations and parts of lineages make short and temporary forays of transient adaptation, but these tiny units almost always die out or get reintroduced into the general pool of the species. . . . [N]ew island populations of lizards . . . , tiny and temporary colonies . . . [,] are almost always extirpated by hurricanes in the long run.

Linguists (of the synchronic as well as the diachronic persuasion) will hear here – for example, in Gould's statement that "Stasis is a dynamic phenomenon" – an echo of Jakobson's (1981: 374) credo that he had, ever "[s]ince . . . [his] earliest report of 1927 to the new . . . Prague Linguistic Circle . . . [*Pražský lingvistický kroužek*,] propounded the idea of permanently dynamic synchrony."

Now, Eldredge (1989: 206–7, 1995: 64–5, 78–85, 1999/2000: 142–3) had in fact already argued that the geographically limited, single-population locus of most evolutionary phenomena plays a major role in promoting stasis – in regard to both "habitat tracking" and the isolation of populations within a species (on these two points, see also Futuyma 1992: 104–7 et passim):

> [B]y far the most common response of species to environmental change is that they move – they change their locus of existence . . . [,] seek[ing] familiar living conditions . . . [,] habitats that are "recognizable" to them based on the adaptations already in place . . . [: t]his is "habitat tracking . . . [", a] constant search . . . , generation after generation, within every species on the face of the earth . . .[. S]pecies tend to change locale . . . [,] rather than anatom[y, as soon as a] . . . suitable habitat can be found . . . [; i.e., they do not] stay put and adapt to new environmental regimes. (Eldredge 1995: 64–5, 78)

> Wright . . . [(1931, 1932, 1982)] gave us the fundamental view of species organization still with us today: species are composed of a series of semi-isolated populations. . . . Species are . . . necessarily disjunct in their distributions, despite the . . . [usually rather] neat line that can be drawn around their entire range of distribution . . . [. Hence] the semi-isolated populations within a given species undergo . . . semi-independent evolutionary histories. . . . Given this . . . organization, it defies credulity that any single species, as a whole, will undergo massive, across-the-board gradual change in any one particular direction. (Eldredge 1995: 82–3)

> [E]ach local population . . . liv[es] . . . in [an] ecosystem . . . with somewhat different physical environments, predators, and prey . . . , with its own sampling of the genetic variation of the entire species, . . . [with a] different mutational histor[y] . . . [and] history of genetic drift . . . and . . . [of] natural selection . . . [. I]t is highly unlikely that natural selection could ever "move" all the populations of an entire species in any one single evolutionary direction for any significant amount of time at all. (Eldredge 1999/2000: 143)

For paleontological data strongly supportive of this view, see now especially Lieberman et al. (1995). But of course all of this only goes to strengthen further the conclusion that the primary mechanism of speciation really is peripatric in nature, thus necessarily involving one or more peripheral, isolated populations.

Using this notion heuristically, we can then further ask whether population-based (i.e., population-constrained) stasis in evolutionary biology has any close analogues in the domain of language change – a question which appears to have a decidedly affirmative answer. As we have already hinted (in n. 75), the most appropriate linguistic equivalent of a biological population (or "deme") would seem to be either a speech-community (cf. here GUY's chapter 8), or – more probably – a social network of interacting speakers; research on the linguistic role of networks has been pioneered by Lesley and James Milroy (cf., e.g., L. Milroy 1980, 1987; L. Milroy and J. Milroy 1992; J. Milroy and L. Milroy 1985; J. Milroy 1992; J. Milroy and L. Milroy 1992) and is here discussed in some detail by WOLFRAM AND SCHILLING-ESTES's chapter 24. Crucially, network studies reveal that, despite the frequent observation (already found in Bloomfield 1933) that language changes tend to start in the most populous and most culturally important urban areas and then to filter down from there to successively less populous cities, towns, and, lastly, rural villages – each time skipping over smaller intervening populations – the prerequisite for such spread

of linguistic innovations is a network structure which includes people with loose ties to many social groups but strong ties to none; that is, a typically urban characteristic. But, in populations with dense, mutiplex social networks involving frequent and prolonged contact among the members of small peer groups across many social contexts, these close ties promote greater resistance to the adoption of linguistic innovations: in short, dense, multiplex social networks promote relatively greater (but by no means absolute) linguistic stasis. It is worth stressing that networks of this sort seem to have been overwhelmingly predominant among humans for essentially all of their prehistory (given that the origin of writing seems roughly to have accompanied the rise of urbanization; cf., e.g., Renfrew and Bahn 2000).

Here – in juxtaposing not human languages and biological species, but instead small, close-knit social networks (to which the Milroys have rightly drawn linguists' attention) and local populations of organisms (the demes on which Sewall Wright helped biologists to focus) – we might initially be tempted to think that we have indeed found a factor which can and does promote punctuated equilibrium in human language(s). At the very least, treating social networks as a crucial element in language change provides a useful corrective for anyone tempted to speak monolithically about changes "in English" (as a whole), or even just "in American English" or "New York City English," since all of these agglomerations not only consist ultimately of individuals but also are highly reticulated. Moreover, it appears accurate to conclude that, when one simply compares all of the dialects (and subdialectal network varieties) of a language, probably the majority of linguistic features which are shared by all varieties represent traits jointly inherited from their common linguistic ancestor, rather than innovations which arose in one variety (or a sprinkling of varieties) but were then eventually diffused from there to *all* other varieties of the language at issue. Individual linguistic networks (and even larger speech-communities and dialects) really can be surprisingly resistant to certain changes.[82] For example, many authors discuss the so-called Great Vowel Shift which marks the transition from later Middle English (ME) to earlier Modern/New English (NE) not only as if it were phonologically uniform (in spite of, e.g., Stockwell and Minkova 1987) but also as if it had affected every dialect of the language. Yet it is well documented in *The Survey of English Dialects* (cf., e.g., Orton 1962; Orton and Halliday 1962, 1963a, 1963b; Kolb 1966; and later atlases) that, in "Northumberland, Cumberland, and Durham . . . [, m]ost of the dialects . . . still have a high back rounded vowel" as the reflex of ME long [u:] in words like *cow, out,* and *mouse* (cf. the summary and related discussion in Janda 1987: 354).

Nor should we forget that, ever since the initial rise of city states in ancient Mesopotamia several millennia ago, urban centers have exercised a continuing magnetic attraction on rural populations that leads to a kind of mobility among humans which strikes us as quantitatively (though perhaps not qualitatively) quite different from the situations of other biological species. For instance, one occasionally hears bandied about, in informal discussions of linguistic change, such statements as the allegation that, "Until 1900, most people in the world never traveled more than 50 miles from their birthplace during their lifetimes" (significantly, we know of no published instantiation of this claim). However, meticulous scholarship by historians like Bailyn (1987: 20–1) has documented findings like the following:

> If . . . one uncontroversial fact . . . has emerged from the . . . decades of research [1955–85] in European social history, it is that the traditional society of early modern Europe was a mobile society – a world in motion. . . . Rich [(1950) had earlier] stressed the relationship between domestic migration and overseas migration . . . [; in addition, h]e found a persistence rate in selected Elizabethan villages over a ten-year period of no more than fifty percent . . . [,] estimat[ing] . . . that only sixteen percent of all Elizabethan families had remained in the same village as long as a century . . . [. Since then], the picture has been greatly elaborated . . . by local historians . . . [and by] historical geographers. . . . We now know . . . that the English population['s] . . . mobil[ity] . . . was a composite of three closely interwoven patterns [= with movements locally over short distances, regionally over longer distance, and London-ward over variable distances].

Moreover, quite apart from the fact that Milroy(i)an (at their finest, Milroyal) network studies have stressed the importance, alongside denser groups, of looser-knit social groupings – which tend to counteract static equilibrium in language – even biologists have been quick to point out that (most of) language and other aspects of human culture are transmitted across time (and space) via non-genetic mechanisms which endow linguistic and other cultural "evolution" with a decidedly non-biological character. On this point, there is complete accord even between "ultra-Darwinians" (cf., e.g., Eldredge 1995: 4), on the one hand, and punctuationists like Eldredge and Gould, on the other hand. Dawkins's (1986) take on the relevant differences-within-similaries is as follows:

> Darwin['s] . . . successors have been tempted to see evolution in everything, . . . [even] in fashions in skirt lengths. Sometimes such analogies can be immensely fruitful, but it is easy to push . . . [them] too far. . . . The trick is to strike a balance between too much indiscriminate analogizing . . . and a sterile blindness to fruitful analogies. (p. 195)

> [I]n human cultural evolution . . . , choice by whim matters . . . [, although c]ultural evolution is not really evolution at all . . . [,] if we are being fussy and purist about our use of words . . . [. Still, i]t has frequently been pointed out . . . that there is something quasi-evolutionary about many aspects of human history. If you sample a particular aspect of human life at regular intervals, . . . of one century or perhaps one decade, you will find . . . true trends . . . , without [all of] these . . . being, in any obvious sense, improvements. Languages clearly evolve in that they show trends . . . [;] they diverge, and . . . [,] as the centuries go by after their divergence . . . [,] they become more and more mutually unintelligible. (pp. 216–17)

Gould (1991: 63–5), for his part, has been even more explicit about the true nature of the parallels under consideration – and, unlike Dawkins, he does not fail to mention the important additional role played by such convergence-promoting phenomena of direct cultural contact as borrowing:

> [C]omparisons between biological evolution and human cultural or technological change have done vastly more harm than good – and examples abound of this most common of all intellectual traps. Biological evolution is a bad analogue for cultural change because the two are . . . different . . . for three major reasons that could hardly be more fundamental. . . . First, cultural evolution can be faster by orders of magnitude than biological change at its maximal Darwinian rate – and . . . timing . . . [is] of the essence in evolutionary arguments. Second, cultural evolution is direct and Lamarckian in form: . . . [t]he achievements of one generation are passed . . . directly to descendants, thus producing the great potential speed of cultural change. Biological evolution is indirect and Darwinian . . . [:] favorable traits do not descend to the next generation unless, by good fortune, they arise as products of genetic change. Third, the basic topologies of biological and cultural change are completely different. Biological evolution is a system of constant divergence without subsequent joining of branches. In human history, transmission across lineages is, perhaps, the major source of cultural change. Europeans learned about corn and potatoes from Native Americans and gave them smallpox in return.

These considerations, though, do not ineluctably obligate us to believe that episodes of language change should be primarily brief and abrupt, rather than continuous and gradual, and they certainly do not appear to favor stasis over innovation(s). On these grounds alone, we are surely justified in concluding that (based on the present sifting of diverse available evidence) a maximally close analogue of punctuated evolution in biology has not so far been established as the *general* case within the set of phenomena often referred to as linguistic evolution. Yet this conclusion is actually not very different from the situation in biology, where it turns out that the most illuminating question to ask is no longer "Does punctuated equilibrium exist?" (since yes, it does), or "Does the evolution of *all* species seem to be punctuational in nature?" (since no, although this is true for many species), but instead "Which aspects of the evolution of which species appear to be punctuational in nature?"[83]

Thus, linguists can most assuredly profit – and profit the most – from investigating which particular aspects of which specific languages subject to which external circumstances seem to have undergone the most rapid changes or to have shown the longest periods of stasis – this last notion more often being referred to by linguists as "stability." That a solid start and some progress along these lines has already been made is demonstrated by a growing body of research that includes such pioneering studies as Fodor (1965) and Mithun (1984). Mithun, for instance, compared "functionally comparable but formally different devices" across six Northern Iroquoian languages and, on that basis, suggested (pp. 330–1) that morphosyntax is more stable than the lexicon, with syntax being functionally more stable than morphology and (within the lexicon) predicates being more stable than particles. The "hierarchy of stability across these . . . interlocking domains" therefore seems to be, "in order of increasing volatility," as follows: syntax, morphology, predicates, particles. (Janda 2001: 310–11n.14 observes that these differential rates of stability versus change render even more implausible the claim of some grammaticalizationists – recall the discussion in the previous section – that a single linguistic element undergoing successive reanalyses across several linguistic domains must always display a constant grammaticalization rate.) More recently, Nichols (1992a and many subsequent works) has devoted particularly close attention to the differential stability of different linguistic elements; NICHOLS's chapter 5 here thus discusses in considerable detail what is presently known about this topic, likewise providing extensive references.

As a general methodological point, it is worth emphasizing at this juncture how much more revealing it is – both in historical linguistics and in evolutionary biology – to adopt the divide-and-conquer strategy of posing many local questions regarding some possibly large-scale trend, rather than making one global query. We have just mentioned the benefits that linguists like Mithun and Nichols have derived from asking numerous small questions (here concerning differential rates of stability across components and units of grammar; cf. also Joseph and Janda 1988: 205–6 (n. 12) and Janda et al. 1994 on the statistical predominance of "local generalizations" over more global ones), but there exists a striking biological analogue to this. Although the particular suggestion by Stebbins (1982) that we have in mind was made in an introductory textbook intended for laypeople, and although it was superseded by more technical later treatments of the relevant phenomena, the fact remains that the analytical tack adopted by Stebbins toward the start of the debate over punctuationism was indeed prescient, being far more productive than the winner-take-all tug-of-war which tended to dominate the time of his writing.

In particular, Stebbins (1982) decided to address punctuated equilibrium in connection with a response to the *Alice-in-Wonderland*-inspired "Red Queen" hypothesis of Van Valen (1973) and others, so named because it has to with active evolutionary "running" just in order to "stay in the same place" (cf. also Stanley and Yang 1982 on so-called "zigzag evolution" – e.g., in clams). Observing that some living animals and plants look very much like their ancient fossil ancestors, despite "constant changes . . . [in] internal, largely biochemical characteristics" that cannot be detected from fossils, Stebbins (pp. 20–1) argued that, at least for these, the Red Queen hypothesis may be valid. He highlighted, for example, the "evolutionary constancy" of small, secretive, or sedentary animals like shrews, oysters, jellyfishes, cockroaches, scorpions, and many kinds of worms, which already have met successfully "all the environmental challenges . . . of scores or hundreds of million years."[84] These, he contrasted with such living things as song birds and mice ("small, highly active creatures") or large carnivores (lions, birds of prey, etc.), for all of whom environmental challenges (e.g., "new and different predators"

for the former, "elusiveness of their prey" for the latter) have continually motivated adaptations whose effects are highly visible in the fossil record. This kind of correlation had not gone unnoticed before, but Stebbins rightly connected it with the punctuationism debate.

In addition, however, Stebbins (1982: 138–9) cited previous research by Wilson et al. (1974) and King and Wilson (1975) – cf. also, (later) Wilson et al. (1987) – suggesting that the same kind of differentiated evolutionary rates may be more directly detectable at the level of individual genes, especially those which code for cellular proteins; this might lead one to conclude that genes coding for cellular proteins "often and perhaps always" evolve at different rates from those that determine overall body plan, including anatomical structure:

> [C]himpanzees . . . [and] humans . . . [show very] strong resemblances between cellular proteins . . . in spite of large . . . differences in external anatomy. Among frogs, pairs of species . . . almost identical in overall body plan and anatomy nevertheless are far more different from each other with respect to cellular proteins than are apes from humans . . . [. T]here [may be] something about their overall genetic constitution that makes mammals more susceptible to changes in anatomy . . . [, whereas] frogs [are] more susceptible to changes in cellular proteins.

However, Stebbins (1982: 139) argued that such reasoning need not point directly to the sort of punctuationism in which a successful response to a challenge can be made relatively quickly – "in a few thousand generations, by anatomical changes" – after which evolution "may proceed very slowly until the population faces another environmental challenge." Still, on the other hand, many environmental challenges may exert what amounts to "only low to moderate selective pressures on cellular proteins," a fact that is well known from comparisons between humans and chimpanzees. Therefore, suggests Stebbins (1982: 139), evolutionary changes in these molecules could continue slowly for long periods of time, and so it is possible that:

> evolution of anatomical structure and function often proceeds . . . punctua[lly] . . . , while evolution of most cellular enzymes proceeds more gradually . . . , with the combined] result . . . be[ing] a hare and tortoise pattern. . . . [I]n a young group, newly evolved lines would differ more from each other with respect to anatomy and outward form than with respect to enzymes . . . [; i]n an old group, the reverse would be the case. . . . This explanation agrees with observations. Mammals are relatively young . . . [, having] diversified rapidly between 50 . . . and 60 million years ago . . . [, whereas f]rogs . . . acquired their present body plan more than 200 millions years ago.

Here again, we would stress that the main import for historical linguists of such earlier ruminations by a biologist like Stebbins (1982) is that they show the advantages to be gained by studying rate of change not globally but componentially, with attention paid simultaneously to various entities on multiple levels involving different relative dimensions of focus (recall, too, the above-mentioned start made in this direction by linguistic diachronicians like Fodor, Mithun, and Nichols). Stebbins's lead was, in turn, borne out by the later and much more broadly based conclusions of Hunter et al. (1988), whose broad survey of recent research suggested that stasis occurs more often in such macroscopic fossils as marine arthropods, bivalves, corals, and bryozoans, while gradualist patterns tend to predominate in foraminifera, radiolarians, and other microscopic marine forms (for a brief survey of these and most other forms of life, see Tudge 2000, plus references there).

We thus conclude that, given the uncertainties which currently reign among evolutionists as to precisely what (non-zero) number and which varieties of taxa (taxonomic groupings of various sizes) are associated with stasis-cum-punctuationism versus gradualism, students of language change should not feel undue concern over the fact that the relative roles and frequency of sudden versus gradual change have not yet been satisfactorily determined in linguistics, either. While this may gladden those linguists who assume that historical research on language and on biology necessarily should (nearly) always yield parallel results, such is not at all our reading of the situation. Our belief, rather, is that uncertainties in another field which is often attended to by one's own specialty can be useful in suggesting that external disciplines are actually most helpful if scouted out heuristically – as available sources for borrowing (or generating) novel hypotheses and other ideas – rather than taken as models for emulation. The danger in the latter case, of course, is that too close a shadowing of another field can tempt scholars to interpret ambiguous cases (and even to nudge their unambiguous results) in the direction which the relevant other discipline would lead one to expect, and the consequences of this strategy can be particularly grave if the model field in question is subject to dramatic or rapid changes in its dominant orientation(s). In the case of language and biology, then, there can be no harm in diachronicians' treating punctuational change, stasis, and gradual change as if those notions had been proposed wholly within linguistics and just accidentally happen to have extradisciplinary counterparts.

Even while saying this, we do not wish to downplay too much the productive interpenetrations and suggestive resemblances that already characterize the relationship between historical linguists and evolutionists. For example, Platnick and Cameron (1977) is an interdisciplinary study of cladistic methods in three domains – linguistics, textual studies, and phylogenetic analysis by evolutionists – and is in fact a collaboratively biologist+linguist-authored article that appeared in the journal *Systematic Zoology*. Harvey and Pagel's (1991) treatment of *The Comparative Method in Evolutionary Biology* is also of considerable potential interest to diachronicians of language (although it tends to bug linguists who read all of its pages, since the book makes essentially no reference to the substantial existence of a comparative method in historical linguistics). And the set of several papers collected in Nerlich (1989), despite its focus mainly on evolution in the sense of language change, does make some connections with evolutionary biology. On the other hand, there are even some publications of a wholly (or at least primarily) biological nature which still provide sobering suggestions for those linguists who are perhaps somewhat too mesmerized by genetics and, in particular, by recent genomic research.

Marks (2000), for example, presents a reaction to such frequently bandied-about facts as the finding that "geneticists have been able to determine with precision that humans and chimpanzees are 98 percent identical genetically" – which could even lead some diachronicians (as well as synchronicians) of language to suggest that studies of chimpanzee communication (whether in the wild or in captivity) might throw a directly useful light on human linguistic abilities. Instead, Marks suggests, we would do better to confess (and confront) our unfamiliarity with genetic comparisons. It is this ignorance which leads us to overlook the fact that, since DNA is a linear array of four bases, there exist only four possibilities as to what base will occur at any specific point in a DNA sequence, and therefore "[t]he laws of chance tell us that two random sequences from species that have no ancestry in common will match at about one in every four sites." Thus, even two unrelated DNA sequences will be 25 percent identical, and this fact has implications not only for comparisons between two kinds of animals, but also for comparisons between animals and plants, since "all multicellular life is related . . . and . . . shares a remote common ancestry." Taking this information and running with it, Marks concludes that:

> if we compare any particular DNA sequence in a human and a banana, the sequence would have to be more than 25 percent identical. For the sake of argument, let's say 35 percent. In other words, your DNA is over one-third the same as a banana's. Yet, of course, there are few ways other than genetically in which a human could be shown to be one-third identical to a banana.

In light of these background considerations, we doubt whether (m)any linguists, historically minded or not, would find much appeal in the prospect of devoting, say, 25 percent of their time to studying the communicative abilities of bananas. Sometimes, it appears, we simply have to let biology be itself. And, actually, an exhortation along these lines has already been issued to us by the often-quoted last sentence of Voltaire's (1759: 86) *Candide*: "Mais il faut cultiver notre jardin" – which (cf. Wootton 2000: xliii, 135) is in fact best translated as "But we have to work our land(s)" or "cultivate our field(s)."[65] That is, protagonist Candide's last(-mentioned) piece of advice is significantly *not* "Il faut cultiver le jardin d'autrui" and especially not "Il faut que quelqu'un d'autre cultive notre jardin," which would respectively mean "We have to cultivate somebody else's field(s)" and "Somebody else has to cultivate our field(s)/land(s)." Of course, historical linguists' labor need not be pure, in the sense that they can profitably crib hints from watching how biologists work in their own field and then apply such inspiration to the field of linguistic change. But, still (with apologies to Bernstein et al. 1955), it would be wise as well as good – and certainly for the best, we know – if diachronicians of language would both thoroughly observe biological practice and also do all the necessary linguistic spadework before they attempt to implant any fruits of evolutionists' thinking within linguistic accounts which deal with specific language changes. Not every garden-variety outgrowth of recent developments in the field of biology can find an insightful application in the work of historical linguists. Before concluding this section, therefore, we take a brief look at a (somewhat indirectly) punctuated-equilibrium-related concept which (i) has been borrowed from biology by certain linguists and used in one kind of research on language change but (ii) has not yet been shown to provide a more satisfactory account than certain other biological or even linguistic concepts would have done.

The evolutionary notion known as the *founder principle* (or *effect*) was adopted by Mufwene (1996) from biology – he cites only Harrison et al. 1988 (*Human Biology: An Introduction to Human Evolution, Variation, Growth, and Adaptation*) – and applied by him to those arenas of linguistic change connected with the study of creole languages. Mufwene's goal thereby was to "analogize 'language' to 'population' in population genetics,"[86] thereby "hoping to account more adequately for some aspects of language restructuring . . . in contact situations, especially those associated with the varieties called 'creoles'" (pp. 83–4). The relevance of the founder principle and of founder populations to the above goal was that these concepts allegedly help to explain "how structural features of creoles have been predetermined to a large extent (but not exclusively!) by characteristics of the vernaculars spoken by the populations that founded the colonies in which they developed." That is, since European colonies often began with large numbers of indentured servants and other low-status employees of colonial companies, the presence of so many speakers of non-standard varieties of the creoles' European lexifier-languages can be invoked in order to explain "the 17th and 18th-century non-standard origin of several features of creoles." The specific relevance of the founder principle emerges more directly when Mufwene states his assumption that "some features which might be considered disadvantageous . . . in the metropolitan varieties of the European lexifier-languages" – "because they are rare, not dominant, and/or used by a minority" – "may well have become advantageous in the speech of the colonies' founder populations." One such example proposed by Mufwene (1966) involves the presence of locative-progressive constructions like *be up(on)* V-*ing* in earlier varieties of English (reflexes of which are still found today, in some non-standard varieties, as *be a-*V-*in'*).

Mufwene (1996: 84–5) focuses as follows on certain additional ways in which, he claims, the biological founder principle bears on the genesis of creoles (for that author's more recent views, see Mufwene 2001 (*The Ecology of Language Evolution*), which manifestly also uses a certain amount of biologically oriented terminology):

> [T]ypical population-genetics . . . explanations for the dominance of . . . disadvantageous features in a (colony's) population are: 1) such features may have been reintroduced by mutation; 2) they may have been favored by new ecological conditions in the colony . . . [,] or 3) the colony may have received significant proportions of carriers of the features/genes, a situation which maximized the chances for their successful reproduction . . . [. I]n creole genesis[. . . ,] the 2nd and 3rd reasons account largely for the restructuring of the lexifier [in/as the creole]. True mutations are rare, though there are plenty of adaptations . . . [. T]he developments of creoles a[re] . . . instances of natural adaptations of languages qua populations to changing ecological conditions. In every colony, selection of the lexifier for large-scale communication in an ethnographic ecology that differed from the metropolitan setting called for the adaptations that resulted in a new language variety.

At this point, we should hasten to state that there clearly are at least superficial similarities between the biological founder principle (for which we quote biologists' definitions further below) and certain linguistic situations. NICHOLS's chapter 5 (in its section 4.2), for example, discusses in some detail a geographical distribution whereby two "low-viability features" (numeral classifiers and verb–subject word order) having no obvious grammatical interconnections are associated with each other in a large group of Pacific Rim languages spoken in the far western Americas – from which Nichols concludes that this association "must reflect the . . . two features' accidental cooccurrence in their ancestral language or population," and that the latter was once a "small colonizing population."

One crucial aspect of founder effects – which, not surprisingly, are invariably due to the founder principle – is thus that a small, isolated founding population is always involved. This is reflected, for example, by the summary of Mayr's (1954) original treatment of the principle in his later (1982) survey of *The Evolution of Biological Thought*. In particular, because he was "aware of the frequency of founder populations beyond the periphery of the solid species range," Mayr (1954) "finally" saw that founder populations "would be the ideal place for a drastic genetic reorganization of the gene pool in the absence of any noticeable gene flow and under conditions of a more or less strikingly different physical and biotic environment" (Mayr 1982: 602). In this regard, it is indeed generally agreed by biologists that the founder principle per se (as opposed to the interacting factor of gene flow) was initially proposed and most strongly advocated by Mayr, and this is indicated by frequent references in the literature to "Mayr's founder principle," as in Ereshefsky (1992: 89, 95). (Hence Mufwene's (1996) failure to mention Mayr at all must simply be an oversight.) However, it is less than clear that those linguistic phenomena which are described as founder effects always involve direct analogs of their alleged biological counterparts.

Perhaps most striking is the disparity between, on the one hand, Labov's (1972a, 1994–2001) defense of unmonitored, casual-style, working-class speech as essentially least marked and, on the other hand, Mufwene's above-mentioned (1996) assertion that the features spread in creoles due to the founder principle might be considered disadvantageous in the metropolitan varieties of the European lexifier-languages "because they are rare, not dominant, and/or used by a minority." Here, on the contrary, it would appear that, aside from the problem of quite probably lacking (overt) prestige, the linguistic features in question would most likely be both frequent and dominant – due to their occurrence in unmonitored, casual-style, natural speech – and it further appears that, as features of working-class speech, such features would not in fact be used by a minority, either, but by a majority or at least a plurality. All of this begins to make Mufwene's (1996) proposed analogy between the genetically governed biological founder principle and its putative linguistic counterpart look much more tenuous; indeed, the relevant linguistic phenomena now increasingly start to sound much more like cultural-behavioral issues. Yet this seems to be consonant with Labov's very recent (2001: 503–4) characterization of the linguistic founder effect in terms of a kind of gatekeeper function:

> The doctrine of first effective settlement . . . [– cf.] Zelinsky 1992 . . . [–] limits the influence of new groups entering an established community . . . [by] asserting that the original group determines the cultural pattern for those to follow, even if these newcomers are many times the number of the original settlers. This is consistent with the fact that New York City, Philadelphia, Boston, and Chicago, cities largely composed of 19th-century immigrants from Europe, show only slight influences from the languages of these ethnic groups in the form of the local dialect . . . [. Only if, i]n any one generation, . . . the numbers of immigrants rise to a higher order of magnitude than the extant population . . . [can] the doctrine . . . be overthrown, with qualitative changes in the general speech pattern. (pp. 503–4)

Moreover, Labov also observes that this principle did not originate in the 1990s, but was in fact "independently formulated . . . in Creole studies . . . by Sankoff (1980) as the 'first past the post' principle."

Yet there is one final observation of a biological nature to be made here, and this is that, since the linguistic data presently being considered come from a creole language, we should at least briefly reconsider Thomason and Kaufman's (1988) view that abrupt creolization involves "shift without normal transmission" (for her more recent, solo views, cf. THOMASON's chapter 23 here). And this should in turn lead us at least to consider the possibility that an equally good or perhaps even better biological analog (than the founder principle) might be involved: namely, hybridization (cf., e.g., a classic paper like Anderson and Stebbins's 1954 discussion of "Hybridization as an evolutionary stimulus" and compare Trudgill 1996 on "dual-source pidgins"). On the other hand, though, hybridization is not inherently linked with punctuated-equilibrium phenomena in the way that the biological founder principle is; Mayr (1997: 183), for example, directly states that, "[i]n peripatric speciation, a founder population is established beyond the periphery of the previous species' range,"

and we know that peripatric speciation seems to be firmly linked with punctuationism. As a result, a decision to abandon biological-founder-principle explanations in favor of biological-hybridization-principle explanations would force us to end our discussions of punctuated equilibrium sooner rather than – as here and now – later.

While this excursion into paleobiology admittedly has not done full justice to the huge specialized literature on punctuationism in the several relevant subfields of biology, it does suffice to show the dangers of glibly importing technical terminology whose specific senses in specialist (i.e., non-linguistic) parlance display, not surprisingly, exactly the number and kind of arbitrary semantic accretions that linguists should expect. If interpreted extremely broadly, as throughout Dixon's (1997) monograph, or in the brief statement by Labov (1994: 24) quoted above ("catastrophic events . . . play . . . a major role in the history of all languages"; recall also Lass 1997: 304), a punctuated-equilibrium approach to language change seems to have much going for it. That is, it does appear that major structural changes in the phonology or morphosyntax of a language are not a yearly or even a centennial occurrence. Observation over time thus tends to reveal a kind of stasis in what could be called the skeleton and organs of a language which most often are relatively unaffected by the constant but minor semantic and other lexical innovations in the covering flesh and skin. But there are linguistic analyses which invoke punctuationism for the sole purpose of justifying accounts expressed in terms of "catastrophes," where a given change occurs (in toto) via one individual speaker's grammatical reanalysis across adjacent generations – even though this approach ignores the crucial limitation of biologists' punctuations to changes taking place in geological time – that is (to repeat), ones occurring over thousands and tens of thousands of years. (Recall that, as Gould 2000: 340 puts it, "even ten thousand years represents a geological eye-blink in the fullness of evolutionary time.") This kind of error, since it arises from misinterpreting one chronological scale of measurement as if it were another temporal yardstick, is thus reminiscent of the 1999 immolation, in the Martian atmosphere, of the multimillion dollar *Climate Orbiter* space probe, which burned up (after months of successful space travel) due to an interpretive mix-up involving the unnoticed combination of Anglo-American and metric units of measurement in the calculation of its trajectory.

Lexical borrowing is certainly familiar to historical linguists (and cf., again, THOMASON's chapter 23 herein), but, rather than just borrowing terms with conceptually suggestive names and then essentially guessing what the meaning of a certain item is "in biology," diachronicians have much to gain from actually reading a variety of biologists' competing views on the relevant topics (cf. the numerous references listed above, plus the synoptic surveys provided by such collections as Sober 1994; Ridley 1997; Hull and Ruse 1998). Those who do, we are convinced, will find that, while the notion of punctuated equilibrium has linguistic analogs, it most assuredly does not motivate the exclusionary focus on individual speakers advocated by so many diachronic and synchronic linguists. While all biologists indeed uniformly recognize that there is a crucial individual side in phylogenetic evolution (especially speciation), as well as in ontogenetic development, they are, on the whole, much more rarely subject to temporary amnesia concerning the importance of interactions within and between ecologically defined groups than linguists seem to be. The proper balance between group focus and individual focus has been well expressed in Mayr's many discussions of "population thinking" (cf., e.g., 1997: 310 et passim, plus references there), which takes biological populations and larger natural groupings (like species) seriously – but at the same time "emphasizes the uniqueness of every individual in populations . . . [,] and therefore the[ir] real variability."

While individuals are not all there is, the fact remains that even groups of people are indeed made up of discontinuous entities, and so we have reason to return, in the next two sections, to the issue of discontinuity between individuals as it relates to matters of change (here, in language) – a topic which was a particular favorite of the distinguished evolutionary biologist Dobzhansky (cf., e.g., 1937: 4–5 ("Discontinuity") et passim, 1970: 19–24 ("The Discontinuity of Individuals" and "The Discontinuity of Arrays of Individuals").

1.2.3.6 *Discontinuity of language transmission even in what "doesn't change"*

Most scholars who study linguistic change would surely agree with Kiparsky (1968: 175) that "a language is not some gradually and imperceptibly changing object which smoothly floats through time and space, as historical linguistics based on philological material all too easily suggests" (e.g., recall the still deceptively well-preserved book from 1775 discussed here in n. 28). Rather, "the transmission of language is discontinuous," as Kiparsky himself had already stressed earlier (cf. 1965: I.4, II.12–13); see, too, the later, similar phrasing of Lightfoot (1979: 148, 1981: 212). In generative grammar, this view was apparently first expressed by Halle (1962: 64–5). But Halle also mentioned several illustrious predecessors – including figures like von Humboldt (1836), Paul (1880), Herzog (1904: 57ff), and Meillet (1904–5, 1929) – who had held similar views long before him. Halle, in turn, reported that Meillet's work had first been brought to his attention by Edward S. Klima, who soon pursued a similar approach in Klima (1964, 1965), while Kiparsky acknowledged the influence of unpublished prior statements by G. Hubert Matthews and Paul Postal (the latter's views later appearing in print as Postal 1968: 269–81, 308–9).

As for Meillet, there is great irony in the fact that, despite the frequency of observations (e.g., here in HEINE's chapter 18 and many references there) that twentieth-century grammaticalization studies began with Meillet (1912), there is virtually no mention in the diachronic-linguistic literature of the great French scholar's very clear views (quoted by Halle 1962: 64n.9–66n.11) regarding the cross-generational discontinuity of language transmission. A substantial (and earlier) statement concerning this topic can be found in Meillet (1904–5: 6–7):

> One must keep in mind from the very start the essentially discontinuous character of the transmission of language. . . . This discontinuity . . . would not in itself suffice to explain anything, but, without it, all the causes of change would without a doubt be powerless to transform the meaning of words as radically as has happened in a large number of cases . . .[. I]n a general way, moreover, the discontinuity of transmission is the prime condition which determines the possibility and the modalities of all linguistic changes.

Elsewhere (1929: 74–5), Meillet describes language as being transmitted through being "recreated by each child on the basis of the speech data it hears." These are Meillet's own words (in translation), but they have been put to various different uses by later writers. For a critical analysis of the generative (re)interpretation imposed by Halle (1962) on his French forerunner, see Baron (1977: 28–34, 47n.11–48n.15).

At least as memorable as Meillet's prose statements on transmissional discontinuity in language, though, are the schematic diagrams later provided first by Klima (1965: 83), then – slightly revising the original – by King (1969: 85), next – again with revisions – by Andersen (1973: 767, 778; cf. also 1990: 13), and lastly – in its most complex form – by Traugott (1973a: 41–5, 1973b: 316–17). See Janda (2001: 274–5) for a discussion that lists not only later, similar diagrams but also many prose discussions implying them.

Unfortunately, many scholars' acceptance of these particular discontinuity-emphasizing diagrams as a general type seems to have been seriously compromised because they embody – or even just because they have been associated with – certain questionable but much less central generativist claims regarding diachrony. Among these secondary aspects, whose objectionableness has been especially harmful in overshadowing the core notion that language is transmitted discontinuously, are the following implications: (i) that children are the primary instigators of linguistic change (via simplification), (ii) that children acquire language mainly from an older generation (whose additions complicate grammar), and (iii) that speakers have only a single, variation-free grammar. Based on numerous actual past misunderstandings of discontinuity claims and graphics, we wish to forestall possible future misinterpretations by explicitly emphasizing – and in the strongest possible terms – that we ourselves categorically reject all three of the above assertions. Hence figure I.5 is likewise intended to imply rejection of these claims, and so we present it as a significantly revised and updated version of diagrams dating from the mid-to-late 1960s and early 1970s (originally derived from Halle, Matthews, Postal, and Kiparsky) that were evolved by Klima, King, Andersen, and Traugott; the diagram reproduced here thus presents the considerably revised version developed by Janda (2001: 277).

In figure I.5, the major focus is on the idiolect of one particular speaker/hearer, here labeled individual C – with an analogous situation understood as holding for any given signer-viewer – but the various pairs of ellipses signal the existence of additional relevant generations besides N–1, N, and N+1, and of individuals beyond A, B, and C within them. Other individuals than C

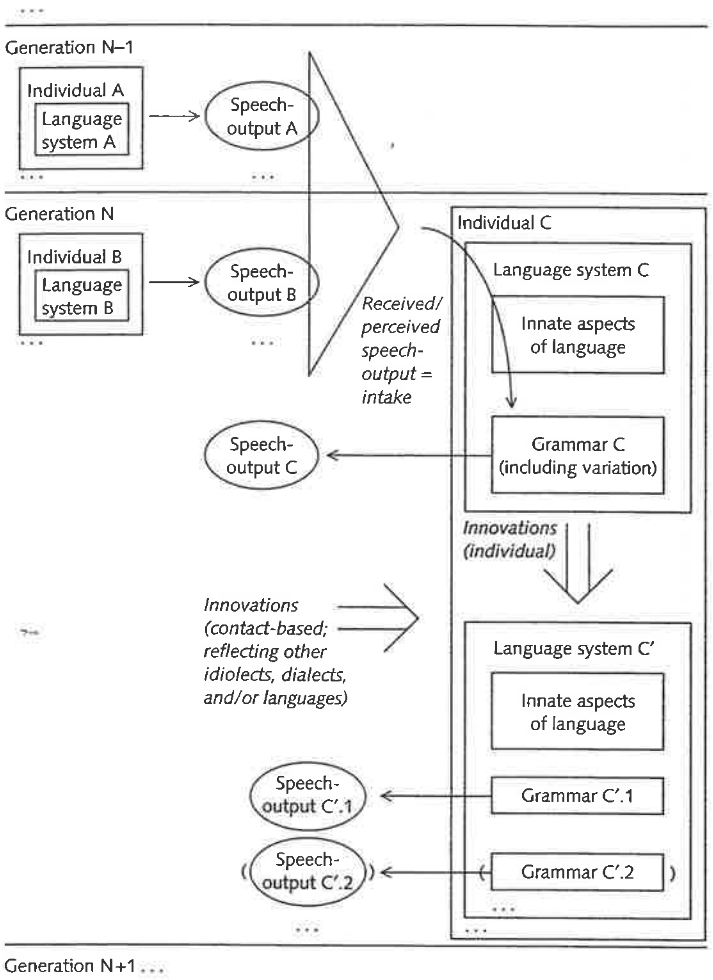

Figure I.5 The discontinuous transmission of language and its relation to change: a revised schema
Source: Janda (2001: 277), after Klima (1965); King (1969); Andersen (1973); Traugott (1973a, 1973b)

clearly also have both (i) innate aspects of language – a.k.a. a(n) LAD (Language Acquisition Device) or UG (Universal Grammar) – and (ii) an acquired grammar, but these have here been collapsed as language systems A and B, etc. The large arrowhead-like triangle intersecting speech-outputs A and B shows not only that the speech of more than one individual (and generation)

is relevant for both language acquisition and language change, but also that no one ever hears the entire speech-output of anyone else, and that what is physically heard is subject to interpretation. That is, there is a difference between input and intake, as stressed for second-language acquisition by researchers like first Corder (1967: 165) and then Chaudron (1985), Zobl (1985), and several other authors in Gass and Madden (1985).

Within individual C, there are two temporally sequenced language states, an earlier (or even earliest) state C and a later (or even latest) state C′; the former is altered into the latter as the result of innovations which sometimes are internally individual (perhaps partly maturational) but more often are contact-based (and so can involve both intended accommodation and unintended hypercorrection). Language system C′ also allows for the parenthesized option of a second grammar C′.2 (and, as suggested by the ellipsis, allows for additional other grammars) besides C′.1, this in connection with diglossic situations (cf. originally Ferguson 1959) where sets of linguistic features vary in tandem and so justify simultaneous multiple grammars (cf., more recently, Kroch 1989a; Lightfoot 1991: 136–40). In addition, though, all of the grammars in the above schema should be interpreted as including variation, some of which may best be treated in terms of variable rules (cf., e.g., Labov 1972, 1994) and/or in terms of competing alternative constructions or multiple analyses (cf., e.g., Fillmore et al. 1988; Harris and Campbell 1995: 51, 59, 70–2, 81–9, 113, 310–12).

As its eclectic and general nature suggests, the graphic figure I.5 is intended to be specific only about those aspects of language transmission and linguistic change regarding which relative certainty or at least consensus can be assumed; the details have been either omitted or only vaguely hinted at for matters concerning which there exists significant disagreement or substantial doubt. Thus, for example, the absence of precise age-related information regarding the language systems of C and C′ in individual C at various stages allows for some influence of a (rather than *the*) child on language change, but without forcing us to view childhood as the primary chronological locus of linguistic innovations (for discussion, cf. Aitchison 1981, quoted from 2001: 201–10, 216; especially Romaine 1989). In light of the still-controversial nature of generations, both as idealized constructs and as agents in models of language acquisition, it seems best to follow the suggestions of Manly (1930) and – more recently – Weinreich et al. (1968):

> [T]here ha[s] . . . been a curious failure on the part of scholars to recognize, or perhaps rather to emphasize, what actually occurs in the transmission of a language from generation to generation. The actual facts are, of course, known to everyone. . . . There is no such thing in reality as a succession of generations. Yet scholars constantly write as if there were. The community is renewed and continued, not by successive generations, but by a constant stream of births. This fact is of importance in all questions concerning the transmission of human culture. It is of supreme importance in the history of human speech. . . . [E]ach and every child, during the formative period of . . . speech, is more closely and intimately associated with children slightly older than . . . [him/herself] than with adults . . .

and is psychologically more receptive of influence from these children than from adults. (Manly 1930: 288–9)

> [T]here is a mounting body of evidence that the language of each child is continually being restructured during his[or her] preadolescent years on the model of his [or her] peer group. Current studies of preadolescent peer groups show that the child normally acquires his [or her] particular dialect pattern, including recent changes, from children only slightly older than himself [or herself]. (Weinreich et al. 1968: 145)

All of these authors, it should be noted, make prominent reference to the fact that the transmission of language is both temporally and spatially trans-individual, and hence also discontinuous in an important sense. On the other hand, it bears repeating (recall sections 1.2.3.1–1.2.3.3 above) that, although the discontinuous transmission of language plays a role in the introduction and propagation of linguistic innovations, even aspects of a language which are acquired by a speaker in a form unchanged from that used by an older generation are passed on and picked up via (or despite) transmissional discontinuity. It is thus the case that, as we have already observed previously, the more challenging fact about linguistic change is not how much of language changes in a short time, but instead how relatively little of it undergoes rapid alteration (cf., e.g., Nichols 1992a; NICHOLS's chapter 5 here) – a situation whose suggestive parallels with biological evolution were discussed in the preceding section (which cited such works as, e.g., Eldredge 1991: 44–47). For further references and discussion, see also Janda (2001: 310–11n.14).

Since figure I.5 above directly connects the discontinuity of language transmission with individual speakers, a further word on individuals vis-à-vis speech communities is in order here. We have already cited Labov (1994: 45n.2) as viewing a "language as a property of the speech community" and "preferring to avoid a focus on the individual, since the language has not in effect changed unless the change is accepted as part of the language by other speakers." Still, it remains the case that, since grammars are properties of individual brains, whereas a community has no (single) brain, there can be no such thing as a "community grammar" except as a linguist's construct. Instead, rephrasing Labov's observation, we can conclude that a given linguistic innovation is potentially more revealing to the extent that it comes to characterize many individuals' grammars. In this regard, it is significant that Labov (1997) has made a start toward a synthesis of views by focusing on those specific – influential – individuals who are most likely to spread linguistic innovations to groups of other individuals, and hence eventually to an entire speech-community. In addition, Labov (1999) has recently discussed the individual "outliers" (quantitatively anomalous speakers) who are so frequently encountered in variationist studies. For more discussion, see again the work of James Milroy (e.g., 1993: 223), to whom is due the extremely useful distinction – whose wider adoption we have already advocated above in section 1.2.1 – between an *innovation* (which may be made by an individual speaker) and a

change (which is a community's increasing adoption of some innovation); this trend has been continued and elaborated by Milroy (1999), among others.

1.2.3.7 *Discontinuity of individual grammars and the last rites of linguistic organicism*

Although all linguists must, at some level, be aware that it is speakers who act in and on language – and not linguistic units that act in and on speakers – one can nevertheless find statements like the following, which comes from Pagliuca's (1994: ix) introduction to a collection of papers all on the topic of grammaticalization (on which herein cf. especially JOAN BYBEE's chapter 19, HEINE's chapter 18, MITHUN's chapter 17, and ELIZABETH CLOSS TRAUGOTT's chapter 20, but also, to a more limited extent, FORTSON's chapter 21, HARRISON's chapter 2, HANS HENRICH HOCK's chapter 11, and BRIAN D. JOSEPH's chapter 13):

> As a lexical construction enters and continues along a grammaticalization pathway, . . . it undergoes successive changes . . . broadly interpretable as . . . a unidirectional movement away from its original specific and concrete reference and to increasingly abstract reference. Moreover, . . . material progressing along a pathway tends to undergo increasing phonological reduction and to become increasingly morphologically dependent on host material. . . . [T]he most advanced grammatical forms, in their travel along developmental pathways, may . . . undergo . . . continuous reduction from originally free, unbound items . . . to affixes.

Yet, given the transmissional discontinuity of languages – and hence of their morphosyntactic and lexical elements and principles – across individual minds, it behooves us to resist the temptation to view particular linguistic constructions (phrases, words, or morphemes) as if they were organisms with lifespans longer than those of humans by several orders of magnitude (much less as entities independent of people). This is not just misleading linguistics; it is also mutant biology.

One factor apparently responsible for the frequency with which grammaticalization studies (like the one quoted above) posit millennia-long "diachronic processes" and "mechanisms of change" is the temptation that exists to use biological – that is, organismal – metaphors for languages and linguistic entities. This misleading practice has already been criticized above, but the temptation is so strong (to judge from the number of linguists who apparently give in to it) that a few more words on this topic seem apposite here. The central point at issue is simply that the lives (i.e., the lifespans and lifetime activities) of biological organisms are not a good model for the "behavior" of – for what happens to and with – elements of language.

Actually, the more nearly accurate biological parallel is one where each speaker in the stream of overlapping generations is engaged in replicating morphemes which show strong phonological and semantic resemblances to morphemes used by a previous generation but often have distinct properties of form, category, or grammatical function (modulo the reservations expressed above regarding idealized generations). It is this ceaselessly repeated replication (on which cf. also Lass 1997: 111–13, 354–81) that allows both for general trends (like the downgrading correspondence that usually holds between instantiations of "the same" morpheme in the grammars of earlier versus later generations) and for occasional reversals where an innovation in one generation vis-à-vis another sometimes proceeds contrary to the statistically predominant direction of reanalysis. The best illustration for the illusion which unfortunately bedevils so many studies of grammaticalization is one similar to the "cloning" analogy (in the non-technical sense) that was just adduced: namely, a child's "flip book" – a low-tech instantiation of the principle that underlies motion pictures (for an example that is readily accessible, see Eames and Eames 1977, *Powers of Ten: A Flipbook* – based on a film of the same name). When a thumb is rapidly drawn down one unbound edge of such a booklet, a single figure appears to move across a single page, but there is in fact a rather long sequence of pages, each with a figure on it, though in a slightly different configuration relative to the figures on the other pages. Since we here have not one thing that changes, but only a temporal sequence of quite similar things, it is clear why, adopting essentially the same perspective as the current work on this specific issue, Coseriu (1982) chose to give his article a provocative title directly expressing its author's view that "Linguistic change does not exist."

Once we recognize that any linguistic phenomenon which appears to persist in relatively similar form over a period lasting hundreds of years necessarily requires multitudes of speakers to perform thousands of (near-)replications for some pattern of language, it becomes clear why innovations like those associated with grammaticalization arise in the first place, and with such frequency, as well as why there cannot be any "diachronic" unidirectionality constraints like those frequently discussed in the grammaticalization literature. That is, given the impossibility of any mechanisms which would restrict contemporary speakers' linguistic behavior in the use of morphemes by forcing them to consult what long-past generations once did, the only valid limits that make sense are synchronic ones relating to: (i) what speakers' minds predispose them to do in reaction to the data that they happen to hear around them, and (ii) their social attitudes of conformity, non-conformity, or hyperconformity to the usage of groups which produce such data. The former point, after all, is basically what Lightfoot (1979, this volume) has always emphasized, though a certain trigger-happy way of phrasing matters may have provoked some misunderstanding. In any case, such considerations should lead us to conclude that such commonly discussed and grammaticalizationally relevant notions as pragmatic subjectivization, semantic bleaching, morphosyntactic reanalysis, and phonetic reduction all actually constitute distinct synchronic phenomena which also exist apart from grammaticalization and so need not yield unitary, unidirectional/irreversible chains of linguistic development.

But, for anyone who adopts or maintains the metaphor whereby individual morphemes (and constructions) undergo putative long-term developments as if they were single living organisms, claims of unidirectionality/irreversibility

are quite consistent, since organisms live only forward. Nevertheless, the length and nature of the "path(way)s" which are thereby assumed provide some grounds for skepticism. In particular, the "path(way)" metaphor compares the sequences typically associated with grammaticalization phenomena to a walkway whose course is determined in advance because all of its parts are present and fixed at the outset. Indeed, with self-reflexive iconicity, much work on grammaticalization – itself often said (cf. Heine et al. 1991) to depend crucially on metaphor – relies heavily on a particular "path(way)" metaphor in which the walkway at issue leads gently but firmly downhill (as if gravity as well as narrowly spaced locking turnstiles prevented any retrograde movement) and is plastered with signs forbidding any wandering off the path to pick flowers or picnic on the grass. Yet it is not clear why and how speakers' use of morphemes at any given moment in the history of a language should be prevented from involving, for example, hypercorrection in such a way as to halt or to reverse a downgrading trend – and, indeed, upgrading phenomena are surprisingly common, once one starts to look for examples.

In short, then, we can actually be grateful to those grammaticalizationists (like Pagliuca 1994) who indulge in biological metaphors that turn, for example, morphemes into organisms. This is because – once we consider such analogies – the lack of evidence for that particular kind of comparison helps lead us quickly to the more insightful comparison of morphemes with patterns of speech which are replicated in interchanges: sometimes between speakers of the same generation, but also between speakers of different generations. And, as regards replication and other aspects of the biological transmission of information, Dawkins (1998/2000: 192–3) suggests some extremely useful distinctions based on the practice of biologists (for an alternative view see Salthe 1993):

> Modern biologists use the word *evolution* to mean a . . . process of systematic shifts in gene frequencies in populations, together with the resulting changes in what animals and plants actually look like as the generations go by . . . [. D]evelopment is not the same thing as evolution. Development is change in the form of a single object, as clay deforms under a potter's hands. Evolution, as seen in fossils taken from successive strata, is more like a sequence of frames in a cinema film. One frame doesn't literally change into the next, but we experience an illusion of change if we project the frames in succession. With this distinction in place, we can quickly see that the cosmos does not evolve (it develops) but technology does evolve (early airplanes are not moulded into later ones . . . [,] but the history of aeroplanes . . . and of many other pieces of technology, falls well into the cinema frame analogy). Clothes fashions, too, evolve rather than develop. It is controversial whether the analogy between genetic evolution, on the one hand, and cultural or technical evolution, on the other, leads to illumination or the reverse.

These distinctions (and comparisons) will be useful to keep in mind as we now proceed to other topics (and leave behind, for dead, the notion that linguistic units of any kind are organisms).

1.2.3.8 *Change is not stable variation or identical but independent recurrence*

In a very real sense, there is an equally important additional question lurking in discussions like the above that absolutely demands to be answered, or at least asked, when we confront the phenomenon of change in language (and elsewhere). It is all well and good to ask what we mean by talking about "change" in the first place, but we must also more specifically ask what it is that comes to be different when a language changes. If – as indeed seems to be the case, in light of the argumentation just presented – the transmission of language is discontinuous, and if language is therefore replicated (mutatis mutandis) generation by generation, then differences between states become evident only via comparisons. But such comparative pairings of different linguistic states come in several varieties, some of which can give the impression of involving change without actually doing so. This circumstance forces diachronicians to exercise particular caution in dealing with those linguistic elements for which speakers employ two or more variants. That is, in cases where an examination of the present confronts observers with ongoing linguistic variation in some aspect of usage, this situation need not actually represent "change in progress," even though that is a ready interpretation, one which is often accurate but just as often turns out not be so.[87] Rather, the coexistence of two or more variants may represent stable variation that can persist over long periods of time and confront the analyst with an opposition whose members possess their own socially interpretable significance.

For example, the current variation between two types of words which English-speakers use in order to address their own parents – little children tend to be the ones who use terms such as *Mommy* or *Mummy* and *Daddy*, while adults tend to employ *Mom* and *Dad* or *Mother* and *Father* – is not a reflection of a currently ongoing change in English. Rather, the use by a speaker (especially a male) of, say, *Mommy/Mummy*, as opposed to *Mom* or *Mother*, says something about his or her age, degree of dependence, and the like, but it does not allow us to conclude that he or she belongs to a particular generation or "vintage" (in the sense of a group defined by the proximity of their birth years and hence also by many shared experiences). For example, any linguist who is told that a randomly chosen English-speaker at some point in time called or calls his mother *Mommy* can easily specify within 15 years that speaker's age at the time (because saying "15 years old" will virtually guarantee success). But estimating such a speaker's birth year is likely to result in blind guessing, since the speaker could have been born in 1995, or 1970, or 1945, or 1920, or 1895, or. . . . That is, all of the available evidence known to us suggests that, for over a century at least, the vast majority of natively English-speaking children have called their parents *Mommy/Mummy* (or the like) up to a certain age, and then switched to *Mom/Mother* (or the like) for essentially the rest of their lives. In short, knowing that young(er) or old(er) speakers currently exhibit differences in some speech-pattern is not a sufficient basis for identifying the direction

or even verifying the existence of linguistic change. Instead, it is only when a situation involving such variability is compared with some other fixed temporal reference point, across real time, that it becomes possible to interpret the initial situation as reflecting change in progress and exhibiting a detectable directionality of change.

A practical consequence of this view is that, in order to make a meaningful assessment of some possible change, one has to establish beyond a reasonable doubt that, quite apart from the language-transmissional issues discussed in the preceding section (1.2.3.7), there really is some continuity between the "before" and the "after" that are being compared. In order to be maximally useful or even meaningful, a comparison of Old English with Modern English would have to control for dialect (as noted above in section 1.2.1.6), in order to ensure there is what we might term "direct lineal descent" between some element in stage 1 and its altered form in stage 2. At the same time, we also need to allow for independent (re)creation of phenomena at different stages.

Thus – to take a very specific, concrete example – the documented occurrence of *mo* for *homo(sexual)* in student slang at Duke University (in North Carolina) during the late 1980s[88] and its earlier attestation in the slang of adolescent boys at Camp Ethan Allen in Vermont during the early 1960s[89] most likely represents a pairing of forms that arose independently of each other. Each occurrence seems to have arisen as an only accidentally parallel selection from among the shared set of word-formation possibilities – a clipping, in this case – that characterize slang.[90] In this sense, there is a diachronic correspondence between 1960s Vermont *mo* and 1980s Duke *mo*, but nothing that clearly connects them via direct lineal descent, because there is nothing that fills in the temporal and geographical distance between them. Even with such independent occurrences, though, there are still diachronic questions to be asked: for example, how did each community come to create the relevant form?; how did it spread within each community?, and so on. Still, with no continuity, with no filling in of the gaps, there is here no connected history to speak of, but only distinct, separate occurrences, each rooted in its own present moment.

In talking about change in language, we necessarily take a diachronic perspective and investigate the effects of the temporal dimension on linguistic behavior by humans. We tend to focus on what has changed between language states, but, in a sense, it is equally revealing to note what does not change and to develop from that a sense of what can remain stable in a language through time. Clearly, anything about language that is truly universal should remain invariant across time,[91] but our knowledge of truly absolute and inviolable universals of human language – "design features," as it were – is rather circumscribed, at best. Recognizing, though, that some aspects of language do not change allows us to see change as something noteworthy when we do become aware of it, and thus as something that needs to be explained. Indeed, in chapter 2, HARRISON takes precisely such a view with regard to the workings of the comparative method, and, in chapter 5, NICHOLS similarly points to various pockets of stability in language over time. Moreover, we know that arbitrary aspects of language can persist through time, and this again shows that there can be stable elements and temporal continuity. Labov (1989a: 85), for example, notes the situation whereby "children acquire at an early stage historically transmitted constraints on variables that appear to have no communicative significance, such as the grammatical conditioning of . . . [*-ing* versus *-in'*] in English," and, among other, similar cases, he discusses the variable deletion of final [t]/[d] in English, as well (see also section 1.2.3.3 above).

To an extent, then, doing historical linguistics, or even just viewing language diachronically, involves an attempt to focus on precisely those aspects of language which require a kind of explanation that is often loosely called "historical," as discussed earlier (see n. 68), but can more accurately be labeled *polysynchronic*. Thus, certain individual present-day phenomena can seem synchronically unmotivated vis-à-vis the overall patterns of a contemporary grammar, but they may turn out to make eminent sense when seen either (i) as survivals – passed on through a connected series of intermediate synchronic states – from a historically antecedent state in which they were synchronically motivated, or (ii) as analogies based ultimately on such survivals. In the above-mentioned case of *mo*, for instance, its post-clipping occurrence in two distinct locales at different times need not be explained with reference to history (the past) – via the positing of a direct lineal link between an earlier and a later synchronic state, since each clipped result can be motivated in its own right, at its own synchronic time and place. But, given the usual arbitrariness of the connection, in linguistic signs, between the signifier and the signified (à la Saussure), the fact that *m-* occurs at all in *mo* cannot be explained in (mono)synchronic terms (except through the accidental convergence of independent spontaneous coinages), much less on universal grounds (in contrast to what might be argued for, say, the *m-* of *ma* "mother"). Rather, the *m-* of *mo* can be explained only in terms of continuing retention from an earlier time, hence polysynchronically (but not really "historically": after all, there are countless other phenomena whose origin in "history" – the past – has not guaranteed their survival into today's present).[92]

1.2.3.9 *Language change as change in language, not of language(s)*

In clarifying here what we mean by *change*, it is important to exclude certain conceivable senses of that word when it follows *language*. For instance, the label *language change* is not used in this volume to refer to what might be termed "language shift" or "language replacement" situations, especially ones involving a transfer of language loyalties and preferences from one tongue to another. This caveat is in no way intended to be facetious: Posner (1997: 3), for example, distinguishes between *linguistic change* (which affects "dynamic systems . . . [having] their own mechanisms of change") and *language change* (since "the language of a community, as an entity, can change"); in so doing, she creates the strong impression that the latter term refers (primarily) to language shift.[93] In any case, to discuss a concrete possibility: if more and more speakers in (the) Ukraine should now begin to use Russian, rather than Ukrainian, in their day-to-day affairs, one could talk about a change in language(s) taking place there, but this switch would involve the partial substitution of one language for another – a replacement of one language by another in a particular social arena – not an immediate change in either one of the two languages involved.[94] As important a topic as this general kind of shift may be, it is not, in itself, directly central to historical linguistics as the field has been defined here.

Rather similarly, the term *change* by itself is often used elsewhere in a purely synchronic sense. Consider for example, the much-discussed Modern High German generalization of "final devoicing" (or, in German, *Auslaut(s)verhärtung*) as it relates to the word-final /g/ which can be motivated at the end of the underlying representation of, for example, the morpheme that means "dwarf" (on the basis of the phonetic [g] that surfaces in nominative plural *Zwerge* "dwarfs" (or "dwarves"). In this specific case, the relevant process is often said to "change" /g/ into phonetic [k] (or, on more structuralist accounts, into phonemic /k/) at the end of the (bare) nominative-singular form *Zwerg*. Now, admittedly, such alterations in form are frequently linked in important ways with historical phonology, since they are often the synchronic reflections of sound changes. See, for example, chapter 3 by RINGE on internal reconstruction, and chapter 9 by RICHARD D. JANDA, which refers in part to neutralization-related (a.k.a. morphophonemic) alternations like German [g] ~ [k] (but also is partly focused on the ways in which the so-called "phonologization" of former allophones really involves morphologization and lexicalization). Still, our interest here in synchronic alternations is restricted to the ways in which they arise from, and may reflect, past situations and events.

1.2.3.10 "Historic linguistics, *you're history!*": *generalizing historical linguistics*

Having devoted close attention to several of the issues connected with the concept and term *change*, we turn lastly to *history*, *historic*, and *historical*, yet another terminological nexus that figures prominently both in this work and in work on diachronic linguistics in general. We do so mainly because, within the field of historical linguistics, the label *historical* is sometimes employed in a way that gives rise to ambiguity (and thus also to at least some confusion), the latter due mainly to the fact that the adjectives *historical* and *historic* show semantic overlap – which arises from the fact that the noun *history* is itself ambiguous.[95]

On the one hand, *historical* can refer to anything that has taken place in the past, possibly with a limitation confining it to exactly those prior events which have been documented in some written form – hence the distinction between *history* and *prehistory*, even though historical linguists often try to determine prehistoric(al) states of affairs and, to that end, propose specific reconstructions (see chapter 1 by RANKIN) or statements of language relationships (see chapter 4 by CAMPBELL). For many scholars who would describe their field as "historical linguistics", one legitimate target of research involves a focus not on change(s) over time but on the synchronic grammatical systems of earlier language stages. This practice can be called (not unrevealingly) "old-time synchrony," and it has made its mark in the form of numerous studies providing synchronic analyses of particular syntactic constructions, word-formation processes, (morpho)phonological alternations, and the like for individual earlier (pre-modern or at least early modern) stages of languages. Thus, for example, Sommerstein (1973) treats the synchronic phonological system of Ancient Greek. Gaining as much synchronic information as possible about an earlier stage of a language must surely be viewed as a necessary prerequisite for doing serious work on the diachronic development of a language: as noted above (in section 1.2.3.1 regarding "vertical" comparison, and see also n. 59 and section 1.2.1.6), it is through the comparison of two stages of a language that we get a glimpse of what has changed (or remained the same, as the case may be). Nonetheless, pursuing the synchrony of earlier language states solely for the sake of (synchronic) theory-building (e.g., discussing proposed global rules in syntax based on agreement patterns of Ancient Greek, in the manner of Andrews 1971), as worthy a goal as it may be, does not count as doing historical linguistics in the literally *dia-chronic* (through-time) sense that we wish to develop here. At least in a technical sense, then, *diachronic linguistics* and *historical linguistics* are not synonymous, because only the latter includes research on "old-time synchrony" for its own sake, without any focus on language change.

But we must now bring in the term *diachronic* again for a comparison with *historical* vis-à-vis their individual combinations with *change*. In this regard, we would argue that it is perfectly legitimate to talk about *diachronic change*, since change indeed takes place through time (or at least is evident from a comparison of states across time) and also since change over time needs to be distinguished from diachronic stasis and/or stability. What we find unnecessarily misleading, however, is the phrase *historical change* (cf., e.g., Pinker 1994: 489), since change itself can never be banished to some historical (i.e., temporally distant) stage of a language. Rather, change is always instantiated over a period of contemporary time – that is, over a series of synchronic states which constitute a succession of present moments. The result of a change could indeed be talked about as something historical, but the process of change itself is always unfolding in some present moment(s) for some speaker(s). Before leaving this topic, let us return briefly to the above-mentioned assumption that, if it is legitimate to speak of *diachronic change*, then it is equally reasonable to talk about *diachronic stability*. Regarding the latter concept, we would like to stress that, as reflected in chapter 5 by NICHOLS, it is just as important – even if this is traditionally a lesser concern for historical linguists – to consider what in a language does not change through time, not just what does change.

Juxtaposing *historical* and *history*, we note that a linguistic diachronician may encounter both of the expressions "historical linguistics" and "language history" (on the earlier use of latter term, albeit from a slightly different vantage point from that assumed here, consult Malkiel 1953). According to one common view, doing historical linguistics in the sense of looking at earlier linguistic stages and making comparisons between and among them can also lead to studying language history: that is, the history of a particular language or languages – a kind of glosso(bio)graphy, so to speak. Such information generally forms the basis for our understanding of language change in general. There thus necessarily exists a link between language change and language history, even though the study of language change can be pursued without any need to venture very far, temporally, from the present – as shown by the work of Labov (along with his students and other collaborators) on urban American English in the latter half of the twentieth century and the beginning of the twenty-first. That is, one does not have to be very historical (much less historic; see below) to be a historical linguist. The field is open (as it should be) to both studies of language history and studies of language change.[96] We might then say that historical linguistics is about the linguistics of history and the history of languages, and includes all that those two areas encompass.

On the other hand, there is an additional moral latent in the fact that the English word *historical* (attested since the fifteenth century) is also sometimes used to mean (or at least to connote) the same thing as *historic* (attested since c.1607), hence roughly "famous or important in history, having great or lasting significance, known or established for an appreciable time." Thus, for example, in the American Automobile Association (AAA) of Ohio's *Home and Away Magazine* 21.2 (for March/April, 2000), there is a vignette (p. 65) with the punning title "Historical Descent." This description initially raises the expectation that what follows will relate either to someone's having had a prominent ancestor or to a famous exploit involving downward movement (say, an early aviator's momentous landing, or a spelunker's record drop deep into the earth). But the text that then follows turns out to present simply a description of a hike down into Heritage Canyon (near Fulton, Illinois), where an open-air museum in a former quarry preserves old buildings moved there mostly from neighboring sites. The descent at issue is undeniably *historical*, since it has to do with local history, but it is hardly *historic* in the sense of being either generally significant or well known, even though the phrase *historical descent* which is at issue here readily invites this inference. On the other hand, *historic* is occasionally used with the meaning 'relating to (or having a) history,' as on an intriguing sign outside a Central California town which orders passers-by to "Visit historic Templeton!" Since Templeton (population 800) does not rate a "Points of Interest" entry in recent editions of the AAA's *California . . . [/]Nevada Tourbook* (over 1200 pages long, in its 1999 update), and since the town (located between Atascadero and Paso Robles) no longer even appears on *Tourbook* maps (as it did in the 1992 edition), but receives only an "Accommodations" listing (for two restaurants), it does not seem at all like a place connected with events of general significance, famous or infamous.[97] Templeton, California, then, is *historic* only in that, like everything else in the universe, it has a history, or else it would not exist. Current use of the adjectives *historical* and *historic* is thus indeed somewhat mixed up, and hence can be misleading.

We do not, however, mention this potential confusion mainly because it illustrates semantic variation or change in contemporary English. Rather, we do so because it provides one of the few explanations available for why certain scholars sometimes appear to interpret *historical linguistics* as if it were *historic linguistics*,[98] the study of languages only insofar as they have either undergone momentous changes or been spoken by communities which have produced people and achievements famous in history: for example, the Athens of Pericles, the Rome of Augustus, or the England of Shakespeare, Chaucer, and whoever composed the epic poem *Bēowulf* (*'Bee Wolf,'* whose hero's vulpine ferocity is matched by a stinging sword).[99] That is, a survey of all the books and articles written up until now by historical linguists would arguably reveal an extreme bias in favor of Indo-European languages – and, within that family, in favor of Classical Latin, Classical Greek, the literary monuments of earlier stages of English, and similar foci in other "languages of culture," as they are sometimes self-promotingly termed. For instance, any readers who attempt to find an introduction to linguistic diachrony that does not exemplify haplology by citing Latin *nūtri-trix* > *nūtrix* 'female nourisher, nurse,' or else older English *Engla lond/land* > *Englond/England* 'Angles' land, England,' will find that even a consultation of Crowley (1997: 42), with its intended "Pacific bias" favoring especially Austronesian and Indo-Pacific Australian languages (p. 10), is going to let them down.

Yet, as we have already stressed in several of the preceding sections (1.2.1.4–1.2.1.6), this skewing imposes on the study of language change not only (i) self-defeatingly narrow horizons (via the elimination of so many language families and languages where change indisputably takes place) but also (ii) artificially binocular-sized perspectives within those already limited horizons (via the exclusion of non-standard varieties and even colloquial styles). It is true, we confess, that the last century and especially its latter decades have seen historical linguists pursuing a historic trend toward an increasingly strong focus on non-(Indo-)European languages and on non-standard, non-formal varieties. Still, the non-academic public apparently remains convinced that the older literary monuments of classical tongues and standard languages should be the focus of diachronic linguists, and this can have repercussions even for research on ongoing change in modern colloquial English. The *Wall Street Journal* reported in 1980, for example, that then vice-presidential candidate George H. W. Bush, after hearing about a large NSF grant awarded to Labov and his colleagues at the University of Pennsylvania for the study of local speech, exasperatedly asked in public why anyone would care how people talk in Philadelphia. It seems safe to draw the historical inference that Vice-President and later President Bush did not agitate for increased funding of quantitative variationist sociolinguistics during his 12 years in or near the White House.

But, just as the philosophical study of events has elicited the comment that "[e]vents need not be momentous: the fall of a sparrow is as much an event as the fall of the Roman Empire" (cf. Mackie 1995: 253),[100] so linguistic diachronists have everything to gain from promoting the view that the texts which comprise their subject matter are often most revealing when they are not historic, but only historical. It must therefore belong to the mind-set of those who study language change to believe (with apologies to W. C. Fields for exploiting what is popularly believed to be but is in fact not his epitaph; cf. Burnham 1975: 123; Boller and George 1989: 26; Rees 1993) that one linguistic interest of George H. W. Bush – and in fact of every George Bush – actually should, on the whole, rather be in Philadelphia: in how people talked there in 1980, and how they talk there now. Even a traditional literary classic like Shakespeare's 1599 *Julius Caesar* (in act III, scene 2) implicitly warns us that broad-based investigations are necessary because the determining influence on future English (or any other standard language) may come from a region, "many ages hence . . . [, having] accents yet unknown." Because it is precisely such broad coverage – of change as well as of variation – at which the determining plan of the present work aims, we follow the next section with a compact overview of this volume and the papers in it, organized by topics rather than by page numbers.

1.3 On time

[W]hat is time? . . . Who can explain it easily and briefly? Who can grasp . . . [it], even in cogitation, so as to offer a verbal explanation of it? Yet . . . what do we mention, in speaking, more familiarly and knowingly than time? And we certainly understand it when we talk about it; we even understand it when we hear another person talking about it. . . . What, then, is time? If no one asks me, I know . . . [,] but, if I want to explain it to a questioner, I do not know.

Aurelius Augustinus (St Augustine), *Confessionum libri 13 "(13 Books of) Confessions"* (*c.*400; critical edition 1934/1981), trans. Vincent J. Bourke (1953)

The besetting sin of philosophers, scientists, and . . . [others] who reflect about time is describing it as if it were a dimension of space. It is difficult to resist the temptation to do this because our temporal language is riddled with spatial metaphors . . . [: e.g., we say,] "Events keep moving into the past". . . . [But] events cannot literally move or change . . . [; a]s Smart (1949) . . . asserted, things change, . . . [but] events happen. . . . Those who spatialize time, conceiving of it as an order in which events occupy different places, are hypostasizing time. What we perceive and sense are things changing. Time is a nonspatial order in which things change.

C. W. K. Mundle, "Consciousness of time," in Edwards (1967: VIII, 138)

With a saintly scholar like Augustine already on record as expressing extreme uncertainty and even anxiety about attempts to define time, it would seem that, perhaps apart from formal semanticists, no linguists – not even historical linguists – should announce their intention to characterize temporal concepts without first recalling the saying (from part 3 of Pope's 1711 *Essay on Criticism*) that "fools rush in where angels fear to tread." Still, we believe that a certain amount of work on language change has been and still is bedeviled by an insistent though usually unspoken adherence to an arguably misleading and ultimately indefensible assumption about time: namely, that what modern-day historical linguists – and other historians – directly study (in whole or in part) is something called "the past" which exists elsewhere than in the present. While there is much to criticize in this view, we also take seriously the proverb that warns: "What's sauce for the goose is sauce for the gander." Thus, precisely because we are convinced that pernicious consequences beset the view – perhaps even the majority opinion – that linguistic diachronicians are engaged in direct study of a non-present "past," it behooves us to outline an alternative approach, even if this should turn out to be a minority pespective that is itself greatly in need of elaboration and refinement. In this section, then, we begin by presenting some remarks on the general nature of time; we then bring these notions to bear on questions of linguistic change and reconstruction.

Devout respect for St Augustine's thoughts on time has not stopped later generations of scholars from continuing to address this topic at length. For example, an International Society for the Study of Time has existed since 1966, holding conferences and publishing proceedings at quite regular intervals (cf., e.g., Fraser and Lawrence 1975.). Hence we disclose no secrets in admitting that even authors in tandem can find time to achieve only the barest sampling of the vast pertinent literature. In atonement, our sole recourse here is to highlight, from among the seemingly endless list of available works, a useful sample of the writings that we have found most cogent. For perhaps the best overview of the literature on time and the broad range of issues involved, see Fraser (1966) and references there. Other helpful anthologies include Gale (1967), van Inwagen (1980), Healey (1981), Swinburne (1982), Flood and Lockwood (1986), Le Poidevin and Macbeath (1993), Oaklander and Smith (1994), Savitt 1995, and Le Poidevin (1998). In turn, virtually all the papers in these volumes themselves list additional references, and some of the books' editors have annotated their lists of further readings (cf. especially Le Poidevin and Macbeath 1993: 223–8). As for concise single-authored works, among those most valuable to us have been Whitrow (1961, 1988), Mellor (1998), and, despite its unusual title, Nahin (1999) – all with extensive bibliographies – plus, especially as a historical overview, Turetzky (1998).[101]

Without seeking to one-up Augustine, we must in all fairness confess that it is much easier to say what time is not than to say what it is. In line with this, we here devote only the barest programmatic remarks to a positive characterization of time, whereas we offer a much more extensive negative critique of certain commonly held competing approaches. Yet, from the etymological sense of definition (i.e., *de-fin-ition)* as marking off ends (*fin-es*) and hence setting limits, it follows that the act of establishing what something is not can also play an important role in defining a thorny concept. At any rate, in essaying to state what time is, we are most persuaded by an overall perspective whose defenders include, among many others, Mundle (1967), who equates time with

change – a view already quoted at the outset of this section (recall "Time is a nonspatial order in which things change") and who thus concludes (p. 138) that "[o]ur consciousness of time's 'flow' is our consciousness of things changing." Similarly, Mellor (1981: 81, 1998: 70) emphasizes that ". . . [c]hange is clearly of time's essence" (cf. also the similar treatment adopted by Le Poidevin 1991).

This change-based approach has the merit of facilitating a direct, non-circular account of a central temporal distinction – variously labeled "before" versus "after," or "earlier" versus "later" (with non-relativistic simultaneity being definable as their joint negation) – which is crucial for any attempt to characterize the directionality of time (cf. also Reichenbach 1928; Earman 1974; Horwich 1987; Mellor 1991; Savitt 1995; Price 1996; and references there). This advantage derives from the fact that ordering in time can be equated with the structuring of changes, because changes are inherently associated with processes, while the latter, in turn, inherently possess an asymmetrical internal organization which is related to matters of cause versus effect. Moreover, given that processes can be interlinked either via overlapping (where portions of two processes are also associated as co-parts of a third process) or via proper inclusion (where two micro-processes co-occur within one macro-process), the totality of such complex and chained processes corresponds to (i.e., "covers") the connectedness and continuousness of time, since there will never be any moment at which "nothing is going on anywhere." (Take a moment to consider, in this regard, how staggeringly many processes involving subatomic particles must be active in the universe at every instant, even for entities ostensibly "at rest"![102]) In Mellor's (1998: 118) words, "the causal theory of time order . . . makes the asymmetry and irreflexivity . . . [of 'earlier' and 'later'] follow from the fact . . . that nothing can cause or affect either itself or its [own] causes." This theory "also tells us why the direction of time has no spatial analogue, since . . . causes have effects in all spatial directions." On such a view, we need not even assume that time exists independently and thus provides a dimension in which processes can take place; rather, we may assume that processes and their structure define time and so can be said to constitute it.

Although it remains controversial, the above-mentioned causal theory of time – arguably anticipated by Greek and Roman philosophers (like Epicurus (*c*.341–270 BC) and his poetic interpreter Lucretius (*c*.95–52 BC); cf. Lucretius *c*.60 BC: 1.198–9, 2.670–1) – has clearly exercised a solid intuitive appeal during the past three centuries. After this viewpoint was first extensively laid out by Leibniz (von Leibniz and Clarke 1717), it was soon after revised by Kant (1781: 188ff), and it has now been further elaborated by modern scholars ranging from Earman (1974) to Mellor (1998). To this causal approach there corresponds a parallel theory in which the central asymmetry at issue is not between cause and effect, but instead between lesser and greater entropy – the latter being a measure of the randomness (i.e., chaos, disorder, etc.) among the part(icle)s of a system (for a general discussion, cf. Kaku 1995: 304–6). This perspective goes back, via Reichenbach (1928) and Eddington (1928), all the way to Boltzmann (1898: 257–8, and even 1872). Strikingly (and fortunately), Hockett (1985) happens to summarize and illustrate exactly this kind of entropy-based approach as part of a detailed discussion relating specific aspects of diachronic linguistics to general considerations in history and historiography. Hence we here quote an extended passage – from Hockett (1985: 328) – at least partly as a down payment on an implied promissory note (from the current authors to our readers) guaranteeing that the present section does, indeed, move from the generally temporal to the specifically linguistic (and historical):

> If you are told that, of two observations made one second apart . . . [– their relative times] not being specified . . . [– one] found the air pressure at both ends of a closed chamber the same, while the other found high pressure at one end and zero at the other, you have no trouble inferring which of these states came first . . . [. T]he second law of thermodynamics is only a statistical generalization, so . . . it is not . . . impossible for all the air in the chamber to rush suddenly to one end, but the probability of that event is extremely small, and you are surely right to make the more likely inference. . . . The example is trivial because . . . extreme, but . . . also . . . clear. The reference to the second law of thermodynamics is not out of place . . . [:] as Blum [(1968/1970)] says, it is entropy that establishes "time's arrow . . ."[. Thus, e]very historiographic decision reduces to elementary inferential acts like th[e] . . . preceding . . . [, or else] it is not valid.

These considerations, being completely general, also apply fully to linguistic reconstruction, which is the ultimate focus of the present section. Hockett (1985: 328) therefore goes on to state that:

> [i]n more general terms . . . [,] there is evidence for two states of affairs (or events), S_1 and S_2, separated in time but not in space. It is known that one of these was succeeded by the other, but not which came first. Now S_1 is of type T_1 . . . [,] and S_2 of type T_2. If there is empirical evidence that type T_1 can give way to type T_2, but that the opposite order of succession is improbable, then, obviously, it is inferred that S_1 preceded S_2; similarly in the converse case. Sometimes there is no such evidence, or the probabilities are even, or it is not clear to what types S_1 and S_2 belong, so that no decision can be made . . . [. I]f the probabilities do not strongly favor one order or the other, the historical inference for the particular case is correspondingly insecure.

From Hockett's well-taken remarks on the necessity of recognizing the role of probabilities in historiography in general,[103] it is a short step to an important point about the nature of linguistic historiography – that is to say, about linguistic reconstruction. However difficult a concession it may be for historical linguists, they must in all honesty admit that it is virtually, perhaps even absolutely, never the case that the probability of full accuracy for a reconstruction of a non-recent past event is 1.0. Thus, even with regard to a form like the reconstructed stem for 'father' in PIE – *pəter-, a reconstruction which is widely accepted and surely believed in to a high degree by most practicing Indo-Europeanists – much remains indeterminate: for example, (i) whether there was any distinctive or non-distinctive aspiration on the initial stop, and, if so, to what extent; (ii) exactly where in the mouth contact was made for the medial stop; and so forth.[104] Surely there can be no less indeterminacy in the reconstruction for 'name' in PIE, where the forms in the various languages match up reasonably well but still fail to agree in certain details.[105] Hence, the primary question here, far from being how close to 1.0 the probabilities of proposed linguistic reconstructions definitely are, is instead how close to 0 (zero) they might conceivably be.

1.3.1 *A skeptical challenge to the unreconstructed nature of reconstructions*

As a result, it has been proposed in all seriousness by Janda (1994a, 2001) that the asterisk as an indicator of reconstructed forms in historical linguistics should be abandoned in favor of a complex symbol roughly of the form *n% (RN)*, where the variable *n* stands for a number showing the reconstructor's (or a later writer's) percentually expressed level of confidence in a particular reconstruction, while the parenthesized *(RN)* stands for the initials of the reconstructor's name (or of a later writer's name). In this revised notation, Schleicher's (1868) reconstruction of 'master' (i.e., 'powerful one') in a shape like PIE *patis[106] would presumably be reformulated as *99.9% (AS)* patis by a revivified Schleicher but as *0% (CW)* patis by, for example, Calvert Watkins (cf. Watkins 1985: 52–3),[107] whose – and many others' – preferred alternative, *potis, we ourselves would in turn give as *90% (RDJ and BDJ)* potis, owing to a number of uncertainties such as those expressed above concerning *pəter-.

That is, we do not doubt for a moment that it is well justifed to reconstruct *some* PIE word meaning something like 'master' and having roughly the shape *potis, but it will most likely never, ever be possible – either for us or for our successors – to verify every detail in the phonetics of the reconstructed form, let alone its semantics. (For example, regarding its range of referents, we may legimately ask whether the term at issue applied only to powerful adults, or also to powerful children, or even – metaphorically – to powerful animals or the like.) Hence we do not consider the *n% (RN)* label for reconstructed items to be in the least a facetious suggestion; indeed, such a notation would in fact be a first step toward devising a reliable index for indicating the degree(s) of (un)certainty associated with many specific proposed linguistic reconstructions. And extending this notational practice to every segment (or even every intrasegmental feature) in reconstructed forms would go a long way toward iconically reflecting the full extent of their iffy, diaphanous nature.

That such a percentual labeling for reconstructed forms has considerable advantages over simple asterisking becomes immediately apparent in cases where the reconstruction of a joint pre-proto-ancestor is made solely on the basis of two (or more) totally reconstructed proto-languages. This kind of reconstruction that goes back beyond (i.e., further back in time than) a given proto-language, via application of the comparative method to two proto-languages, has been discussed as a procedure by – among others – Haas (1969). And it has been practiced to an extreme degree by so-called Nostraticists (cf., e.g., the discussion pro and con in Joseph and Salmons 1998, as well as Campbell 1998 and CAMPBELL's chapter 4 here). Comparisons of this sort are generally treated as if they were just like reconstructions based solidly on two sources of attested data. But if one proto-form that is less than fully secure (e.g., rated at only 70 percent, in the *n% (RN)* notation) is compared with another proto-form that is similarly less than fully secure (and thus again rated at only 70 percent), then the result is the reconstruction, not of a 70 percent certain pre-proto- (or even "proto-proto-") form, but rather of a form that is 49 percent "certain" – and so clearly has a score that is closer to 0 percent than to 100 percent. It is admittedly true in such instances that, if one piles up the asterisks, then the multiplicity of stars does iconically tend to suggest that there is (or should be) greater uncertainty among scholars as to the probable accuracy of the relevant reconstructions. The monograph on Indo-European (IE) /a/ by Wyatt (1970), for example, – though its focus is not on comparative but on "internal" reconstruction (cf. RINGE's chapter 3 herein) – uses * for reconstructed Proto-IE (PIE), ** for pre-proto-IE (PPIE), and *** for pre-pre-proto-IE (PPPIE); hence, in proposing a particular (and particularly static) prehistory for the root meaning 'drive; lead,' Wyatt (1970: 56) writes "****ág-* > ***ág-* > **ág-*."

However, the rapid dropping-off of confidence which necessarily accompanies the act of reconstructing items from reconstructions alone is indicated much more accurately via the multiplicative effects of the percentual notation, since in principle a pair of reconstructed forms bearing respectively a %X and a %Y label can together yield at most a %X·Y-labeled pre-proto-form, where the product X·Y must necessarily be lower than either X or Y. (We presuppose that a reconstructed form can surely never have a value of 1.0, for full confidence.) In sum, the use of a(n) (un)certainty index for proto-language forms makes possible a far more realistic assessment of probabilities (i.e., the likelihood of actual prior existence) in cases where essentially "proto-proto-" forms have been reconstructed on the basis of two or more sets of already-reconstructed proto-forms. As indicated by the rapid drop-off of the percentual scores in such cases, uncertainty ramifies much more quickly at greater (= more profound) time depths when only proto-forms are used, according to the method of Haas and many Nostraticists, in order to base reconstructions on reconstructions (on reconstructions (on reconstructions . . .)).

Further, while many linguists limit their use of the term "reconstruction" to the positing of forms and constructions for linguistic stages from which no records survive, it is actually the case that even attested stages of languages require considerable interpretation and filling-in of details – as well as more substantial aspects. Hence virtually all historical linguistic research merits the descriptor "reconstruction." And, finally, it must be conceded (if one is truly honest) that the presence of *re-* in *"reconstruction"* presupposes a degree of certainty about the accuracy of proposals regarding earlier states of linguistic affairs which flies in the face of the (im)probabilities just discussed. To be blunt about it, we do not so much "reconstruct" a proto-language as "construct" it in the first place (although subsequent revisions of such *cónstructs* could perhaps be called *ré-cònstructs*). In fact, it might be preferable, as a precautionary measure, for diachronicians to talk about "speculating" a proto-language (or part of an attested language state), rather than about "reconstructing" it.

We emphasize this point (at the risk of belaboring it) because some linguists engaged in linguistic reconstruction give the impression that they take their proposals to be 100 percent accurate, acting almost as if they believe that the original linguistic objects which they seek to reconstruct still exist somewhere, frozen in time at some other place or in some other dimension – which, if only it could somehow be accessed, would confirm their proposals.[108] But is this kind of cocksure certainty not tantamount to a belief in the possibility of time travel back to, say, the Pontic steppes in *c.*3000 BC (on one view of where PIE might have been spoken)? (cf. Harrison, this volume, section 2.2.)

As a result, we think it appropriate at least to touch briefly on the issue of whether time should be conceptualized and discussed in spatial terms (another topic which is perennially discussed in philosophical disquisitions on time) – partly because it intersects with the issue of whether or not so-called "time travel" is now or someday will be possible, and what that might (or might not) mean for historical linguistics.

1.3.2 *Time is not space (and diachrony is not diatopy) – but is time travelable?*

In order to explore time and space, and time as space, we return to the aforementioned matter of discussing what time is and what it is not. First of all, one must guard against the tendency (surely an understandable temptation) to confuse time itself with the measurement of time. Thus forewarned, one can more readily see that any and all references to durations such as picoseconds, nanoseconds, milliseconds, seconds, minutes, hours, days, months, years, centuries, millennia, etc. actually reduce to using phenomena that recur at regular intervals as a background available for correlation with other events. But, obviously (we say along with most but not all philosophers and physicists), time must surely involve more than the measurement of time, and to pick one method for measuring time is not to define time itself.[109] A second and much more relevant misconception about time, however, arises from unconscious but no less real reductions of time to space. Now, ever since shortly after they were stimulated by Einstein's (1905) paper on special relativity (summarized and explicated in Fölsing 1997: 178ff), physicists have widely exploited the idea of "space-time." As Minkowski (1908: 54) put it, "space on its own and time on its own . . . decline into mere shadows, and only a kind of union between the two . . . [can] preserve its independence" (for insightful discussion, cf., e.g., Greene 1999: 47–66 et passim). But physicists' space-time is not the notion that needs cautioning against in historical investigations (linguistic and otherwise). Rather, there are quite a number of approaches to time which either view events and times as "moving" (see, e.g., Williams's 1951 much-cited critique of "The myth of [time's] passage," i.e., the view that time literally passes (by)), or, what is worse, treat times as if they were places. It is this latter perspective which, we argue, is most pernicious for historical linguistics, because it appears to provide the unspoken premise behind certain proposed reconstructions whose presupposition of eventual verification in fact (or at least of verifiability in principle) would otherwise have no leg to stand on.

The problems that attend this view of time as place are numerous; we mention only a few of them here. For one thing, there is a matter of consistency. Though it is incompatible with the dominant view that the past is by definition over and gone, the opinion that the past (still) exists somewhere as a place is admittedly not without adherents, but how could the future exist as a place if it has not yet happened, and thus presumably could not really be located anywhere (at least not yet)? Also, if individual times were places, would it not then be the case that revisiting ("reliving") the past would involve flitting from temporal location to temporal location? If so, how would a time traveler physically continue into the next state that lies ahead of the state currently being visited, since that next state would itself be a place with its own location?

And what would be the length – the temporal duration – of such individual states? If they are short enough (say, one picosecond in duration), could a visitor see anything significant happening there? With all the traveling in-between states, would this perspective on time not be even jerkier than watching the frames of a movie as if they were a fast slide-show? Or would the individual states themselves be long enough to have their own temporality (their own internal time structure, with events happening before versus after one another)? Would a visitor to state X alter it in some substantive way, and thus create a state X′? If so, where would the latter be located, and would the visitor instantly enter such state? Where, in fact, would any state of this sort have its existence? If the relevant location is "in some other dimension," then what is the ontological status of this dimension? Much more specifically, if there actually should be some subpart of the past which is the place(s) where PIE "perdures" (as Michael Silverstein might say), how many temporal states does this represent? Would it be possible to reconstruct the range of variation surely extant in such a language from one individual time-state/place? What would ensure that a visitor to any such state would travel in the right sequence to one or more of the subsequent states? And so on and so forth.[110]

Given the multiple problems attendant upon the space-as-time approach (≠, to repeat, the relativistic notion of space-time), we here reject it – whereby we follow such similarly minded scholars as Smart (1949, 1955, 1967), along with the above-mentioned Mundle (1967) and Williams (1951). This conclusion renders impossible one major proposal on how travel through time might be possible, since some notion of past as place(s) seems to underlie the popular conception of how time travel could work – as a physical journey to some place(s) where past states continuously wait for out-timers to visit them. This is, for example, one interpretation of H. G. Wells's (1895) novel *The Time Machine* (recently refilmed), which ends with its narrator wondering whether the book's protagonist "may even now – if I may use the phrase – be wandering on some plesiosaurus-haunted Oolitic coral reef, or beside the lonely saline seas of the Triassic age." In this case, it would seem that, with the publicly declared bankruptcy of the spatial theory of time, there are no prospects that time travel could ever get off the ground.[111] But die-hard advocates of the view that linguistic reconstructions are somehow still verifiable in principle might continue to argue (or at least to assume) that, even if time is not spatial, time travel (of another sort) is nonetheless possible.

Although premising a short story, novel, or film on the possibility of travel through time can lead, in the best cases, to entertaining and even riveting plots, it is ironic that most writings or lectures by philosophers on the subject of time travel have the effect of making the reader or listener look repeatedly at his or her watch. Admittedly, there are certain works (some now almost with the status of classics) which are often discussed and thus bear mentioning here: for example, Earman (1974), Meiland (1974), Lewis (1976a), MacBeath (1982), Ehring (1987), Horwich (1987, 1995), Craig (1988), Flew (1988), Maudlin (1990), J. Smith (1990), Edwards (1995), Vihvelin (1996), and N. Smith (1997).[112] Yet we must agree with Earman's (1995: 268) assessment that "[t]he philosophical literature on time travel is full of sound and fury, but the significance remains opaque . . . [, and there is a rather narrow] focus . . . on two matters, backward causation and . . . paradoxes." Indeed, Earman (1995: 280–1) points out that:

> [t]he darling of the philosophical literature on . . . time travel is the "grandfather paradox" and its variants. For example, Kurt travels into the past and shoots his grandfather at a time before grandpa became a father, thus preventing Kurt from being born, with the upshot that there is no Kurt to travel into the past to kill his grandfather . . . [,] so that Kurt is born after all and travels into the past.

– and shoots his grandfather . . . , thus preventing Kurt from being born. . . . From this kind of fixation on the part of philosophers of time travel, Earman (1995: 269n.3) draws the (surely correct) conclusion that "the philosophy of science quickly becomes sterile when it loses contact with what is going on in science."

Yet the reason why the preceding sentence is true, and why we echo it here, is – as Earman (1995: 268) points out – that, "[during the last few years . . . [,] leading scientific journals have been publishing articles dealing with time travel and time machines." For example, just in 1990–2, there were 22 papers on these subject, involving 22 authors, in such highly respected and rigorously refereed journals as *Physical Review D* (11 articles), *Physical Review Letters* (5), *Classical and Quantum Gravity* (3), *Annals of the New York Academy of Sciences* (2), and *Journal of Mathematical Physics* (1 article). That this continuing development is not better known outside of physics is partly due to the fact that some of these papers are camouflaged (intentionally so, though this is less often the case now) because their titles refer to "closed time(-)like curves [CTCs]" and "closed time(-)like lines," or "wormholes" and "causality (violation(s))." But especially more recent articles are not afraid of titles mentioning "time travel" and, much more often, "time machines."

In order to put some teeth into these assertions – with their obvious potential implications for students of language change – we need to provide some hard references to a set of representative articles by physicists which relate to the subject of time travel. Hence we give the following brief list of chronologically varied but mainly recent works: Feynman (1949), Gödel (1949), Everett (1957), Newman et al. (1963), Hawking and Ellis (1973), Tipler (1974, 1976a, 1976b), Morris et al. (1988), Aharonov et al. (1990), Frolov and Novikov (1990), Kim and Thorne (1991), Gott (1991), Hawking (1992, 2000, 2001), Headrick and Gott (1994), and Li and Gott (1998). Selecting just a few of these for more than nominal mention, we can begin with Feynman's (1949) suggestion that the previously discovered positron (from *posi(tive elec)tron* – since it is the anti-particle of the electron) might really be, despite forward-looking appearances, an electron traveling backwards in time. But most later discussions have explored questions at a more cosmic level, and thus in connection with the curved space-times (related to the interpretation of gravity as the warping of space-time by mass) which came to the fore with the publication of Einstein (1916). Gödel (1949) thus proposed a solution to Einstein's field equations for general relativity which was applicable to a rotating (thenceforth "Gödelian") universe composed of perfect fluid at constant pressure – a place where space-time shows natural instances of closed time-like lines (of the Minkowskian "world lines" sort) which induced Gödel to conclude that "it is theoretically possible to travel into the past."

Similarly, Tipler (1974) builds on earlier work to suggest that a long enough, very dense cylinder rotating with sufficient surface speed would allow the formation of closed time-like lines connecting events in space-time, reasoning that, "if we construct a sufficiently large rotating cylinder, we create a time machine." Morris et al. (1988) invoke subatomic considerations and argue that the quantum "foam" filling space-time must contain tunnel-like "wormholes" allowing virtually instantaneous travel between the regions connected by them – regions existing in different time periods – so that time travel is probable under certain conditions. Aharonov et al. (1990), in turn, use a major principle of quantum mechanics (that certain particles can exist in various states simultaneously until they are observed) in proposing to build quantum-mechanical "balloons" which exist simultaneously in all their possible sizes and whose occupants must therefore simultaneously exist in many different rates of time – with this allowing particles to be sent into their own past. Gott (1991), on the other hand, showed for any two sufficiently long, dense, straight, but also extremely thin cosmic strings (presumed relics from the Big-Bang origin of the universe) that, if they approach each other from opposite directions and pass each other at high speed, then this should warp space-time via the formation of closed time-like loops encircling the two strings, thereby allowing observers to travel into their own past.

There are three reasons why there is no need for linguists, even diachronicians, to be at all put off or frightened by physicists' time-travel research along these lines. First, there are many books (and a few articles) which present excellent summaries and discussions of the above-mentioned articles and so make it less pressing to consult the original texts (or direct reprints thereof). Relevant here are, more generally, Hawking (1996), Parker (1991), Thorne (1994), Kaku (1994: 232–51 et passim), Price (1996), Novikov (1998), and Ehrlich (2001: 146–71 et passim), but most of all (because more specifically) Earman (1995) – a model of both concision and thoroughness already extensively quoted above – as well as Pickover (1998) and especially Nahin (1999), a volume of awe-inspiring breadth and depth. Nahin (1997), on the other hand, is devoted to apprising literary authors that some of their ideas which were once only fiction are now science, and Simpson (1996) is a posthumously issued but (in general) still paleontologically sound example of a science-fiction novel by a major figure in evolutionary biology. Second, neither the conclusion that time travel cannot be shown on theoretical grounds to be impossible in principle (accepted by a large number of physicists) nor the stronger claim that time travel can be shown on theoretical grounds to be possible in principle (accepted by a smaller but still impressive number of physicists – though not, e.g., by Hawking 1992) forces us to believe that time travel as a practical reality is achievable at present or will be so in the foreseeable future. Third, even if the theoretical possibility of time travel should someday become realizable in the distant future, the earliest periods that will thereby become visitable are likely (on most theories) to be ones close to the departure date of the relevant travelers, and thus much *later* than our own time. Given their significance, we next briefly address the second and third points just mentioned.

As for establishing that practical considerations now render impossible even theoretically imaginable forms of time travel like the above-mentioned proposals from the recent physics literature, we believe that two observations should suffice. First, in the paragraph prior to the immediately preceding one, we have used the word *sufficient(ly)* in places where the original works used either the term *infinite(ly)* or an astronomically high number. Hence Tipler's (1974) rotating cylinder must be infinitely long and turn at at least half the speed of light, whereas the fastest speed currently achievable is less than one tenth of light speed. And Gott's (1991) passing cosmic strings not only must be infinitely long but also must (on one interpretation) move almost at the speed of light. Second, the infinities and astronomically great speeds (and densities) involved in these scenarios do not seem to bother physicists much, since the latter seem much more concerned with "the principle of the thing." Thus, for example, Nahin (1999: 370n.13) emphasizes that Gödel (1949) himself calculated the necessary speed of his potential time travelers as 71 percent of the speed of light and assumed that, if the needed rocket ship could "transform matter completely into energy," then the weight of the fuel would be greater than the rocket's weight by a factor of ten to the twenty-third power divided by the square of the duration (in rocket time) of the relevant travel as measured in years. But Gödel's point, as Nahin (1999: ibid.) stresses, was that, despite the "formidable numbers" involved, "they require no violation of physical laws, and that is what really . . . [would be needed] if time travel is to be disproved."

For present purposes, then, the finding that time travel is both completely impracticable now and also likely to remain so for quite some time means that historical linguists can heave a mixed sigh of relief and disappointment. On the one hand, individual diachronicians of language can be fairly sure that the linguistic work on past times which they have achieved at second hand (i.e., at a later date, usually a much later one) will probably not be drastically overthrown by a returning time traveler who has had first-hand experience with the same speech-community. Neither do historical linguists need to fear that their best work will be obviated if a traveler back in time succeeds (as long as the usual paradoxes can be avoided) in inducing the speakers of the relevant speech-community to adopt new changes – say, as innovations common in speech (and thus audiotapable by the time traveler) but never used in writing – which contradict the way in which the language has been reconstructed from documents. Nor, lastly, is there any reason for Indo-Europeanists to torture themselves with the thought that the ancestral language to which they devote so much time was not wholly an outgrowth of its earlier past, but instead might have arisen when, say, Eric Hamp passed through a time warp and (again *pace* the usual paradoxes) unknowingly created PIE by talking to speakers of some other language while he thought he was doing fieldwork on Albanian (which, at least in this fantasy, might originally have been a language isolate). On the other hand, the present and foreseeably future impossibility of time travel as a practicable option means that, as we have repeatedly stressed here, there is essentially no hope (barring rarities equivalent to the discovery and decipherment of Hittite) that any particular reconstruction of an unattested language (state) will ever be absolutely confirmed – that is, that Jane or John Doe will ever be entitled to write, for example, 100% (JD) potis for PIE "powerful" or the like.[113]

At the same time, the other (third) point mentioned further above – the probability that even the time travel which could become practicable far in the distant future would most likely be limited to visiting time periods which are closer to a traveler's moment of departure, rather than (to) today's present (2002) and/or earlier times – also bears some useful implications for today's diachronic linguists. Relevant here is the fact that many of the space-time-related scenarios for travel through time involve one person (or set of persons) who moves faster than another person (or set of persons). This is because, via the Einsteinian phenomenon of "time dilation," time progresses more slowly at higher rates of speed (i.e., time effectively compensates for motion) – indeed, for a person who could somehow travel at the speed of light, time would actually stop. But, for a relatively stationary person (or set of persons), there is no time dilation, and so someone traveling away from such stationary person(s) at near light speed would return to find that she or he in some sense represented their (slight) past, since less time would have passed for her or

him (as a traveler) than for the other(s). Yet, here, the traveler cannot meet up with her or his own past (in the sense of the time before she or he started traveling). Because similar phenomena tend to hold for many of the physicists' time-travel models listed above, the strong overall trend is that these scenarios generally are incapable – even theoretically speaking (quite apart from practical matters) – of taking anyone back into a past prior to today's present (2002). There simply seems no earthly way for Indo-Europeanists to gain direct access to their ancestral object of interest, even by time travel.[114]

Yet, as we have already mentioned several times in previous sections (and will stress again at the end of this entire introduction), there are already independent reasons to study the present as a source of information regarding language change, given that (i) we have greater and more varied access to the present than to any other time, and (ii) all that one has to do in order to have the present turn into the past is to wait. In a nutshell, then, this relatively brief consideration of the possibilities of time travel within modern space-time physics has shown that even this once-science-fictional (but now theoretically science-factual) phenomenon still does not permit access to the language states which constitute the primary interest of most historical linguists, but instead provides an additional reason to concentrate on the present as a valuable source of data bearing on linguistic change as well as linguistic variation. But, as for the possibility of absolutely validating reconstructions proposed for, say, *c.*3,000 BC, *c.*5,000 BC, or even longer ago, it is this fond hope which is most likely to remain the stuff of films and novels. Still, it is revealing to return one last time to the matter of why the data of such ancient times (as well as of more recent ones) are so much less accessible to us, and especially why it is not possible to reconstruct (verifiably) the past in anything close to its original detail – since, if we could do so, we truly would be entitled to claim that a certain past time and state now exist (again) in some place.

A resolution to this question begins to emerge once we concede that, for all its humor, *The Hitchhiker's Guide to the Galaxy* (Adams 1980) is entirely correct when it emphasizes (p. 76) just how "vastly hugely mind-bogglingly big" the universe is – and not just how big space is, but how much there is in it. That is, we need only consider, for a given instant, (i) the total number of all the subatomic particles within all the atoms in all the molecules of the entire universe and (ii) the fact that this universe of particles can be viewed as standing in some overall relation to one another. It is beyond belief that this whole universe of particles could possibly be identically configured at any two moments, given the complexity and sheer volume of what would have to remain constant (and the ante is only upped further if we bring in anti-particles, on which cf., e.g., Greene 1999: 8–9). Once we delve into micro- as well as macro-levels, therefore, it must be the case that, from each instant to the next, the universe is changed into a unique new state. Thus, for an earlier time to be (re)constructed as a place, or to be fixed so as to be visitable as if it were a place, one would really have to realign every bit of matter at every level and every state of energy (even those entities, like gases, which are defined, in their ideal state at least, by random movements of constituent particles). The implications of this conclusion surely are directly relevant for all forms of scientific and historical study, among them historical linguistics.

We turn once more to Hockett (1985: 336) for a characteristically insightful observation in this regard:

> Some . . . events . . . are in principle unobservable in detail. If . . . [one] spill[s] a bowl of sugar, is it possible to have recorded the exact positions of all the grains in the bowl before the spill so that, afterwards, they can all be carefully picked up with tweezers and restored exactly to their former positions? If . . . [one] pour[s] a spoonful of sugar into . . . [one's] coffee, can any record be made of the exact sequence in which the grains – or the molecules – dissolve? Can one label a molecule without destroying it? Can one determine the exact number of cells in a particular human brain, or the exact number of stars in our galaxy? . . . As we contemplate smaller and smaller things, or more and more numerous aggregates, we pass eventually through a hazy boundary beyond which precise determinations are both impossible and unimportant . . . [–] *because* they are impossible.

The view of time that is most consistent with these observations is the one in which time is basically a process – or collection of processes – transforming one state of the universe into another (an approach that should be acceptable even to the many linguists who do not otherwise posit transformations, since it does not really involve movement from one state to another). But, if time is indeed the continual transformation of states via processes, then it can also quite justifiably be described as literally destructive (or, at a minimum, deformative) in its consequences, since time's effects make the universe as a whole unrestorable from one state to the next, at least given our current understanding concerning the (un)likelihood that substantial portions of the particulate universe will be manipulable by human or other agents in the foreseeable future.

That is, taking seriously the vastness of the universe and of all the matter in it makes it clear why restoring or recreating the past, as well as conceiving of it as a fixed place to be visited in confirmation of hypotheses formulated in the present, is impossible and really no more than an illusion. This last point is especially important, because it gets to the heart of what we do as historical linguists, and what we actually study when we do historical linguistics. We thus end this section with a closer consideration of this very point.

1.3.3 *Whence reconstruction?*

There clearly exists a strong human inclination – of nostalgic origin, perhaps – to try to recreate or at least glimpse the past: consider, for example, the willingness with which laypeople (i.e., non-linguists) accept such notions as the reputedly unchanged survival of Shakespearean (= Elizabethan or early Jacobean) English into modern times somewhere in the Great Smoky Mountains of Tennessee or on a remote island off the Virginia Coast.[115] Some such drive,

it appears, is what leads so many linguists – and so many historians in general – to attempt reconstructions of the past. It is also clear that a minimum of reasonable inferences can indeed be made about the past, including the linguistic past; sometimes, indeed, historical material is available that seems to provide a direct "window" into (or at least onto) the past. We have in mind here such phenomena as the aftermath of cataclysmic events like the eruption of Mt Vesuvius in AD 79 or certain kinds of shipwrecks. Regarding the latter, it is particularly appropriate to cite the description by Goodheart (1999: 40) – since, in the opinion of that author (a polar opposite of this introduction's two authors in his degree of historical confidence), "everyone agrees that":

> for all intents and purposes, the deep oceans remain a closed time capsule. And every indication is that it is an exceptionally rich time capsule – archaeologically as well as monetarily. The value of shipwrecks generally, besides what they have to tell about maritime history, is that, unlike most land sites, each freezes in time a particular moment of history, the moment of its sinking. Each is, in a sense, a small-scale Pompeii. And . . . [,] like the ash of Vesuvius, the ocean can, under certain conditions, be an extraordinary preservative environment. This is especially true in its cold, lightless depths, where fewer destructive microorganisms live, and where wrecks lie mostly beyond the reach of storms, trawler nets, and scuba divers.

For all their vigor of expression, though, Goodheart's assertions pale next to those of many archeological works designed to appeal to general readers. For instance, the dust-jacket of Nick Constable's (2000) *World Atlas of Archeology* confidently alleges that "[a]rtifacts, relics, bones, and ruins provide us with *first-hand* evidence and irrefutable proof of the practices of historic civilizations . . . [, f]rom the pyramids of Egypt . . . [onward]" (emphasis added). Here, one is tempted to respond that, yes, we can certainly have first-hand contact with any of the relevant objects that have survived into the present – but by what means (other than time travel, which we have seen to be currently a practical impossibility) could we gain literally "first-hand evidence of . . . historic civilizations"? Similarly, in 1998, as part of their "Ancient Voices" series, a consortium of the BBC, The Learning Channel, and Time-Life jointly issued a video, titled *The Secret of Stonehenge*, whose accompanying description invites its viewers to see lost worlds "brought to life again through state-of-the-art virtual reality reconstructions, stunning location-filming and evocative reenactments." Perhaps the makers (and viewers) of such productions think that, as long as enigmatic relics from earlier times are "brought to . . . life," it does not really matter much whether such reconstructions and re-enactments closely correspond to – that is, bring *back* (to life or to cloned imitation) – anything that was once real and true.

In this regard, introductory books and films about paleontology tend to be more honest and up-front regarding the degree to which they reflect the filling-in of fragmentarily preserved remains via present-day conjecture. The following rather frank admission has been made (cf. Gibson 1999) by Tim

Haines, producer of the three-hour, 9.6-million-dollar BBC mini-series *Walking with Dinosaurs*, which was watched by 13.2 million British viewers (one fourth of the UK's population) and later shown in the US by the Discovery Channel (in April of 2000): "All paleontology requires you to interpret something that's dead. . . . This series is our best guess and the best guess of some very intelligent scientists" (the latter being eight well-known paleontologists).

It is not entirely clear why there should exist greater diffidence in paleontology than in archeology concerning the details of reconstructed entities, but one possibly relevant factor may be paleontologists' need to flesh out many extinct creatures based solely on remains among which few or no traces of soft tissues have been preserved. Thus, one can see (in museums) reconstructions of dinosaurs whose feathers and purple skin are clearly labeled as speculative in accompanying descriptions. This can be contrasted with current practice in so-called "anthropological archaeology," a tradition within which a work like Wells (1999) confidently maintains that the artifacts dug up from large pre-Roman settlements in Western and Central Europe suffice "to show just how complex native European societies were before the [Roman] conquest," with "remnants of walls, bone fragments, pottery, jewelry, and coins tell[ing] much about . . . farming, trade, religious ritual . . . [, and other aspects of] the richly varied lives of individuals." Here, there appears to be a stronger temptation to fill in cultural gaps by extrapolating from the wealth of ethnographic material known to be available from myriad nineteenth- and especially twentieth-century studies of contemporary peoples. In this regard (a point to which we return below), practitioners of linguistic reconstruction seem to show degrees of confidence closer to those of anthropological archeology than to those of paleontology.

There is another possible reason why paleontologists tend to be less vehement in promoting their reconstructive work, and this has to do with past embarrassments caused by (aspects or wholes of) detailed concrete reconstructions of some creature which were first confidently proposed but then ignominiously withdrawn. One of the most notorious cases of this sort has to do with the spike of *Iguanodon*, a large plant-eating reptile whose fossil remains were discovered in England in the 1820s and led to its becoming only the second officially named dinosaur (in an 1825 publication; for thorough discussion of these and related facts, see Wilford 1985: 27–31, 56–65, 78–84, 129–32).

British physician Dr Gideon Mantell, who (along with his sister) had found the fossils and who first described them, made two major wrong assumptions about *Iguanodon*: (i) he thought that the animal had walked on four legs, like an oversized iguana, and (ii) the fact that only one spike-fossil had been found led him to mistake the dinosaur's spiky thumb-bone for a horn. Mantell's drawings thus placed this spike on top of the snout, making the creature look like a rhinoceros, and his sketch was later taken as a blueprint when, in the 1850s, a sculptor was hired to "revivify . . . the ancient world" by shaping cement, stone, bricks, and iron into life-size restorations of *Iguanodon* and other dinosaurs. The resulting *Iguanodon* looked like a reptilian rhinoceros, with its on-all-fours posture and a spiked horn for its nose – errors which remain for all to see today, since the huge sculpture at issue is still to be found in a park at Sydenham on the outskirts of London. Soon, however, Thomas Henry Huxley noted the resemblance of *Iguanodon*'s hindquarters and three-footed toes to those of birds, therefore arguing that this dinosaur must have been capable of erect posture and able to hop or run on its hind legs, a prediction that was eventually confirmed. In 1878, moreover, coal miners in Belgium stumbled on 30 nearly complete *Iguanodon* skeletons, from which it became clear that the above-mentioned spikes appeared in pairs and came from the front/upper limbs – since they were in fact thumb bones, not nose horns. Such cases of egregious (but fortunately only temporary) misreconstruction by paleontologists of the nineteenth century should lead us to ask whether there exist any rough parallels in the field of historical linguistics which can serve as similar caveats, especially because archaeology also has its share of corresponding examples.

For example, in an engaging conversation with an unusually knowledgeable interviewer – cf. Miller 1995 – which was published not long ago, Egyptologist and curator Emily Teeter (now also co-author of Brewer and Teeter 1999) mentioned (p. 9):

> a famous boo-boo . . . in Egyptology . . . where things have been completely misinterpreted . . . [, one involving some] little knives . . . which people used to say were ritual circumcision knives with a . . . wonderful mystique about them. It turns out they're just plain old razors for scraping faces. When you're not quite sure, the cult significance can get built up tremendously [so as] to make it fit into . . . [some] magical, mysterious sense of Egypt . . . If you spend enough time going through the publications or . . . the tombs, it's very likely you'll find a picture of somebody holding one of these things up. And very likely the pictures are accompanied by a hieroglyphic caption, just like in comic books. So if you're not quite sure . . . [,] you read the caption, and it says "razor for cutting hair."

In this instance, a mistaken interpretation involving the reconstruction of cultural behavior was avoided due to the fortunate discovery of label-like writing on or near (a picture of) an artifact. In cases where there are no (decipherable) inscriptions, however, archeologists (as well as diachronic linguists) are left rather in the dark, and their speculations are inherently less constrained. The attendant pitfalls are well enough known in Egyptology that scholars like Teeter find it salutary to challenge one another with occasional invocations of David Macaulay's satirical (1979) book *Motel of the Mysteries*, whose premise is that, sometime in the distant future, two amateur archeologists unearth an ordinary US motel and then proceed to misinterpret it complely by treating virtually every item unknown to them as a cult object – with a television set being analyzed as "the great altar" and a toilet bowl as "the sacred urn." Given that historical linguists are at least dimly aware of real gaffes nearly as extreme as these in the parallel fields of archeology and paleontology, can we ever be sure that some or even many of our linguistic reconstructions will not turn out, in retrospect, to be outrageous or ridiculous? And, for that matter, are there any unmistakable tell-tale signs of an outrage- or ridicule-provoking reconstructed language form?

Actually, there are some fairly well-known reconstructive examples from the middle of the nineteenth century which are so extreme in nature that they now function almost as advertisements for how not to do reconstruction. As discussed, for example, by Kiparsky (1974b) at some length, the German classicist Curtius (1877) and certain earlier Indo-Europeanists (grouped by Kiparsky as "Paleogrammarians" in order to set them off from the later, better-known Neogrammarians) applied a kind of semantically based reconstructive operation to PIE. Thus, 1.pl. pronominal forms were assumed to be a conjunction of 1.sg. + 2.sg. pronominal forms, whereas the assumption for 2.pl. forms was that they conjoin 2.sg. + 2.sg. In addition, active-voice person-endings of verbs were treated as simply tacked-on personal pronouns, while the endings of PIE's so-called "middle" voice were assumed (since the latter was a somewhat reflexive-like structure where a subject acts on his or her own behalf, and thus affects himself or herself) to be essentially double-pronominal, and so to consist of reduplicated active-endings.

Hence Curtius proceeded logically from the agreed-on 1.sg. pronoun and active-(ending) *ma*, and from the 2.sg. pronoun and active *tva* (the use of asterisks for reconstructions was not yet obligatory), to 1.pl.act. *ma-tva* and 2.pl.act. *tva tva*, and from there to 1.pl.mid. *ma-tva-tva* and 2.pl.mid. *tva-tva-tva*, with the latter two showing partial reduplication (of only the last element of the corresponding active-ending). In this, though, Curtius was distancing himself from August Schleicher's (1861–2) even more repetitive-seeming earlier reconstructions (likewise semantically based), with their noticeably full(er) reduplications: cf., for example, the 1.pl.mid. suffix as Schleicher's PIE *ma-tva-ma-tva*, or his even more relentlessly logical reconstruction of the PIE 2.pl.mid. suffix as *tva-tva-tva-tva*. Today, however, both Curtius's and Schleicher's reconstructive proposals of this sort stand out like a sore thumb; they are now viewed as rather bizarre. Yet, at the time, Schleicher did not hesitate at all to publish bold suggestions regarding reconstruction, and thus Schleicher (1868) caught considerable flak even from his Paleogrammarian colleagues (and especially from his Neogrammarian successors) for attempting to write a short fable in his version of (heavily Sanskrit-leaning) PIE, although some twentieth-century scholars have dared to follow his example (e.g., Hirt, as cited in Jeffers and Lehiste 1979: 107–8, and see also Lehmann and Zgusta 1979).

Admittedly, the above primarily semantics-driven nineteenth-century reconstructions stand out by their combination of length and brute-force repetition, but we believe it necessary to repeat the question: how do we really know today whether a given reconstructed form is accurate or even plausible? With no practical chance in sight for verification via time travel, most proposed reconstructions would in fact seem to be inherently incapable of direct verification – either pro or con. And this, in turn, explains the justification behind the suggestion that reconstructions are inversely related to treason. That

is, whereas Har(r)ington (1618, quoted from 1977: 255) penned the rhyme that "Treason doth never prosper: what's the reason? / . . . [I]f it prosper, none dare call it treason," we can turn this around as follows: "Reconstruction doth ever prosper; what's the reason?/No one from the past returns to call it treason!"

Summarizing so far, then, we find that, despite the considerations discussed in this and the preceding sections, much current (as well as earlier) research in diachronic linguistics still harbors an implicit – even, on occasion, explicit – presupposition that reconstructions and historical inferences can somehow be definitively verified,[116] and talk of allegedly "frozen" time states would certainly feed such a belief, as would the view (discussed in most detail in the previous section) according to which time states might have a spatial existence (if only in some other "dimension"). Yet, as discussed above, there will always be myriad aspects of the past which must remain unknowable, and hence verification can be at best a relativistic enterprise. Moreover, and more importantly, though, it needs to be asked just what is being studied in such "reconstructive" work – is the past really the object of study, or, rather, pieces of a present? Collingwood's (1946, here quoted from 1993: 484–5) discussion of this point focuses on historians' task in dealing with their evidence:

> [Historical] records, which may be of various kinds – . . . [dispatches,] correspondence, descriptions by eye-witnesses or from hearsay, even tombstones and objects found on . . . [a] battlefield – are traces left by the past in the present. Any aspect or incident . . . which has left no trace of itself must remain permanently unknown . . . [,] for the historian's business can go no further than reconstituting those elements of the past whose traces in the present [she or] he can perceive and decipher. . . . In this sense . . . [,] history is the study of the present and not of the past at all. The documents, books, letters, buildings, potsherds, and flints from which the historian extracts . . . all [she or] he can ever know . . . about the past . . . are things existing in the present. And . . . [,] if they . . . in turn perish – as, for instance, the writings of . . . historian[s] may perish – they . . . in turn become things of the past, which must leave their traces in the present if . . . [historians are] to have any knowledge of them. These traces must be something more than mere effects. They must be recognizable effects . . . [–] recognizable, that is, to the historian.

The general and especially economic historian Wallerstein (1974: 9) made this point even more bluntly: "The past can only be told as it truly *is*, not *was*" (original emphasis). In consequence, both linguistic and other diachronicians must label as actually unrealistic and ultimately unattainable the seemingly modest goal stated so famously by the nineteenth-century German historian von Ranke (1824: vi) when he said that a historian "just wants to say how it really was" (in the original: "Er will bloß sagen, wie es eigentlich gewesen [ist]"). Much more realistic – because much more aware of the later biases unavoidably imposed on reconstructions and interpretations of earlier times and things by historians, as well as private citizens – are the remarks of the urbanist and historian Rybczynski (1999: 32–4). Concerning certain gems of eighteenth-century US architecture, that is, Rybczynski pointed out that "[f]amous houses like Monticello and Mount Vernon reflect . . . Virginia planters' dreams of classical Rome, a reminder that a hallmark of the American house is a continuing reinterpretation of history . . . [– o]r perhaps one should say . . . [a continuing] reinterpretation of the past, a past that is both real and imaginary." And reinterpretations (like language change) always take place in the present, ultimately on the basis (or at least under the influence) of present phenomena – a point made with admirable clarity, cogency, and concision in the following statement by Collingwood (1946/1993b: 110):

> [H]istorical thinking . . . is . . . based on the assumption . . . that there is an internal or necessary . . . [connection] between the events of a time-series such that one event leads necessarily to another and we can argue back from the second to the first. On this principle, there is only one way in which the present state of things can have come into existence, and history is the analysis of the present in order to see what this process must have been.

In this regard, a useful caveat is provided by Bertrand Russell's thought-experimental point that even events which we have personally experienced do not exist in some special past-space, but only in our present memories, and that these are subject to all sorts of interfering factors. Russell's (1921: 159–60) dramatic example is worth quoting at length (with the original emphasis):

> [E]verything constituting a memory-belief is happening *now*, not in that past time to which the belief is said to refer. It is not logically necessary to the existence of a memory-belief that the event remembered should have occurred, or even that the past should have existed at all. There is no logical impossibility in the hypothesis that the world sprang into being five minutes ago, exactly as it then was, with a population that "remembered" a wholly unreal past. There is no logically necessary connection between events at [non-contiguous] different times; therefore nothing that is happening now or will happen in the future can disprove the hypothesis that the world began five minutes ago. Hence the occurrences which are *called* knowledge of the past are logically independent of the past; they are wholly analysable into present contents, which might, theoretically, be just what they are even if no past had existed. . . . I am not suggesting that the non-existence of the past should be entertained as a serious hypothesis. Like all sceptical hypotheses, it is logically testable, but uninteresting. All . . . I am doing is to use its logical tenability as a help in the analysis of what occurs when we remember.

It thus cannot be overemphasized that, in studying the past, no scholar of any kind, whether historian or historical linguist, has direct access to past states; rather, the most that anyone can consult is those aspects of the present which can be interpreted as suggesting something about an earlier present which we call "the past." When we reconstruct, therefore, we are indeed really dealing with the present and using it to speculate about the way things were in past

states. In this way, much of what any historian does is really akin to linguistic internal reconstruction (see again RINGE's chapter 3), since that methodology involves working back to past (earlier) linguistic phenomena on the basis of language data drawn from a later, more contemporary synchronic state – that is, from the historian's present, more or less.

Yet, even with this methodology, there are sometimes chasms that cannot be bridged. An instructive linguistic example is the history of Modern English *went*. If one looked only at *go/went* in present-day English, one might be inclined to think that there had been an earlier time when there was some other, less irregular pattern. For example, one might conjecture that *go* originally had no associated past tense (i.e., was a praesens-tantum verb), but that the accretion of the past form *went* onto *go* introduced suppletion into the picture. Or it might be speculated that *go* earlier had a (more) regular past-tense form of some kind, either a so-called "dental preterite" form similar to *goed* – often produced by children learning English as a first language – or a so-called "strong" (ablauting) form similar to, say, *gew*, which follows the *knew* of *know/knew/known* (compare *go/ . . . /gone*). Otherwise, one would probably be most likely to think that the pattern *go/went*, being irregular, reflects the original state of affairs in earlier English and in the language state(s) ancestral to Old English.

Thus, any linguistic analyst with knowledge only of Modern English would be hard-pressed if called upon to deduce the truth here. This is, namely, that there earlier existed a different suppletive past form, as can be seen by comparing Old English infinitive *gān* (the ancestor of *go*) with Old English suppletive past-tense *eode* (with reflexes like *yode* which survived into Middle English before being ousted by what had originally been just the past tense of *wend*, as in *wend one's way*; compare *wend/went* with *send/sent*). That is, one suppletive paradigm has been replaced by another, without any trace of the earlier suppletive form surviving into subsequent synchronic language systems. Only the accident that information about the past tense of 'go' in Old English is still available today, in texts that have been preserved and studied – that is, texts which really represent facts about the present state of affairs concerning our knowledge of Old English – reveals this truth about that earlier state. Without specific knowledge of suppletive *eode*, nothing certain or even approximately accurate could have been achieved by conjectures that propose an ancestral form for the suppletive past-tense part of English *go/went* solely on the basis of internal reconstruction.

Besides the often insurmountable barrier posed by suppletions which replace suppletions, as in the example just summarized, there are two other problematic aspects of reconstruction that deserve at least brief mention (for discussion of other reconstructive difficulties, cf. such works as, e.g., the masterful study of etymology by Watkins 1990).

First, there is the problem of (non)simultaneity – which, given its intersection with notions like (linguistic) structure and system, receives far too little discussion in the literature on language change and reconstruction. The first horn of the dilemma faced by historical linguists on this score is that, given the huge number of postulated proto-elements often involved in attempts to arrive at a reconstruction for an entire proto-language, there is often extremely little evidence available from attested, later languages or dialects as to the relative chronology of different reconstructed elements; that is, for any two reconstructed entities, whether (and, if so, for how long) they occurred at the same time in the proto-language – which should be no less characterized by shifting configurations of elements, especially lexical ones, than any modern or otherwise attested language. (There is, after all, no such direct evidence for (non)simultaneity available from the actual time of the real but unattested language whose reconstitution is being attempted.) Yet, on the other hand (and horn), when considerations of structure and system are brought in as helpful factors for organizing the many and varied phenomena of a reconstructed language state, it is rarely obvious that the number of simultaneously present proto-elements which has been securely established is large enough to justify the conclusion that a particular system was present at any one time (and could thereafter serve as a guide for resolving the status of ambiguous elements, filling in gaps, and the like).

Typology is frequently appealed to, of course, as a way to resolve chronological and other difficulties of a reconstructive enterprise, but the abuses to which typology has been put in the name of reconstruction (especially for syntax) have already been emphasized here (in section 1.2.1.7). It must thus be concluded that the dilemma of proto-(non-)simultaneity remains a major bane of reconstruction efforts by historical linguists, and that probably the most common situation is for diachronicians to have evidence only that a certain number of reconstructed elements all probably have occurred in the proto-language at some point in time, but not necessarily the same point – so that, in unlucky instances, one is stuck with basically a laundry list of proto-items floating together in a temporal wash.

A second and much more general problem of reconstruction – albeit one which receives even less attention in the literature than does (non-)simultaneity – involves not a dilemma but a paradox. Namely, given the frequency and earnestness with which historical linguists tend to talk about seeking explanations for synchronic phenomena in the past, via diachronic investigations of change,[117] it seems ironic that reconstructed proto-languages[118] are the only language states which have no real past (since the only thing that can be immediately prior to a proto-language is another proto-language – arrived at via, e.g., internal reconstruction). One consequence of this fact-cum-irony is obvious and not infrequently commented on. That is, since virtually every attested language state having an attested subsequent history is known to show some linguistic variants which do not appear in any later language states, it must surely also be the case that virtually every proto-language must have included certain aspects of language which were not passed on to any of its descendants. But, in that case, such variants are inherently unrecoverable – although this would obviously not be true if (*contra hypothesem*) we possessed a past for the relevant proto-language.

Also relevant here, however, is the fact that using reconstructed entities to explain their subsequent reflexes (and the changes relating them) is essentially circular, because the (changes and) later forms which reconstructions (and changes) are sometimes claimed to explain are themselves the basis for the reconstructions (and attendant changes) in the first place. There is no way around this, of course – as had been said, virtually everything in science is ultimately circular, so the main thing is just to make the circles as big as possible. Nevertheless, we must still remind ourselves how easy it is to be misled into thinking that reconstructions and related changes provide an essentially complete explanation for their reflexes/consequences, whereas it would be much healthier for diachronicians of language to ask themselves more frequently: "What did I learn from carrying out this reconstruction of a proto-language that I really didn't already know from studying the data found in its descendants?" We emphasize these issues because it is well accepted in non-linguistic historiography that the best explanations push inexorably from the facts of earlier times to the events of later ones, as it were, rather than pulling prior facts forward toward the present on the basis of already-known subsequent outcomes. This point has been made forcefully by Weinberg (1994b: xv) – as also via other forums – in a way that is directly relevant to issues of circularity and explanation in linguistic reconstruction:

> A . . . special problem appears . . . to affect much of the literature on the . . . [Second World W]ar. It is too frequently forgotten that those who had choices and decisions to make were affected by memories of the preceding war of 1914–1918, not by the Cold War, the Vietnam conflict, or other issues through which we look back on World War II . . . [. But t]hey did not know, as we do, how the war would come out. They had their hopes – and fears – but none of the certainty that retrospective analysis all too often imposes on situations in which there were alternatives to consider, all of them fraught with risks difficult to assess at the time. The [present work makes an] effort to present the war in a . . . perspective looking forward rather than backward, and to do so at least in part on the basis of extensive research in the archives . . . [– a pursuit which is truly] challenging.

Alas, in the case of proposals regarding proto-languages themselves (as opposed to their descendants), it is precisely archives which we do not possess. It was partly the fact that information about unattested earlier language states is so often extensively obliterated by subsequent changes which led Schleicher (1848–50: ii. 134) to speak of "history, that enemy of language" (in the original: "die . . . [G]eschichte, jene . . . [F]eindin der . . . [S]prache").

The relative degree of this obliteration – this destruction (to which we earlier referred in section 1.2.1 above) – is in fact the critical element in the study of all diachrony, linguistic or otherwise. We have already referred to Hockett's (1985: 336) observation that much of the past is unrecoverable partly because it would have been virtually impossible to record it all synchronically – recall his two related queries: "If . . . [one] spill[s] a bowl of sugar, is it possible to have recorded the exact positions of all the grains . . . before[hand] . . . so that . . .

they can all be . . . restored exactly to their former positions? If . . . [one] pour[s] a spoonful of sugar into . . . [one's] coffee, can any record be made of the exact sequence in which the grains . . . dissolve?" But the most extensive discussion known to us of these issues is that of Sober (1988: 3–5), who in fact actually compares the possibilities for recoverability (and thus, by implication, reconstruction) against the ravages of change in astrophysics, biology, and historical linguistics:

> It is an empirical matter whether the physical processes linking past to present are information-destroying or information-preserving. Indeed, we must fragment the single and seemingly simple question of the past's knowability into a multiplicity . . . [of questions and] ask whether this or that specific aspect of the past is knowable. . . . [No] *a priori* argument . . . show[s] that . . . history must always be recoverable . . . [; w]hether this is true depends on contingent properties of the evolutionary process. . . . [T]he folly would be great . . . [if one] were to try to produce . . . some general philosophical argument to the effect that the past as a whole must be knowable . . . [solely on the basis of the present]. The history of stars, of living things, and of human languages, to mention just three examples, . . . [is] retrievable only if empirical facts specific to the processes governing each are favorable . . . [. T]he pertinent questions are local in scope, . . . [and] the astronomer, the evolutionist, and the linguist can each address [these queries] by considering the discriminatory power of available data and [of available] process theories [– i.e., theories mapping from possible initial conditions onto possible subsequent ones].

In this regard, the question of information-destruction versus information-preservation is the central issue, and we therefore initiate the conclusion of this section by presenting Sober's (1988: 3–4) overall treatment of this matter, given its crucial bearing on reconstruction and in fact all aspects of the study of language change (original emphasis):

> [M]apping from possible initial conditions onto possible subsequent ones . . . engender[s] a continuum of epistemological possibilities . . . which reflect . . . whether historical inference will be difficult or easy. The worst possibility, from the point of view of historical science, arises when the processes linking past to present are *information-destroying* . . . [, when] the present state would have obtained regardless of what the past had been like . . . [– since] then an observation of the present will not be able to discriminate among alternative possible pasts. However, if even slight differences in the past would have had profound effects on the shape of the present, then present observation will be a powerful tool in historical reconstruction. . . . The worst-case scenario . . . arises if the system under investigation *equilibrates* . . . [, like] a bowl . . . on whose rim a ball is positioned and released . . . [,] roll[ing] back and forth, eventually reaching equilibrium at the bottom . . . [–] after which nothing can be inferred about its starting position. . . . It is sometimes thought that historical sciences have difficulty retrieving the past because the systems under study are complex, or because theories describing those systems are incompletely developed. Although this is frequently true, matters are otherwise in the present example. It is not the complexity of the system or our inability to produce an accurate theory that makes historical inference difficult in the case of the ball [in the bowl]. It is the nature of the physical process itself, correctly understood by a well-confirmed theory, that destroys information. The fault . . . is not in ourselves . . . but in the bowl. In contrast with this circumstance . . . is a physical system in which different beginnings lead to different end states . . . [: e.g.,] a bowl contain[ing] . . . numerous wells, s[uch] that a ball placed on the rim will roll to the bottom of the well directly.

The major question facing us here, then, is whether or not there are effectively pits in the bowls of data on which the theories and methods of historical linguists are constrained to operate in particular instances. An honest appraisal of the typical situation in linguistic diachrony would, we believe, compel us to admit that our field is less often blessed with pitted bowls and more often cursed with pitted, lacunar texts that represent obliterated information. Yet yeoman efforts by students of language change have often achieved great coups even in the face of recalcitrant texts – for example, via recourse to detecting scratched-out letters by scrutinizing parchment in sunlight, or by using ultraviolet light and other, newer means by which technology can sometimes help us to thwart history's apparent enmity toward language and linguists. Nonetheless, in all of this, one thing above all remains forever true: what we are engaged in at first hand is actually a questioning of the present for what it can tell us about the past, not an interrogation of the past itself.

Thus, any preserved document – even a film or an audiotape-recording (cf. n. 20 regarding an early film in American Sign Language and the general notion of "document') – represents a present-day artifact from which we can infer information about the past. It simply happens to be the case that we are generally convinced that some recording media undergo less degradation over the course of time than certain other means for attempting to make linguistic texts (more) permanent. What we are explicitly denying here is that there are any objects or phenomena in the present which could even "honorarily," so to speak, be considered as belonging to – that is, existing in – the past rather than the present.[119] We can have glimpses on the past, yes, but only through present-day windows.

During the more than two centuries of its modern period, mainstream historical linguistics has tended to take the very view regarding the object of its study that we argue against in this introduction. We have thus attempted to refute it – or at least present a counterbalance to it – by emphasizing the diametrically opposed stance adopted here, so as to sound a caution against falling into what we see as a trap. At the bottom of this trap is, we feel strongly, a fundamentally misguided conception of what it means to deal with the past – one putting forth every indication that its adherents believe scholars to be capable of truly restoring the past, that the reality of the past is directly accessible, and that diachronicians can (and do) study the past literally and first-hand. All of these points contribute to giving some scholars the feeling that, through their reconstructions, they are directly recapturing the past, instead of just formulating generally unprovable, even if compelling, hypotheses about past states, linguistic or otherwise. Yet, paradoxically, all such study really does involve dealing with the present, and so there is surely even more reason (than we have previously discussed) for diachronic linguists to cultivate a focus on language variation and change in the present for its own sake, as well as for the purpose of establishing baselines to allow the charting of linguistic developments in the future, when today's present will have become the past.

Even though we have taken issue, in this section, with various common practices in the field of historical linguistics, we accept full responsibility for the fact that these approaches figure quite prominently in numerous chapters of this handbook. Indeed, we would be derelict in our editorial duty if they did not do so, since the practices in question characterize the way in which much work in historical linguistics long has been, and still is, carried out by many productive scholars (diachronicians who clearly do not share our – possibly idiosyncratic – views on these matters), and since these same practices have, over the years, been used by researchers to achieve some truly stunning successes. That said, we now therefore turn, by way of introducing the main body of the work itself, to a more detailed consideration of the nature of this handbook: what it contains, what it omits, and how to use it.

2 Part the Second: Historical Aspects of the Linguistics in this Handbook

> *Thus saith the Lord . . . [:] Remember ye not the former things, neither consider the things of old. Behold, I will do a new thing; now it shall spring forth; shall ye not know it?*
>
> (Deutero-)Isaiah (*c*.585 BC), from the ["Authorized"] King James Version of Bible (AD *c*.1611)

> *Those who cannot remember the past are condemned to repeat it.*[120]
>
> George Santayana, "Flux and Constancy in Human Nature" (Chapter 12), from *The Life of Reason, Vol. I: Introduction and Reason in Common Sense* (1905: 284)

In the course of our discussion, in part 1 above, of central issues having to do with language and linguistics, change and history, or language change and historical linguistics, we have already had occasion to make reference to many of the chapters in the present volume. Still, more discussion of the book as a whole and of its contents is in order, and this part 2 is reserved for such matters.

2.1 Reconstructing from absences – or, topics to be found elsewhere

> *[S]he is not there . . . , and the entire world . . . seems a negative imprint of her absence, a kind of tinted hollowness from which her presence might be rebuilt, as wooden artifacts, long . . . [disappeared], can be recreated from the impress they have left on clay, a shadow of paint and grain. . . .*
>
> John Updike, "Harv Is Plowing Now," in *The Music School: Short Stories* (1966: 180)

> *Some books are undeservedly forgotten; none are undeservedly remembered.*
>
> W. H. Auden, "Reading," from part 1: Prologue, in *The Dyer's Hand and Other Essays* (1962: 10)

Let us begin by briefly noting what this work does *not* include.[121]

For one thing, this volume contains no chapter devoted solely to lexical diffusion – the putative item-by-item spread of sound changes through the lexicon. Admittedly, this notion has quite a long and continuous pedigree, in that it was already implied, not only by Jaberg's (1908: 6) and Gilliéron's (1912) Schuchardt-inspired dialectological dictum that "Every word has its own history" (see Malkiel 1967 and references there), but also by some post-Neogrammarians' covert recognition in practice (as opposed to theory) that a sound change can be implemented sooner in some words than in others. (For an example, see Prokosch's 1939: 62–7 discussion of Hirt's 1931: 148–55 claims regarding the apparently inconsistent realization of Verner's Law in Gothic.) As a proposed major mechanism of phonological change, however, lexical diffusion was first specifically addressed by Wang (1969), then elaborated on by Chen and Wang (1975), and later discussed extensively by Labov (1981, 1994) as well as, among others, Kiparsky (1988 and subsequently). Our decision to forego an entire lexical-diffusion chapter reflects our belief that, while there often are diffusionary effects in the spread of phonological change through the lexicons of speakers, such effects are actually epiphenomenal, being the result of already-needed mechanisms of analogical change and dialect borrowing. Thus, in our view, lexical diffusion is not a separate mechanism of change, in and of itself.[122] Still, it deserves mention in any handbook-format work on historical linguistics, and, indeed, it is not ignored here, though discussion of it is dispersed across four different places: see chapters 6 and 11 by KIPARSKY and HOCK respectively, as well as chapters 7 and 8, by HALE and GUY respectively.

Similarly, there is no single chapter here devoted exclusively to the use of typological information – already discussed above (in section 1.2.1.7) as a controversial reference point for reconstruction(s) – in investigations of language history and language change. Admittedly, a heavily typological methodology has been employed for reconstructive purposes by, for example, Lehmann

(1974), regarding PIE syntax, and, as noted earlier (in section 1.2.1.7), by Gamkrelidze and Ivanov (1972, 1984), Hopper (1973), and others, regarding the PIE stop system, but their proposals have been tellingly challenged: Lehmann's, by Watkins (1976) and others; Hopper's and Gamkrelidze and Ivanov's, by Dunkel (1981) and others (see n. 37). Still, discussion of these methods, at least in passing, finds a place at various later junctures in this volume: for example, in chapter 1 by RANKIN, and in chapter 2 by HARRISON.

That no chapter here directly addresses what some might consider the ultimate historical question concerning speech – the origin of language itself – is due mainly to the fact that it is not obvious how the standard methodologies of historical linguistics can currently offer anything to illuminate this issue.[123] Rather, an approach to this subject from a multidisciplinary perspective incorporating insights from archeology, cultural and physical anthropology, ethology, evolutionary biology, paleontology, primatology, and many, many other -ologies appears to be indispensable. And, even then, the results remain, of necessity, quite speculative. Still, we do not want to seem as if we wish to revive the famous ban imposed on the topic at issue by the Société de Linguistique de Paris in 1866. Hence we refer all interested readers to Carstairs-McCarthy (1999, 2001) for highly readable discussions concerning the origin(s) of language, and to Callaghan (1997) for a review of recent books dealing with the relevant issues. See also the more specialized treatments (focused on particular issues and/or adopting particular viewpoints) in Armstrong et al. (1995), Beaken (1996), Calvin and Bickerton (2000), Hurford et al. (1998), Jablonski and Aiello (2000), and Sykes (1999), as well as Hauser's (1996) much broader perspective in *The Evolution of Communication*; all of these works provide extensive references to earlier literature.

Further, due to an omissive trend in the field that comes close to being a global gap, there is no discussion here of diachronic pragmatics per se – for example, of changes in the types of inferencing used by speakers to extract meaning from contextually embedded utterances,[124] or possibly in the frequency of direct versus indirect speech-acts within certain types of interactions, or the like. Nonetheless, some of the chapters in this volume do discuss various aspects of change that are closely tied to matters of real-world context and/or pragmatic setting, and so they offer at least a tip of the hat to historical pragmatics. For example, in chapter 20, by TRAUGOTT, grammaticalization is approached with a focus on forms as used in discourse – and thus as rooted in pragmatic context – while, in chapter 21, FORTSON discusses changes in lexical semantics that have their origin in facts concerning alterations in the real-world use of words (or even in the real world itself). Still, diachronic pragmatics is certainly not as well-developed an area of research as many others treated more systematically in this volume – for example, phonological, morphological (especially analogical), and syntactic change – for each of which the relevant literature is vast and reflects well over a century of research.[125]

There is one area of study that certainly has the potential to provide instructive examples of change involving pragmatics, but it is here subsumed under a rubric which likewise receives little discussion in this volume, and for compelling reasons – ones having to do with linguistic characteristics that (outside of punctuation) are rarely, if ever, represented in writing. In particular, intonational change can often be linked with pragmatic factors, since pragmatic contexts are regularly (if not invariably) linked to the meanings and functions associated with particular intonational contours. Thus, the handful of existing studies summarized in Britain (1992) – including Ching (1982), Guy et al. (1986), and James et al. (1989); cf. also McLemore (1991) – are all initial contributions to an understanding of intonational change, though it is clear that much more information is needed about the form and function of intonation in prior language states before we can conclude that any interpretations assigned according to contemporary usage truly represent innovations vis-à-vis earlier patterns.[126] And intonation is far from being the only prosodic phenomenon which, because of its infrequent (direct) indication in writing, it is difficult for historical linguists to trace over time.

Thus, as an additional topic about which little is said here, prosodic change more generally (and not just intonation) should be flagged for an additional word of explanation. As noted above regarding intonation, this comparative gap stems partly from the relative paucity of relevant written evidence, in that there is often no marking in texts and earlier documentation to hint at what the full extent of prosodic information can be (a small sample would include length, moraicity, syllable- and foot-structure, stress- or pitch-accent, and tone). Still, there is admittedly no shortage of specific works on historical accentology and other aspects of prosody,[127] though general surveys are much fewer in number.[128] However, on the one hand, prosodic change seems fully tractable in terms of analytical methods and notions that, by now, are time-tried for other aspects of phonological change (e.g., the comparative method, regularity of sound change, social mechanisms governing the spread of innovations, etc.), so that there is no apparent need for a distinct subfield of "diachronic prosod(olog)y" (though Page 1999 takes a somewhat contrary view). And, on the other hand, there is as yet so much to be learned about the physical realizations and formal patterning of synchronic intonational curves and other prosodic phenomena that we may actually still be in the same position that we are with diachronic pragmatics: that is, the present lack of data may enforce, at a minimum, one or two generations of waiting until two or more richly described contiguous points in time are available for comparison. Nevertheless, insofar as changes involving traditional prosodic phenomena like length are well or at least better understood, they are here dispersed among the various chapters on general aspects of phonological change.

In addition, there is no extended discussion here of glottochronology, a method which attempts to determine the length of chronological separation between related languages by comparing the extent to which they share "basic" or "core" vocabulary.[129] It is true that some textbooks on language change – for example, Anttila (1989), Lehmann (1992), Fox (1995), Trask (1996), and Crowley (1997) – include substantial sections or even entire chapters on the topic. Still, we have been content to let the admittedly brief mentions in chapters 1, 2, and 4 – respectively by RANKIN, HARRISON, and CAMPBELL – suffice, due to our moderate doubts as to the utility of glottochronology, other than in very rare circumstances,[130] and our strong doubts concerning its basic premises. In particular, the method's crucial reliance on a relatively constant (average) rate of vocabulary replacement over millennia seems to presuppose that speakers somehow possess or can gain occasional access to a diachronic perspective on where they and their core-vocabulary items "are" (relative to earlier speakers and speech-forms) within a chronological span over which a certain number of innovations are expected – not too many more, not too many less. But we doubt that anyone has access to the historical information which would be needed in order to obtain and (unconsciously) act on such a perspective, and we are not aware of any external forces which could otherwise guarantee that vocabulary replacement should proceed at a constant rate over a thousand years.[131]

Mention of glottochronology brings to mind another, related area which, after some deliberation, we chose not to include in this volume: namely, the whole enterprise usually referred to as "linguistic pal(a)eontology." This (sub)field has to do with how linguistic evidence can be brought to bear on (or be correlated with what is known about) cultural reconstruction – that is, it concerns the relationship between linguistic reconstruction and what is known (or at least believed) about the material culture of (specific) ancient peoples: what they ate, drank, and otherwise ingested, what their religious practices were, what forms of poetry and narrative they used, what their social organization was, and the like. A set of ancillary issues still often addressed by such investigations centers on attempts to determine the *"Urheimat"* (German for "original homeland") of various groups: for example, how and why they migrated from this area and later settled where they did, whom they came into contact with, how long ago such movements took place, and so on. There is an extensive literature on such questions, and perhaps the best-known writings within it involve research into the lives and times of speakers of PIE – though, with regard to other linguistic groups, see, for instance, Siebert (1967) concerning the Proto-Algonquian homeland.[132]

The allure of the past is strong, indeed, and work in these areas is of great interest not only to linguists but also to language specialists, anthropologists, historians, and prehistorians – as well as being intrinsically interesting in its own right, and thus possessed of considerable appeal for the layperson. Still, we ultimately decided not to include this topic in the present handbook, since it is a subject which focuses less on issues of language change per se, and more on the historical insights that one can gain into non-linguistic matters by employing the results gained from various applications of (both diachronic and synchronic) linguistic methodology. In that sense, it would have been less in keeping with the rest of the material in this book.

Finally, readers may be surprised to learn that this volume does not have a special section or chapter on pidgins and creoles, though THOMASON'S chapter 23 deals with language contact in general. The latter apportionment reflects our view that contact must figure as a crucial aspect in any comprehensive treatment of historical linguistics and language change. At the same time, it is generally agreed that pidgins are not full-fledged languages, and we follow a recent trend in creolistics – see, for example, several of the papers in DeGraff (1999b), though also the contrary view in McWhorter (1998) – according to which creoles are treated as not qualitatively different from "ordinary" (non-creole) languages. The social and communicative conditions under which creoles arise are such as to compel great interest, of course, and, in certain ways, they show great temporal compression vis-à-vis more usual rates of change. Still, as far as the study of linguistic change is concerned, our belief that pidgins are essentially too different from non-pidgins, while creoles are basically not different enough from non-creole languages, has led us (admittedly with some qualms) to have the courage of our convictions, and so to conclude that neither of those two linguistic varieties deserves a privileged status in a work such as this.

2.2 *Constructing a present – or, topics to be found here*

That historians should give their . . . [home side] a break, I grant you, but not so as to state things contrary to fact. For there are plenty of mistakes made by writers out of ignorance, and which any [hu]man finds it difficult to avoid. But, if we knowingly write what is false . . . , what difference is there between us and hack-writers? . . . Readers should be very attentive to and critical of historians, and these in turn should be constantly on their guard.

Polybius, *Historiae* XVI. 14.6–8, 10 (c.150 BC), trans. after S. Morison (1968)

We live in a world already made for us but also of our own making . . .[– one] that has its clarities and its ambivalences. . . . These qualities of the world of the present, we must assume, were qualities of the world of the past . . . [To] ambition to tell what actually happened . . . [is to] ambition as well to describe the painful mix of force and freedom that life tends to be.

Greg Dening, *Mr. Bligh's Bad Language: Passion, Power and Theatre on the "Bounty"* (1992: 5)

So, then: what does this volume include? Let us answer that query by considering, next, a selection of five key issues and controversies which fuel much research in historical linguistics and are addressed by several (non-overlapping) sets of chapters in this volume. In presenting this overview, we deliberately do not rehearse the well-known and influential listing of major questions that Weinreich et al. (1968) formulated, named, and discussed in their ground-breaking article of more than thirty years ago. Rather, the reader is referred to that work, to Janda (2001), and especially to Joseph (2001b) for discussion and elaboration of the matters touched on there. The themes at issue here are as follows:

1 What is the role of children in language change? In particular, is it children who largely drive change, via the necessary (re)constitution of language that occurs when they acquire their mother tongue (due to the potential for reanalysis that such a process entails), or are children actually tangential to the personal forces and central arenas of interaction and language use which most strongly determine variation and change in languages? Substantial passages in chapter 25, by AITCHISON, as well as prominent parts of the contributions by HALE (chapter 7), LIGHTFOOT (chapter 14), PINTZUK (chapter 15), and especially FORTSON (chapter 21), discuss this matter to at least some extent – in a number of cases, with quite different answers being advocated.[133]

2 What kind of relationship exists between externally motivated and internally motivated changes in language? As for the principles and constraints governing changes that emerge in situations of language-contact (discussed in chapter 23 by THOMASON) or dialect-contact (discussed in chapter 24 by WOLFRAM AND SCHILLING-ESTES), for example, are these the same as, or different from, those holding in situations which seemingly involve no outside influences beyond the resources that speakers have entirely at their own disposal? This is a long-standing debate, and it is made even more vexed by the added possibility of independent innovations on the part of different speakers (as with the slang use of *mo* discussed above in section 1.2.3.8).

3 What is the relationship of linguistic theory to linguists' views of language change? It is important to stress here that (as already briefly mentioned above, in section 1.1.1) one's view of what "language" is unavoidably colors one's view of what language change is. There exists something approximating what is intended to be a theory-neutral perspective on this matter,[134] in which language is viewed as a collection of utterances and words, potential and actual, and where language change is thus merely a change in that collection. But there also exists a more consciously theory-dependent perspective: hence, for structuralists, all language change is system change, whereas, for (some) generativists, all language change is rule change and grammar change, while, for (classical) Optimality Theoreticians, all language change is change in constraint rankings,[135] and so on and so forth. Comparisons between and among various views of analogy and morphological change are inherent in the juxtaposition of chapter 10 by RAIMO ANTTILA with chapter 11 by HOCK and chapter 12 by WOLFGANG U. DRESSLER, while differing perspectives on phonological change lock horns with one another across chapter 6 by KIPARSKY, chapter 7 by HALE, chapter 8 by GUY, and chapter 9 by JANDA. Meanwhile, a panoply of views on syntactic change are brought into mutual close proximity in chapter 16 by ALICE C. HARRIS, chapter 14 by LIGHTFOOT, chapter 15 by PINTZUK, and chapter 17 by MITHUN. All such juxtapositions here bear eloquent witness to the interdependence of general theoretical stances and specific views of language change; thus, for example, a functionally or semiotically oriented synchronic approach tends to go with a functional view of change, while a formalist approach to synchrony tends to correlate with a non- or even anti-functional view of change, to mention just two such correlations – even though these alignments are not strictly necessary.

Related to this point is the fact that, even though this is a book on historical linguistics, much of what is said here has great relevance for synchronic analysis. This is especially so in the contribution by GUY (chapter 8), where an understanding of change depends crucially on a recognition of synchronic variation, but also in that by MITHUN (chapter 17), since the syntactic changes discussed there make sense only if one views synchronic syntax as rooted in discourse structure. Similarly, an extension of the perspective taken by HALE's chapter 7, in which he argues for a purely phonetically driven type of sound change, could lead one to a view that, synchronically, the role played by the relatively abstract patterns of phonology is more limited than is usually assumed. Further, one premise of many studies involving grammaticalization, as illustrated here especially in chapter 18 by HEINE, as well as the contributions by BYBEE (chapter 19) and by TRAUGOTT (chapter 20), is that grammar is an emergent phenomenon – that is, in the sense of Hopper (1987).[136] Generally speaking, we cannot avoid being reminded, in this regard, of a succinct statement in Joseph and Janda (1988: 194) which, by defining how synchrony and diachrony interrelate in such a way as to obviate the need for an independent theory of change, bears on the relation between a theory of language and a theory of language change. Moreover, no less a figure than Roger Lass (1997: 10) has declared that this passage "deserves quotation," and so we feel justified (and not unduly immodest) in quoting from that study:[137]

> In denying . . . [the sharp distinction between] synchrony and diachrony, the view that there is only a panchronic or achronic dynamism in language suggests that there exist grammatical principles or mechanisms which direct speakers to change their languages in certain ways other than through cross-generational and cross-lectal transmission. To the best of our knowledge, however, there is absolutely no evidence suggesting that this kind of asocial individual causation of linguistic change really exists. But such questionable devices can be dispensed with on the usual view, taken here, that language change occurs solely via two independently motivated entities: the present (synchrony) and time (a succession of presents, i.e., diachrony).

Indeed, in Joseph and Janda (1988: 194), pursuing this line of reasoning further, we argued that "language change is necessarily something that always takes place in the present and is therefore governed in every instance by constraints on synchronic grammars."

This claim that (in its short version) "language change always (and only) takes place in the present" receives surprisingly, even vanishingly little discussion in the literature on either diachronic or synchronic linguistics. In fact, to the best of our knowledge, this view has rarely even been mentioned outside of such publications and presentations as Joseph and Janda (1988), Janda (1990, 1994a), Joseph (1992), Fischer (1997), and (less explicitly) Posner (1997: 2). Although this situation may simply represent one of those cases where a scholar should be tempted to say, "The very ubiquity of this belief within our field explains why so few publications ever refer to it," we believe that this is unfortunately not the case. Rather, we fear that it just never occurs to most historical linguists that changes in language cannot legitimately be conceived of as happening elsewhere (or, to coin a useful new term, *elsewhen*) than in the present.

One reason for this may relate to an issue that has already been briefly mentioned above (in this section as well as in section 1.2.3.6): the overemphasis on child language acquisition among diachronically minded generativists of the 1960s and 1970s. After all, children's acquisition of language is usually treated as a clearly synchronic phenomenon. Hence it is possible that diachronicians who have remained acquisitionophiles (like LIGHTFOOT, FORTSON, and HALE in their chapters here) may feel that there is nothing to be gained by affirming a more general "diachrony-as-sequential-synchrony" approach, whereas acquisitionophobes (like HARRIS, GUY, and AITCHISON in their chapters) may gradually have soured on synchrony-in-diachrony due to an acquired distaste for seemingly non-stop appeals to "the" language-learning child (cf., e.g., Allen 1995: 15, who "focus[es] on the language-learner as the locus of structural change").

Still, we believe it more realistic to conclude that the main reason why most historical linguists fail to discuss language change as always occuring in the present is that they continue to focus on diachronic correspondences much more than on actual processes that lead to the innovation and adoption (or rejection) of novel forms. Since diachronic correspondences necessarily include one point in time which lies further back in the past than another, and since they often involve a second time point which is non-present, synchrony can easily disappear from sight when a historical linguist's attention is fixed mainly on a past time without any compensatory strengthening of the realization that the period when a particular change happened was once the present.

Regardless of the reason(s) for its relative neglect, though, we insist on the cogency of the view that linguistic change is always present – in both senses. That is, ongoing variation-and-change is never absent from language, and it always occurs in the present – with obvious implications for (or, rather, against) any attempts to deny the relevance of change-related issues for synchronic analyses or to treat diachronic and synchronic linguistics as non-intersecting subfields.[138] We would only add here that the "present change" approach has an eminent pedigree. For example, it clearly is already implied in the words of the German sociologist Georg Simmel quoted above in section 1.2.2: "[O]ne does not need to distinguish between nature and history, since what we call 'history', if seen purely as a course of events, takes its place as part of the natural interrelationships of world happenings and their causal order" (1908, quoted from 1957: 86). This view could even be said to have holy origins, given that another passage than that already quoted above (in section 1.3) from the *Confessions* of Aurelius Augustinus (St Augustine) leaves no doubt that the present is the only time whose existence is real, since "the past . . . does not now exist . . . , and the future does not yet exist" (*c.*400, quoted from 1981: 276, line 8). Still, in addition to considering whether language change is most closely linked with the present, the past, or the future, there is the logically prior necessity of establishing criteria for determining precisely when a change has occurred, as we briefly discuss next.

4 All of the preceding issues point to, and/or hinge on, the crucial question of when it is that we can talk about change: namely, does this moment arrive after speech-forms are altered by the first appearance of an innovation, or only after there has been some spread of that innovation? (Cf. section 1.2.1 above.) Moreover, if one presupposes that at least some spreading of an innovation must occur before a change can be said to have occurred, must the relevant spreading be to other individuals – and, if so, how many – or could a single individual's increasingly consistent use of an innovative form be considered a type of spread (i.e., to additional linguistic and expressive contexts within that person's spheres of usage) which shows the innovation not to be a one-time error or nonce-form, even if no one else ever adopts that innovation? Some authors here – for example, HALE in chapter 7 and FORTSON in chapter 21 – take the view that an innovation by itself (and it alone) defines a change, that this alone is all that diachronically oriented linguists need to be concerned with. On this view, spread is a matter for sociology, not for linguistics proper. Other authors, conversely – for example, GUY in chapter 8 – see spread as the defining mark of "real" change. While the latter position, already strongly advocated by Weinreich et al. (1968: 104–25, 188 et passim), was subsequently reiterated by Labov (from 1972a: 277–8 through 1994: 310–11), Labov has since moderated his position at least to the extent of emphasizing the role of "influentials" (influential individuals) in language change (cf., e.g., Labov 1997).[139]

If spread defines change, then contact among speakers becomes crucial and the distinction between internally and externally induced change (see above) becomes blurred; the point of origination for an innovation may be internal or external, but in this view, its spread, via external, social factors, is criterial for "real" change. It then becomes a matter of some interest that studies of contact-induced change, as reported on in chapter 23 by THOMASON, have shown that anything can be borrowed, since the absence of constraints on externally induced change would suggest that there is no qualitative distinction to be made between internal and external change, given that there are no clear limits on what can happen internally as well. Similarly, it must be admitted (following Milroy 1993) that certain factors may promote innovations – in both internal and external change – that are individual, yet simultaneous and hence massive to the point of being global). Such situations mimic instances of local origin plus later spread: for example, if many individuals sharing the same language as a common structural "filter" react in like fashion to the same contact stimulus, the effects will resemble both widespread diffusion of something borrowed by one individual or even many acts of borrowings by many individuals based on more extensive contact (see also n. 90 regarding the onomastic experience of Mr Warren Peace).

Before leaving this topic, we should mention that there may possibly exist a diametrical opposite to contact-induced change: namely, contact-induced stability. The crucial issue here concerns whether linguists (both synchronic and diachronic), in reasonably denying much efficacy to adults' "corrections" of language-learning children, have not been led to downplay the effects – other than hypercorrection, on which see Janda and Auger (1992) and references there – of adults' correcting other adults, and hence to underestimate the influence exercised by those whose advocacy of conservative speech-norms is active or even fanatical, like some teachers in compulsory schools or clerics who preside over daily churchgoing.

Although this topic must be saved for later research, we would briefly like to draw attention here to a relevant proposal made by Timothy Vance (1979: 116–17) in response to the finding that only 14 percent of his Japanese native-speaker subjects would extend to new (nonsense) forms the Japanese (morpho)phonological rule of so-called "sequential voicing" (*rendaku*, as in *ori* 'fold' + *kami* 'paper' = *origami*). Vance wondered whether this number might in fact represent the approximate percentage of the entire natively Japanese-speaking population who are in some sense committed to the *rendaku* rule – but with such fanaticism that they decide to become schoolteachers, usage commentators, and the like. Could this small band of dedicated *rendaku*-advocates, he asked, induce large portions of the general population to maintain sequential voicing as a regular rule of existing vocabulary, even though they cannot lead them to apply the rule productively? Of course, a complete answer to this question would require a full-fledged variationist study employing quantitative methods (in order to determine the extent to which the various social classes actually apply *rendaku* in more colloquial styles of speech). Still, suggestive evidence is provided by the fact that certain other (morpho)phonological alterations which are today found across all social groups and speech-styles were once much less widespread – until they received the strong support of grammarians and other academicians (e.g., cf. Janda 1998b: 351 for sources discussing variation between *vieux* versus *vieil* with vowel-initial masculine nouns in seventeenth-century French).[140]

5 Finally, there are issues concerning the causation of change. Here, again, the topic of deciding the relative importance of system-internal versus system-external forces arises, but one can go beyond that basic question and pose more specific queries. For example, whether sound change is a matter more of articulation or of perception – that is, speaker-driven versus listener-driven – is addressed in chapter 22 by OHALA, and whether analogy is more structurally driven or semiotically driven (with a motivation rooted in cognitive processes) is discussed in the chapters by ANTTILA (10), HOCK (11), and DRESSLER (12). Finally, there is the question of whether syntactic change is a matter of alterations in abstract structures, as suggested in the chapters by LIGHTFOOT (14) and PINTZUK (15), or else rooted in the structure of discourse and thus tied to the unfolding of communicative acts in real time, as suggested in the chapters by MITHUN (17), BYBEE (19), and TRAUGOTT (20).

2.3 *Synthesizing tradition and innovation – or, topics here in a new light*

A real tradition is not the relic of a past that is irretrievably gone; it is a living force that animates and informs the present . . . [–]implying . . . [, not] the repetition of what has been, . . . [but] the reality of what endures. It . . . [is] a heritage that one receives on condition of making it bear fruit before passing it on to one's descendants. . . . Tradition thus ensures the continuity of creation.

Igor Fyodorovich Stravinsky, *Poétique musicale sous la forme de six leçons* (1942: 39); trans. Arthur Knodel and Ingolf Dahl as *Poetics of Music in the Form of Six Lessons* (1947: 57)

Whether I think, on the whole, the French Revolution [1789–99] was a success? It's still too early to say.

Zhou En-lai (once a student in Paris, 1920–3, later prime minister of China), in an interview (*c.*1965) widely cited thereafter: for example, by the [Bloomington, Indiana] *Herald-Times* (December 8, 2000: A10), itself quoting Zhou from an editorial in the *Independent* of London on assessing the success of the Internet

Besides devoting particular recognition and discussion to the issues listed in the preceding section, the present work includes several features not easily found, if at all, elsewhere.

First and foremost, as the title *The Handbook of Historical Linguistics* shows, this is indeed a handbook (a manual) and, as thus conceived, follows the precedent set by an entire genre of works in historical linguistics – that of the traditional handbook – by aiming to sift through and sum up the received wisdom and accepted body of knowledge in a particular field. The institution of the handbook thus gives not only necessary background but also up-to-date, maximally definitive statements on timely major issues in the field. Moreover, the substantial bibliography is in itself a valuable resource for comprehending the breadth of the field as a whole.

Second, although this volume includes much that is traditional in historical linguistics – for example, the comparative method, internal reconstruction, dialectology, language contact, etc. – it attends equally to issues of more current relevance. Thus, the past decade's truly remarkable surge of interest in grammaticalization – a topic not even mentioned in, for example, the index closing Hock (1986), a widely used upper-level textbook – has resulted in the present book's including five chapters directly concerned with that phenomenon – those by BYBEE (19), HARRISON (2), HEINE (18), MITHUN (17), and TRAUGOTT (20) – and further discussion of grammaticalization elsewhere, as well: for example, in chapter 13 by JOSEPH, in addition to those by FORTSON (21) and by HOCK (11).

Third, for most topics which have occupied historical linguists extensively over many years and which involve key areas of study in linguistics (especially sound change, analogy, diachronic syntax, and language comparison), this book's editors (as noted earlier, in section 2.2) have deliberately commissioned several chapters, rather than requesting a single summary statement from just one researcher. Also deliberate is the present juxtaposition of formal, functional, and/or variationist approaches to the study of particular subjects – which, by bringing in at least one representative of each differing methodology, gives a fullness of voice to each topic overall.[141] It is in these ways that we have attempted to carry out our intention to ensure that multiple viewpoints are represented and that there is some internal dialogue between and among authors (as with the discussion by HALE in chapter 7 of the claims made by KIPARSKY in chapter 6 concerning sound change). Similarly, while there are entire sections of the volume dedicated to the examination of change as it affects one particular linguistic domain (e.g., for sound change, diachronic morphology, and syntactic change), brief but significant discussions of these areas are in fact also to be found in other parts of the volume. OHALA, for instance, in chapter 22, treats sound change within the section on causation, and JANDA, in chapter 9, discusses it within the section on morphological change. Hence, in actuality, the issue of causation is not restricted to the last section: both HALE in chapter 7 and FORTSON in chapter 21, for instance, discuss cognitive and acquisitional aspects concerning the causes of particular changes.

Fourth and finally, this book seeks to cover a broad range of languages, even though historical linguistics as we know and practice it today largely began with (i) the recognition of the Indo-European language family in general, after which came (ii) intensified research by nineteenth-century scholars into the nature of and relationships among the various Indo-European languages, including the branches into which they cluster. Though much work has by now been done on other language families, Indo-European studies still dominate the literature, and, indeed, Indo-European languages are well represented in this volume. At the same time, significant attention is paid in this work to native languages of North America (e.g., Algonquian, Siouan, Eskimo-Aleut) and to languages of the South Pacific (e.g., Austronesian), of the Caucasus (e.g., Kartvelian, Chechen-Ingush, etc.), and of Africa. Indeed, the language index for this volume is quite robust.

Thus, even with the recent flurry of publishing in historical linguistics,[142] to the extent that the field seems to be enjoying a real renaissance (after what felt like years of neglect and marginalization within the overall field of linguistic science), there is still a need for such a volume as this one, with its combination of breadth and depth, of traditional background and current controversy.

3 Epilogue and Prologue

3.1 *Passing on the baton of language – and of historical linguistics*

Le tems s'en va, le tems s'en va, ma Dame, / Las! le tems non, mais nous nous en allons.

Pierre de Ronsard, from "Je vous envoye un bouquet . . ." (1555, original orthography; in *La continuation des amours* (1558), but suppressed, apparently due to its metrical unevenness, in the 1578 revision; reprinted in *Oeuvres complètes*, *Vol. II*, 1965: 814)

"Time goes, you say[; Time goes, you say, my Lady]? Ah no! / Alas, Time stays, WE go."

Austin Dobson, "The Paradox of Time (A Variation on Ronsard)" (original emphasis), in the journal *Good Words* (1875), reprinted in Dobson (1923: 116)

Time is the substance I am made of. Time is a river that carries me away, but I am the river; it is a tiger that mangles me, but I am the tiger; it is a fire that consumes me, but I am the fire.

Jorge Luis Borges, "Nueva refutación del tiempo" (1947), trans. Ruth L. C. Simms as "A New Refutation of Time"

As the foregoing sections have demonstrated, our aim in conceiving the plan and commissioning the chapters for the current book has been the ambitious one of trying to be all things to all people – in terms of topics covered, languages discussed, viewpoints represented, and so on and so forth. We thus conclude these introductory remarks with an invitation – and a caveat – to readers of this volume. It should be clear that this work is primarily addressed to those who have at least some background in linguistics and/or the history of particular languages; such prerequisites belong to the essential nature of a handbook. In that sense, too, this is not a textbook and not an introduction. Still, we believe that this volume can be used for introductory purposes, especially for bringing in a diachronic perspective as a balance to the heavily synchronic (and non-diachronic) viewpoint assumed by most courses in linguistic theory and analysis. In this way, any reader who begins to gain a minimum of experience with linguistics as a field, in any subfield of the discipline, should soon find substantial portions of this book to be extremely relevant and highly informative. At the same time, there are many senses in which the level of presentation targeted by the current work is advanced enough that "professional" linguists ought to be able to benefit greatly from this collection of chapters – even professional historical linguists. Our expectation, therefore, is that there will indeed be something for all readers in this work.

At this point, however, there is no longer anything more that we can do here in pursuit of such a goal. The rest, as they say, is history – we mean this more literally than our readers might perhaps be tempted to think. The rest is history in the sense that what follows this essay should be – or at least can be – research in historical linguistics. As we presently reach the end of our introduction, it begins to belong simultaneously to our own past and to our potential readers' future. This juxtaposition of times by one pair of authors emboldens us to conclude by suggesting that a similarly paired set of joint approaches to the study of linguistic change is likely to guarantee the greatest possible success for both this domain and the field of linguistics in general.

From the discussions in several sections above, we believe it follows that the most productive way to study changes in language – either in some particular period(s) from the past or in general – involves a combination of efforts which can be achieved if more diachronicians will apportion their time more equally (say, 60–40 percent, if not 50–50) between investigating the linguistic history of earlier eras and investigating changes currently in progress.[143] In the eloquent words of Schlink (1995, quoted from 1998: 130): "Doing history means building bridges between the past and the present, observing both banks of the river, taking an active part on both sides." After all, as suggested by our earlier recasting (in section 1.2.2.2) of the so-called "uniformitarian principle" as a principle of informational maximalism, we historical linguists have everything to gain from building up an inventory of well-studied present times which, as they cumulate into a store of well-studied pasts, will slowly but inevitably provide a more solid database for formulating and testing increasingly sophisticated hypotheses regarding language change. Yes, some of these hypotheses will turn out to be ridiculously wrong. But, we maintain, a scientific (sub)discipline cannot make significant progress by refusing to propose any generalizations until it has "gotten everything right." As more hypotheses are made regarding linguistic changes in the future, students of diachrony will be forced to look more closely and alertly for evidence of innovations in particular linguistic and social contexts, and later hypotheses can still profit greatly (and not just in terms of morale) from the risibility of earlier ones. Perhaps it will seem at first as if we are merely engaging in alchemy, so to speak, but chemistry will lie just over the horizon. . . .

Thus, while it may be difficult to argue with Lass's (1980a) conclusion that historical linguistics as currently practiced is a discipline little capable of even *ex-post-facto* predictions (or, in the terminology of Thom 1975: 115, "retrodictions") concerning what changes in language are likely to take place, we would argue strongly that historical linguists have yet to put their best foot forward. On this view, our goal should lie in exactly the opposite direction from Lass's (1997) call to study language change in terms of past linguistic structures themselves, rather than via reference to speakers (of any era). Instead, what we need are many more studies of many more groups of contemporary speakers. Indeed, far from concluding that a speaker-based linguistic diachrony has already tried and failed to elaborate an exegetic-hermeneutic methodology, much less a deductive-nomological one, we would urge our colleagues to keep in mind what Captain John Paul Jones expostulated at the height of a naval battle on September 23, 1779 (during the American Revolutionary War). Asked if he was ready to surrender, Jones retorted: "I have not yet begun to fight!" (cf. Dale 1851, quoted from 1951: 173). Alternatively (supplementing Jones's answer in a more international vein), historical linguists could do worse than adopt the words attributed to Maréchal de France (≈ Field Marshal) Ferdinand Foch on September 8, 1914, during the First Battle of the Marne (at the start of World War I; here in translation): "My center is giving way; my right is being pushed back: the situation is excellent; I am attacking!"[144]

However, just as there is no need for any diminution of the esprit de corps among students of language change, so also such martial metaphors should be tempered with an emphasis on the fact that cooperation among historical linguists of differing interests and expertise is also likely to be a *sine qua non* for future breakthroughs in linguistic diachrony. Our discipline will continue to be broadened with new specializations (e.g., when speech analysis reaches the point where 10,000 hours of spoken conversation can accurately be transcribed automatically, even across dialect boundaries – which will surely be possible before the end of this new century) and to be deepened via the further development of existing areas of expertise. But the study of linguistic change is also being eroded by the steady disappearance of positions once specialized for historical linguistics (e.g., in language departments). We therefore believe that it is closer cooperation among diachronicians of various sorts which will both hold historical linguistics together and ensure its greatest success. As the theologian Reinhold Niebuhr (1952: 62–3), albeit in another context, put it so inspiringly: "There are no simple congruities in life or history . . . [, due to] the fragmentary character of human existence. . . . Nothing that is worth doing can be achieved in . . . [a] lifetime. . . . Nothing . . . virtuous . . . can be accomplished alone." It is with such convictions in mind that we have dedicated this volume to the spirit of collaboration and cooperation in historical linguistics (see the preface preceding this introductory essay).

In short, less a division of labor than a sharing of labor by students of language change appears to be the most promising approach: a collaborative endeavor in which scholars across the spectrum of diachronic, psycho-, socio-, and general linguistics link forces to focus not on the past states of "old-time synchrony" (static non-diachrony), but on a combination of past changes (dynamic diachrony) and present changes in progress (dynamic synchrony).[145] It is undeniably true that much excellent recent work has been wrung from "the use of the present to explain the past" (≈ the title of Labov 1974/1978; cf. also Labov 1994).[146] But we would argue that the greatest benefit available from a revised interpretation of the "uniformitarian principle" as informational maximalism (see section 1.2.2.2 above) can actually be gained if we pursue the above-mentioned goal of accumulating a solid quantity of broadly detailed (and "thickly . . . described"[147]) data from a succession of present times that starts now and continues into the future – with these "presents" thereby becoming the past that will eventually allow us to explain a future (coming) present.[148] Someday, we are convinced, diachronicians will use the present (when it has become the past) to explain at least part of the future (when it has become the present) – just as, in some of Conan Doyle's stories about him (e.g., "The Speckled Band" from 1891), Sherlock Holmes was able not only to explain past events but even to predict what was still to come.[149] Still, far from equating linguistic change with crime, we hasten to emphasize that, on the contrary, it is only the failure to devote adequate study to ongoing changes in language which deserves to be seen as criminal.

3.2 *Envoi*

We can only pay our debt to the past by putting the future in debt to ourselves.
John Buchan (Baron Tweedsmuir of Elsfield), "Address to the People of Canada upon the Coronation of King George VI" (May 12, 1937)

If you cannot enter passionately into the life of your own times, you cannot enter compassionately into the life of the past. If the past is used to escape the present, the past will escape you.
Henry H. Glassie, "Meaningful Things and Appropriate Myths: The Artifact's Place in American Studies," *Prospects: An Annual of American Cultural Studies* 3 (1977: 29)

While this essay has not hesitated to criticize certain aspects of historical linguistic work, and while it has not engaged in forced optimism about cases where the possibility of our ever gaining much confidence about specific past phenomena seems weak, if not bleak, we want the overall and final impression left by this introduction to be an upbeat one of hope, expectation, and even exultant impatience. Linguistic diachronicians have done much in the past, but even the study of diachrony should be at most *partly in* the past (through an awareness of what our predecessors accomplished), rather than *wholly of* the past (in terms of the periods studied). In short, we believe that the greatest achievements of historical linguistics are still to come. For this reason, and because we see so much promise in the strategy of accumulating a set of closely described presents for future use as soon-to-be explanatory pasts illuminating a later present – and, just as importantly, because the major part of this *Handbook of Historical Linguistics* (the meat and potatoes, so to speak) still lies literally ahead of our readers – we would press upon you these words: Forward to the Past!

And yet, it still might be asked, should such a thoroughgoing reorientation of, and rededication to, the study of language change – as something that always occurs in the present – really be viewed as a tremendously urgent task? Perhaps, some might suggest (at least metaphorically), it might be best to appoint a large and diverse committee to reflect at leisure on the matter and

then report back, while the business of historical linguistics proceeds as usual in the meantime. But we could not disagree more: the proper time to investigate the intersection of language and active time is now. And, as for the urgency of this undertaking, we believe it best to conclude by citing a highly relevant parallel invoked in 1962 by an influential twentieth-century statesman, John F. Kennedy, just as we began this introduction with an 1862 remark (Lincoln's dictum that "*We* cannot escape history") by a nineteenth-century leader of no lesser stature. Kennedy drew attention to an incident in the life of Louis-Hubert-Gonsalve Lyautey (1854–1934), a soldier, statesman, and writer who was elected to the Académie Française in 1912, made a (Field) Marshall of France in 1921, and is remembered, among the many colonial administrators of his time, as unique in his respect for local institutions, especially in Morocco (Lyautey's tomb in the Hôtel des Invalides – not far from Napoleon's – bears inscriptions in both Arabic and French). Addressing an academic audience in March of 1962, Kennedy recalled: "Marshall Lyautey . . . once asked his gardener to plant . . . [a certain tree, but t]he gardener objected that the tree was slow-growing and would not reach maturity for a hundred years . . . [, to which t]he Marshall replied, 'In that case, there is no time to lose; plant it this afternoon!' "

I hate quotation. Tell me what you know.
Ralph Waldo Emerson, *Journals* (May, 1849), reprinted (1965: 141)[150]

By necessity, by proclivity, and by delight, we all quote. . . . Next to the originator of a good sentence is the first quoter of it.
Ralph Waldo Emerson, "Quotation and Originality," in *Letters and Social Aims* (1876: 158, 169)[151]

NOTES

1 *Bunk* here means 'claptrap, drivel, nonsense; humbug; deceptive, empty, foolish, or insincerely eloquent talk.' But these senses arose via a radical semantic shift in – and subsequent clipping of – a word which had once been just a personal and place name: viz., *Buncombe* (ultimately from the transparent Old English compound *bune* 'stalk, reed' + *cum(b)* 'valley'; cf. Cottle 1978: 75 and Brown 1993: 223, 300, 506). This unusual etymology has a combination of two further properties that is nearly unique and thus surely justifies granting pride of place to *bunk* within this first note in an extended general discussion of language change. The following summary draws on Bartlett (1877), Barrère and Leland (1897: 193), Holt (1934/1961: 42, 129), Morris and Morris (1977: 97, 283), Lighter et al. (1994: 315–17), and especially Hendrickson (1998: 111), plus Bryson (1994: 287, 379n.1); other senses and origins of *bunk*(s) are listed in some of these works, but more fully by Cassidy

(1985: 463–4). The near-uniqueness of "nonsense"-*bunk* lies in our knowing not only (i) the full name and the detailed identity of the person whose particular actions led directly to the semantic change at issue, but also (ii) the precise year, month, date, and even time of day when this person's actions set the relevant change in motion. Namely, on the morning of February 25, 1820, Felix Walker – a North Carolina congressman from Buncombe County (where Asheville is the county seat) – subjected the US House of Representatives to a seemingly pointless and endless oration totally unrelated to the general topic then being debated in the House (the so-called Missouri Compromise, which included a limited allowance for the territorial expansion of slavery). When Walker's colleagues interrupted him to request that he keep to the main topic at hand, he replied, "I am only talking for Buncombe" (in fact, his speech had been written some time before and was indeed intended to impress only his constituents back home). Walker's answer was reported in many newspaper accounts devoted to the great debate in which he had, so to speak, taken part. Almost immediately, US English-speakers began to use the phrase *to be talking for Buncombe* with the meaning "to be talking flowery political nonsense," and this was rapidly shortened to *(. . . talking) Buncombe* – with its noun soon variantly spelled *bunkum* – and finally (during the 1850s) also to . . . *bunk*. Even by 1827, attestations show that the expression's earlier sense of "bombastic political talk" had been extended to cover "any empty, inflated speech clearly meant to fool people," a meaning which appears to have become dominant by about 1845 and also occurs in British usage starting *c*.1856. Partridge and Beale (1989: 68) describe *bunk* as colloquial in the nineteenth century but standard in the twentieth. Lighter et al. (1994) make the important observation that *bunk*'s link with deception was surely influenced by the non-cognate word *bunco* (from the Spanish card-game *banca*; cf. *banco* "bank"), a term for a dishonest game of cards, dice, or the like. *Pace* Henry Ford, the achievements of historical linguists in ferreting out all of this information are anything but *bunk*.

2 That this is not merely a question of terminology – or just another illustration of the fact that, if you push down on a water bed at one end, it rises up correspondingly at the other end – is shown by the fact that those who favor the lumping together of morphology and syntax tend to view the result not as "morpho(-)syntax," but essentially as "greater syntax," within which (former) "syntax proper" constitutes "(greater) syntax *par excellence*" and (former) morphology is something of a stepchild. For such analysts, phenomena which could have received either a purely morphological or a purely syntactic account – in the earlier senses of these words – tend to get the latter kind of treatment, and this obviously has major consequences for diachrony as well as for synchrony. For further discussion, see Joseph and Janda (1988), plus Janda and Kathman (1992) and Janda (1994a), along with their references. (The need to show that these issues are substantive and not merely terminological was impressed upon us by Barbara Vance.)

3 Furthermore, word structure is far from negligible even in grammatical accounts where sentence structure receives a plurality of attention: thus, for Modern Greek, Joseph and Philippaki-Warburton (1987) devote 47 percent of relevant text (119 pp.) to syntax but still 43 percent (108 pp.) to morphology, versus only 10 percent (24 pp.) to phonology. Even works of this sort may actually discuss a greater number of morphological patterns than syntactic ones, though this fact may be hidden because syntactic descriptions – with their sentence-length examples and frequently three-part presentation (= original and two translations: morpheme-by-morpheme and idiomatic) – inherently take up more space than morphological ones. In support of this conclusion, it bears mentioning that Joseph and Philippaki-Warburton were closely following Comrie and Smith's (1977) "*Lingua* Descriptive Studies: Questionnaire," in which the apportionment of guiding questions is as follows: morphology with 47 percent (30 pp.) versus syntax with 28 percent (18 pp.), plus phonology with 12 percent (8 pp.), lexicon with 11 percent (7 pp.), and ideophones with 2 percent (1 p.). And the ongoing LINCOM Europa series "Languages of the World/Materials (LW/M)," with numerous 60- or 120-page grammatical descriptions, is organized according to an even more lopsidedly morphocentric plan: 25 sets of queries (nearly 70 percent) for morphology, versus 7 groups of questions (just over 19 percent) for syntax, and 4 (barely 11 percent) for phonology.

4 Regarding *cf.* here: partly for convenience (and welcome variety), but also in order to provide an iconic illustration of language change at work in a work on language change, we follow the growing practice of using *cf.* to mean 'confer, see' – taking it to abbreviate English (finally stressed) *confér* – even though its etymon, Latin (initially stressed) *cónfer*, actually meant (among other things) 'collect, compare, contrast.' But we draw the line at this point, and so do not join those writers of Modern English who, by analogy to *i.e.* and *e.g.*, use *c.f.* as an alternative punctuation. In other disciplines, though, *cf.* retains adversative, even adversarial meaning, as Grafton (1997: 8) points out: "Historians . . . often quietly set the subtle but deadly cf. ('compare') before . . . [a citation of a work; t]his indicates, at least to the expert reader, both that an alternate view appears in the cited work and that it is wrong."

5 We are reminded here of the bon mot (known to us from Calvert Watkins's class lectures on historical linguistics at Harvard University during the early 1970s and at the Linguistic Institute in Salzburg during the summer of 1979) according to which – with reference just to "laryngeal theory" (see Lindeman 1970; Bammesberger 1988) and to the glottalic interpretation of its obstruent system (see Gamkrelidze and Ivanov 1972, 1973, 1984, plus n. 37 below): "No language has ever changed more during a short period of time than reconstructed

Proto-Indo-European during the 20th century."

6 It is certainly true that many introductory works on historical linguistics spend some time giving an overview of selected key events in the history of the field, such as Rasmus Rask's and Jacob Grimm's formulations of the First Germanic Sound Shift or Karl Verner's account of certain exceptions to Grimm's Law, since these findings revealed important truths about the nature of sound change (see, e.g., Hock and Joseph 1996: ch. 2). Moreover, there are some surveys of historical linguistics that give considerable space to facts about the history of the field: for example, Anderson's (1991) discussion of Pāṇini's Sanskrit grammar (which was not, however, historically oriented) and Greek debates in the Classical period about the nature of language (though those discussions did have a bearing on matters of etymology). Hence we must stress that the present volume does not treat the history of linguistics, and there is no compelling need for it to do so, given that there already exists a sizable literature on this very topic. Relatively specialized studies dealing with the histories of particular periods, linguistic subfields, or countries include such representative works as Pedersen (1924), Aarsleff (1982), Anderson (1985), Hymes and Fought (1981), Joos (1986), Newmeyer (1986), Andresen (1990), and Matthews (1993). For conciseness and compactness, few article-length overviews can compete with Collinge (1994a, 1994b) and Koerner (1994a, 1994b). Among the numerous general book-length histories of linguistics that are available for consultation, we call special attention to the following: Arens (1969; essentially an annotated anthology), Waterman (1970; extremely brief), Sampson (1980), Amsterdamska (1987), Robins (1997), and Seuren (1998) – all single-authored books – as well as three collections: Hymes (1974b; eclectic), Koerner and Asher (1995), and Auroux et al. (2000ff). Besides highlighting the two last-mentioned works, which are co-edited by E(rnst) F(rideryk) Konrad Koerner, we can at this juncture more generally incorporate by reference virtually the entire set of works written or edited by Koerner. For the latter's formidable bibliography on this and related subjects, see Cowan and Foster (1989) and Embleton et al. (1999), plus the journal *Historiographia Linguistica* and many of the proceedings of the International and the North American Conferences on the History of the Language Sciences (ICHoLS and NACHoLS). Several useful compendia on personages in the history of the field should also be noted: Sebeok (1966), Bronstein et al. (1977), Stammerjohann et al. (1996), and Ohala et al. (1999: vi–vii, 75–126, plus, on institutions, 39–74, and, on other organizations and projects, 1–38), as well as the series so far instantiated by Davis and O'Cain (1980) and Koerner (1991, 1998).

7 Except where noted (as here), translations from non-English originals are our own.

8 Delbrück's (1880) *Einleitung in das Sprachstudium* . . . seems to take a similar view, suggesting (p. 19) that Bopp's organismal terminology involves obvious "metaphors . . . – very natural ones, too . . . [– and,] probably, if anyone had called his attention to the point, Bopp would have acknowledged that . . . [,] in reality . . . [, mental] activities take place, not in language, but in speaking individuals." Conversely, (p. 42f), as for Schleicher and "the natural sciences . . . , . . . he really possessed considerable knowledge of them . . . [, being] especially versed in botany . . . [; a]ccording to scientists who knew him, he was celebrated for his admirable preparations for the microscope, as well as for certain productions of horticultural art." That is, Schleicher was also an avid gardener, especially of cactuses and ferns; cf. Schmidt (1890: 415). Moreover, Tort (1980: 49) points out that, at the beginning of his years as a professor at the University of Jena (1857–68), Schleicher sat in on courses in physiology and botany, while both Desnitzkaja (1972) and Koerner (1974: xiii n.13) present evidence that (in the words of the latter) "Schleicher consciously adopted both terms and procedures from the natural sciences, particularly biology." For further discussion and many additional references concerning Schleicher's organicism in his linguistics, see especially Desmet (1996: 48–81 et passim), but also Goyvaerts (1975: 39–44), who points to the Neogrammarian penchant for exceptionless sound laws as one legacy of Schleicher's natural-scientism. Jespersen (1894: 2–17ff), for his part (cf. also McMahon 1994a: 319–23ff), singles out Hegel as a major additional influence on Schleicher's views: for example, the latter's predilection for ternary distinctions, and his positing of prehistoric versus historic periods differentiated according to criteria of (un)consciousness, progression/retrogression, conflict/stability, and the like. A final piece of evidence for the complexity of Schleicher's personality and thought comes from the fact that, in 1844 (during his early twenties), he developed a passionate interest in phrenology and proudly co-founded the second phrenological society in Germany (cf. Schmidt 1890: 403/1966: 376), though this new enthusiasm seems to have been bumped off rather quickly by an avid return to amateur music-making.

9 Even as linguistic organicism wilted away in France during the 1920s, there occurred an isolated efflorescence of at least partly similar views (cf. the discussion by Wils 1948: 135–9) in the later work of the Dutch linguist Jacques van Ginneken, whose 1929 article on the hereditary character (= the biological heritability!) of sound laws concluded (p. 44) by arguing that two related developments were essentially inevitable. First, he suggested that Schleicher's family-tree diagrams would eventually be reinterpreted in a literal, biological sense, thereby regaining a place of honor in linguistics. Second, he predicted "that . . . older expressions . . . like, e.g., *the life* or *the organism of spoken language* . . . [would] necessarily win back again a corrected portion of their old meaning." Although comments like van Ginneken's were explicit enough to exclude the possibility that merely a metaphorical use of a term such as *organism* (*Organismus*, in the original German) was intended, this was not the case for all writers of the period. Thus, while Hermann Paul might have been

expected to avoid even the slightest hint of the organicism which had been so roundly criticized by his fellow Neogrammarians, the first chapter of his most famous work (1880) uses the words *Organismus* and *Sprachorganismus* repeatedly (19 times on pp. 27–9 and 32). Apparently, though, these always have (despite the literal rendering in Strong's 1890: 6–9, 13 translation of Paul 1880) the metaphorically extended meaning "(cohesively organized) system" (rather than "organism" – or "organization," or "organ": for example, p. 15 refers to the "organization of mind and body" as *geistige . . . und körperliche . . . Organisation*, and p. 28 to the "speech organs" as *Sprechorgane*). Paul's avoidance of the term *System* "system" itself appears to reflect the latter's residual but strong connotations of "grandiose overarching speculative scheme" (see Burkhardt 1977), with which it had become tinctured during the preceding 100–50 years, as the pendulum swung away from such schemes. Thus, Rudwick (1972: 94) describes "a new generation of naturalists['] . . . distaste for grand syntheses" like those of Buffon (1778), and Gould (2000: 116) comments on how Lamarck's "favored style of science" (e.g., in his 1820 foray into psychology) – "the construction of grand and comprehensive theories . . . [,] an approach that the French call *l'esprit de système* (the spirit of system building) – became notoriously unpopular following the rise of a hard-nosed empiricist ethos in early-nineteenth-century geology and natural history."

10 Both here and subsequently, we use "American" with apologies to our Canadian, Mexican, and Central or South American colleagues (also believing that the other authors represented in this volume would concur with us), but there is at present no commonly accepted truly adjectival form for *United States* (or *US(A)*) in English – as opposed to, say, Spanish *estad(o)unidense* or French *éta(t)sunien* (≈ "United-Stat(es)-ian"). We ourselves advocate the wider adoption of *Usonian*, a term first promoted in the 1930s by the architect Frank Lloyd Wright, albeit mainly for a particular building style (see, e.g., Thomson 1999: 324, but also 14, 170, 258, 336, 339, 356, 383, 394, 398, 400). Wright explained *Usonian* as consisting of an acronym based on the first four initials of *United States of North America* plus *-ian*, but he credited the British novelist Samuel Butler (1835–1902) with its creation – despite the fact that an occurrence of the term in any of the latter's works has yet to be found.

11 This conclusion should not, however, be taken as vitiating the fact that biology can sometimes serve as a convenient metaphor (cf., e.g., Hock and Joseph 1996: 445–6) or as a hypothesis-generating heuristic – for example, as a source of suggestive parallels (like those drawn in Dixon 1997) – once we have gotten it straight that the only organisms which are centrally relevant to language are human beings. It is also worth noting that organicist metaphors apparently helped some nineteenth-century linguists to think of (a) language as a system by letting them treat it as "an organic whole" ("ein organisches Ganze"; cf. Windisch 1886: 325 on his late teacher Georg Curtius's use of this phrase) – see, too, the list in

Tsiapera (1990) – whereas many Neogrammarians were tempted, in this regard, to throw out the baby with the bathwater (at least in the view of Jakobson 1931). We do not follow Tsiapera (1990), however, in seeing nineteenth-century organicist linguists like Bopp as having been influenced primarily by the general "intellectual climate" of pessimism connected with Romanticism, whose emphasis on decay as a major force in life would somehow have led that movement's advocates to analyze, for example, the loss of inflections as due to the deterioration of an aging organism. Indeed, Verburg (1950: 466) argues that "Bopp was very old-fashioned in his basic conceptions. At a time when the Enlightenment, Kantianism, and Romanticism were still very . . . actual . . . [up-to-date, "in"], Bopp still . . . [swore] by the theoretically exact scientialism of the rationalism of earlier days, which had been given up by th[ose] . . . movements."

12 After some reflection, we have opted to follow the practice of scholars who continue to use BC and AD as qualifiers for all dates given in terms of years, decades, centuries, and millennia, rather than switching to the competing labels (B)CE (for *(Before the) Common Era*). In particular, we reject the allegation that BC/AD represents a partisan favoring of a particular theology. After all, since it is known that Jesus of Nazareth was born before the death of Herod the Great in 4 BC – cf., for example, Fuller (1993: 356–66, especially 356), Hoehner (1993: 280–4), Levine (1998: 470–4, especially 471), Reicke (1993: 119–20), and their references – then Jesus must have been born before or at least during 4 BC, though this date obviously cannot have been literally four years before (the birth of) Christ. Further, the English vocabulary of calendrical terms is already broadly ecumenical, or at least multidenominational: for example, most of the terms for the months (as in many other languages) reflect names of Ancient Rome's gods (Janus, Mars, Maia, and perhaps also Juno), deified rulers (Julius and Augustus), or religious festivals (the Februà, a feast of sacrificial purification). On the other hand, as admirably summarized by Whitrow (1988: 68–9), the institution of the seven-day week has a Sumerian and Semitic (Babylonian and Judaic) origin, while the ordering of the days within it has an astrological basis relating to planets and other heavenly bodies whose names again are connected with Roman deities (viz., the sun, the moon, Mars, Mercury, Jupiter, Venus, and Saturn) via their Germanic counterparts (including Tiu, Odin, Thor, and Frigga). For general discussion of these and related issues, see Whitrow (1988: 66–74) and Blackburn and Holford-Strevens (1999: passim).

13 We say "this world" because there are conceivable possible or virtual worlds without temporal constraints. For instance, the "world" of grammars as psychologically interpreted entities may be one such world, since it is possible to model grammatical systems as having simultaneous application of rules – even though, in the real world, precise simultaneity of sound changes affecting the same portion of a word (e.g., adjacent sounds) seems to be a rare event and is perhaps even impossible.

14 This position (that the past cannot be changed even by divine agency) is also that of orthodox Jewish theologians, according to Dummett (1964: 34). Dummett himself, however, adopts a different stance on the issue. Likewise in direct contradiction of Agathon's claim are at least seven medieval Roman Catholic philosopher/theologians (including one saint) who argued that a proper understanding of divine omnipotence leads inescapably to the conclusion that God does have the power to undo the past. As copiously documented by Courtenay (1972–3: 226n.6/1973: 148 nn.90–1, 149nn.95–8, 157–63nn.124–51), this assertion was made by all of the following: Cardinal Bishop (and Saint) Peter Damian (*c*.1060), William of Auxerre (*c*.1075), Bishop Gilbert of Poitier (*c*.1130), Rodulphus de Cornaco (*c*.1343), Archbishop of Canterbury Thomas Bradwardine (*c*.1344), Augustinian Vicar General Gregory of Rimini (*c*.1345), and Pierre d'Ailly (*c*.1375). The writings of these scholars on divinity and preterity were, as a rule, produced before they reached their positions of greatest authority, but it is striking that their claims, even though provoking much vehement opposition (again see Courtenay 1972–3), did not prevent them from later being assigned posts of considerable responsibility. For further discussion of this and related issues in modern philosophizing, see the treatment of earlier work in Gale (1968) – who cites more than a dozen relevant papers, some of them anthologized in Gale (1967) – as well as the dispersed remarks in Turetzky (1998) and the more concentrated ones in Mellor (1998: 34–5, 105–17, 125–35), along with several recent articles and references in Le Poidevin and MacBeath (1993: 225–6) and Le Poidevin (1998).

15 We are being deliberately vague here as to the ontological status of the "happening" referred to; what really matters is that, somewhere, there occurred in real time an event which someone wants to describe and to account for scientifically.

16 For example, the presentation of grammaticalization in McMahon (1994a) – admittedly an introductory-level textbook, and thus somewhat simplificatory in nature – discusses the development of the Modern Greek future marker (p. 167) solely with reference to Ancient Greek *thélō hina* . . . 'I want that . . . ' and Modern Greek *θa*, citing not a single stage from among the many attested intermediate forms (for which see chapter 13 below by BRIAN JOSEPH; Joseph 2001a; and Joseph and Pappas 2002).

17 In assessing the relative utility, for diachronic linguists, of viewing change as individual innovation versus viewing it as group-wide spread, the experience of researchers in the non-linguistic sciences seems relevant, especially since advocates of the child-oriented, change-as-innovative-acquisition approach so often adduce parallels from evolutionary studies by biologists (e.g., geneticists, ethologists, and certain neurologists). For instance, in just four works (from 1982–99) by one diachronic syntactician writing within the Chomskyan "Principles and Parameters" framework, there can be found references to biology-related research by, among others: W. Bateson, J.-P. Changeux, C. Darwin, T. Dobzhansky, D. Hubel, F. Jacob, N. Jerne, S. Kauffman, R. Lewontin, J. Monod, J. M. "Smith" (= J. Maynard Smith), R. Sperry, and N. Tinbergen, as well as to Bickerton's (1984) "Language Bioprogram Hypothesis." And, in this volume itself, for example, LIGHTFOOT's chapter (14) likewise cites D. Hubel (and T. Wiesel) and R. Sperry. Still, a salient finding of paleontology – the aspect of evolutionary biology which most closely resembles typical work in historical linguistics – is that, while focusing on individual organisms as the locus of evolutionary change may be a laudable goal theoretically (in both senses of the latter word), such a focus is a hopeless proposition practically, since no serious paleobiologist really expects to find the fossils of the very first creature to evince some innovative trait. Engelmann and Wiley (1977: 3), for example, bluntly state that they "do not know of any paleontologist who would claim to recognize an individual ancestor (as opposed to a populational, species, or supraspecific ancestor) in the fossil record," and so they "dismiss this type of ancestor from further consideration" – whereas the "identification of species and populations as ancestors is a common practice." In consequence, the concrete discourse of most current paleobiologists, when translated into linguistics-compatible terms, turns out to deal with changes less as individual innovations than as either diachronic correspondences or instances of spread. This, at least, is what strongly emerges from a reading of, for example, Dawkins's (1986: 240–1) discussion of migration as a crucial factor explaining apparently abrupt transitions in fossil records; after all, migration is clearly a contact- and group-related social factor, and so arguably a form of spread. In short, precisely because individual-child-based accounts that view innovative acquisition as the main source for linguistic change so often invoke biology – for example, Lightfoot (1999a) repeatedly mentions, and LIGHTFOOT's chapter 14 here briefly discusses (as we also do in section 1.2.3.4 below), the "punctuated equilibrium" of Eldredge and Gould (1972) and Gould and Eldredge (1993), among others – they must face a paradoxical methodological implication for historical linguistics that emerges from the above-mentioned paleontological findings: namely, explanations in terms of individuals are likely to be revealing only for studies of ongoing contemporary changes, not for the study of large-scale language change(s) in the past – change(s) like Lightfoot's "abrupt . . . [Thomian] catastrophes." On the other hand, the same reasoning suggests a positive reaction to the invocation of population-genetic factors by Lightfoot (1999a) and, within this volume, not just by LIGHTFOOT's chapter (14) but also by JOHANNA NICHOLS's (5).

18 The German scholar Hans Mommsen (1987: 51), writing on Germany in the World War II era (and before), has pointed out that research on history not only involves the filling in of gaps ("on the one hand . . . [,] historiography relies on constant generalization of concrete historical evidence"), but also imposes the necessity

of ignoring a certain amount of (over-)attested data ("[a]ny historical description of the past is . . . a tremendous reduction of the overwhelming variety of singular events . . . [, o]n the other hand"). Similarly, Hockett (1985: 2) discusses the requirement "that historiography must involve abridgment . . . [– t]here has to be deletion" – in light of the fact that an "accurate icon of what has happened in the past would occupy as much space and time as the happenings themselves, and there is no room for it." This parallels earlier conclusions (acknowledged by Hockett) drawn by Kroeber (1935: 547–8): "[For h]istory . . . to tell 'what really happened' . . . obviously . . . is impossible: the 'real' retelling would take as long as the happenings . . . and be quite useless for any conceivable purpose. The famous principle is evidently to be understood obversely: history is not to tell what did not happen; that is, it is not fictive art."

19 In this way, historical linguistics is tied to other disciplines that attempt to describe and explain past entities and events. However, linguists may be somewhat better off, in that the insights into language which the present offers (see also subsequent discussion in the main text) often are ultimately better grounded in cognitive and sometimes even neurological aspects of human biology – as opposed to vague appeals to human behavior in general – than are insights that historians might derive from, say, synchronic surveys of how current agrarian societies "work."

20 We intentionally take a broad view here, referring to "documentary" evidence (and not the more usual "textual" sources) in order to emphasize that sound recordings from a hundred years or so ago can (if playable) help provide evidence of change – for example, Syracuse University possesses a *c.*1885 Edison wax-cylinder recording of Pope Leo XIII, who was born in 1810) – and the same is true of movies, even silent ones. For instance, some films presenting messages conveyed in American Sign Language were produced by the National Association for the Deaf in the United States in 1913 (fully 14 years before the introduction of sound into motion pictures in 1927, when Al Jolson starred in *The Jazz Singer*), and these still serve as an early record of that language against which later forms can be compared. Indeed, "documentary" evidence of *some* sort is always critical, since, as Hockett (1985: 318) observes: "An initial point of importance about every possible sort of historical evidence is that . . . it must endure. Instantaneous observation is impossible."

21 In our view, this fact casts serious doubt – perhaps even fatally so – on the "Uniform Rate Hypothesis" (URH) that has evolved from Kroch's 1991 "Constant Rate Effect" (CRE). Admittedly, PINTZUK's chapter (15) portrays that proposal (the URH) in a quite favorable light, but it is our conviction that the order in which specific changes appear in written language need not reflect the order in which they first appeared in colloquial speech. In particular, we believe that novel patterns which arise individually in spoken language may cumulate for a long period of time before they jointly achieve a breakthrough, as a set, into writing. If we are right about this, then uniform rate increases across patterns characteristic of written texts may correspond to chronologies for spoken forms which were far from uniform as regards the latter's origin and spread. We therefore find it quite astounding that diachronic syntacticians – both formalists (who focus heavily on the apparent simultaneity of certain developments) and quantitative variationists – so rarely discuss the fact that their crucial and often only data are documents whose religio-juridico-belletristico-commercial nature represents exactly the kind of high-style written language whose reliability as evidence for the vernacular engine driving changes in progress has been consistently called into doubt by sociolinguistics like Labov (1972a) and Kroch (1978). In short, empirical verification of the URH will not be forthcoming until students of syntactic change begin to carry out serious long-term investigations of ongoing developments in contemporary colloquial speech. Still, it remains true that much can be learned from historical-syntactic work based on written texts as long as (i) the documents at issue are simultaneously subjected to careful selection and to evaluative grading (vis-à-vis their degree of (in)formality; see, e.g., n. 29 below) and (ii) it is understood in advance that not all apparent "results" actually possess the direct bearing on questions of linguistic change that they superficially seem to have. In short and in general, then, research into the language of any given historical period can only work with the best evidence at hand, but (to echo the title of a synchronically oriented anthology on Optimality Theory (OT) compiled by Barbosa et al. 1998) we must periodically challenge our conclusions with the query: "Is the best good enough?"

22 There is also the possibility of gaining information about change from the comparative method, as discussed below; see also chapter 1, by ROBERT L. RANKIN. The method of internal reconstruction, described by DON RINGE in chapter 3, could likewise be mentioned here, although that method could also be taken to be mainly a matter of applying what we know about change in order to learn something about language history, rather than as a means to gain new information about change per se. As a supplement to the much more detailed but somewhat dry overview of "Sources of historical linguistic evidence" in Hodge (1972), see Rauch (1990) for an engaging but still quite detailed account surveying the variety of information about change that can be gleaned from textual interpretation (of various sorts, including the analysis of loanwords) and from considerations of other sorts, including typology and reconstruction. Cable (1990) and Kyes (1990) may also be consulted for general discussion of a similar nature, especially regarding orthography; for discussion of philological methods in the study of Native American languages, see Goddard (1973).

23 This is because, in Lightfoot's framework (based on Wasow 1977), indirect passives would involve a non-local application of a

passive rule and thus would have to be derived transformationally. (Here, we use the terminology of the 1970s – i.e., lexical versus transformational passives – even though many more current versions of the intended distinction would no longer refer to "transformations.")

24 The relevant sound changes are the loss of *w and the loss of *s – intervocalically as well as, later, initially. The meaning for *éor* given by Hesychius is not only 'daughter; relative; kinfolk,' but also "(male) cousin," so we must clearly reckon with a semantic shift, too – not surprisingly, perhaps, since Greek innovated a new word for 'sister' (*adelphḗ*, from *sm-g^{w}elbh-, literally 'having the same womb').

25 There is another side to the accidental aspect of attestation. Since the availability of documentary information largely determines the accuracy of any account of the past, any skewing of available knowledge has the potential to have a significant impact on how the past is interpreted. Thus, Weinberg (1988), observing that the paper upon which documents were written by most officials during World War II was of exceptionally poor quality and thus will not survive as long as will, say, the papyri or cuneiform tablets of the ancient Eastern Mediterranean and Middle East, notes further that only one World War II German official, one "Bevollmächtigter des Reichsführers-SS für das gesamte Diensthunde- und -Taubenwesen" (i.e., the "Plenipotentiary of the National Leader of the SS for All Military Dog and Pigeon Affairs"), was "equipped with an exemplary stock of paper." He then comments wryly that "it will be interesting to read histories of World War II based on the surviving records of that agency." Such histories based on selective – and accidental – survival are not just a figment of Weinberg's imagination, since one can cite works such as Chadwick (1976), in which one of the decipherers of the Mycenaean Greek Linear B tablets deliberately set out, in a very interesting and enlightening study, to "present a picture of Mycenaean Greece as it can now be reconstructed from the documentary evidence" of the tablets alone (p. x), rather than relying on (supporting) archeological evidence. What we see of Mycenaean life in such a (perhaps artificially restricted) study is thus selectively, and accidentally, restricted to what can be gleaned from the records of activity in the Mycenaean palaces in the few weeks before their destruction at the end of the thirteenth century BC. Similarly, as Bailyn (1986: 9) points out, "the most extensive run of detailed information about any large group of immigrants [to America] in the colonial period was produced just before the Revolution by the British government, responding to fears that the mass exodus to America then under way would depopulate the realm"; this skewing of information about who settled in British North America has been a boon to linguists, for it has enabled research into the bases of varieties of American English, and into the important role played by Irish and Scots settlers, that would be impossibly speculative otherwise (see, e.g., Rickford 1986 and Winford 1997 on the influence of

these settler dialects in the formation of African American Vernacular English). See also n. 28 below.

26 For example, Spanish *mierda* and French *merde* are both used in this way, and they continue a different lexical proto-form from their Modern English counterpart.

27 Anyone tempted to turn up his or her nose at the subject matter of this paragraph should see n. 31 below.

28 The degree to which surviving documents – especially printed ones – create an illusion of at least locally unchanging permanence in language is quite striking. To take a concrete example, we have open before us at this writing, as one outcome of the vagaries of book preservation (and collecting), a 1775 German work printed in Quedlinburg, Saxony-Anhalt (then part of Brandenburg-Prussia): *Des Claudius Aelianus vermischte Erzählungen* (in English, "*Aelian's '(Historical) Miscellany'*"; in Latin, "'*Varia historia*'"), translated (and annotated) by one J. H. F. Meineke from the Greek original (*Poikílē historía*) written by a Roman author who flourished in the third century AD. The covers of this book are somewhat the worse for wear, but the 600-plus pages between them are better preserved than those of most volumes printed in, say, 1875, and the text of the language thereon has, to all intents and purposes, now remained unchanged – in a documentary sense – for well over two centuries. If this collection of "morally improving" human stories and unrelated animal facts were the only available document from the East Middle German area for, say, fifty years on either side of the date in the translator/annotator's foreword, we would have little sense of the linguistic ferment characteristic of German during this era. The relevant paradox, then, is that such long-surviving linguistic artifacts can misleadingly tempt us to underestimate the speed and extent of language change, but we at least have access to them, whereas non-surviving documents (when we know of their existence, as we often do) connotatively suggest a more realistic picture of variation and change, but we cannot consult them. (Readers can test this assertion by asking their historical-linguist colleagues, "Which language had more dialectal diversity, Old High German or Gothic?"; we wager that the most common answer – or at least initial response – will pick Old High German, due to the relatively wide temporal and geographic variety of OHG's written attestations versus the extreme concentration of written Gothic in Wulfila's Bible translation – despite the much greater geographical dispersal of various Gothic-speaking groups: for example, from Crimea to Iberia just along the east/west axis.) For a twentieth-century historian's masterful discussion of electronic-age parallels to such problems of documentary preservation and their consequences for later historiography, see Weinberg (1988: 329–31, 335–6). And, for a specifically linguistic perspective on this and related matters, see Hockett's (1985: 32) discussion of such issues as the fact that "an inscription or manuscript may last for centuries or millennia before it has crumbled or faded beyond legibility."

29 Historical linguists sometimes are in the fortunate position of having access to earlier texts which are deliberately crafted so as to approximate colloquial usage or the like, such as plays or other works of fiction containing vivid dialogue. Still, since these works are constructed and so may contain stereotyped linguistic features or atypical frequencies (even if these exaggerations have some basis in reality), they must be used judiciously; they certainly cannot be uncritically taken at face value. (A relevant cautionary note along these lines is already sounded by Labov's (1972a) demonstration that the speech of "lames" – marginal members of American inner-city social groups – seems authentic to outsiders but can be shown by variationist techniques to be quantitatively deviant from the speech of core group-members.) For an intriguing study utilizing dialogue from Portuguese poems and plays of the fifteenth and sixteenth centuries in order to support a particular position on the origins of pidginization, see Naro (1978).

30 The most vivid and eloquent characterization of the relation between colloquial speech and written varieties of language is – in our opinion – that of Vendryès (1925: 275–6, trans. Paul Radin): "The . . . creation of written language[(s)] may be compared to the formation of a film of ice on the surface of a river. The ice borrows its substance from the river . . . [;] it is indeed the actual water of the river itself – and yet it is not the river. A child, seeing the ice, thinks that the river exists no more, that its course has been arrested. But this is only an illusion. Under the layer of ice . . . [,] the river continues to flow down to the plain. Should the ice break, one sees the water suddenly bubble up as it goes gushing and murmuring on its way. This is an image of the stream of language. The written tongue is the film of ice upon its waters; the stream which still flows under the ice that imprisons it is the popular and natural language; the cold which produces the ice and would fain restrain the flood . . . is the stabilizing action exerted by grammarians and pedagogues . . . [. A]nd the sunbeam which gives language its liberty is the indomitable force of life, triumphing over . . . [prescriptive] rules and breaking the fetters of tradition." We note though that for many speakers of a "dialect" (or linguists describing one), the sociolinguistic reality typically involves measuring their usage against that of the standard, often leading to a diachronically inaccurate, but synchronically no less real, mapping between the standard and their dialect, with dialect rules and generalizations derivable from those of the standard language (via what Andersen 1973 has called "adaptive rules").

31 The ennobling of coprolites via their use for modern scientific purposes surely reached its acme in the literally celestial heights aimed at by Buckland (1836: 154), whose treatise "on the power, wisdom and goodness of God . . . as manifest in the creation" included a long section on the evidence for masterly design found in the structure of ichthyosaurus intestines – which, though reconstructible only from fossil feces, fully demonstrate the extent of divine care and attention to detail inferable from "the beneficial arrangements and compensations . . . even in those perishable . . . yet important parts" (cf. Gould 1987: 99–100). On the subject of the archeological (and paleontological) value of coprolites more generally, cf. Renfrew and Bahn (2000: 12, 240, 244, 255, 269, 296, 306, 379–380, 424, 442, 477, 481–2, 501–2, 566).

32 Cf. the similar comments of an anthropologically well-versed linguist – Hockett (1985: 323): "archaeologists['] . . . evidence is . . . especially those rich concentrations of human byproducts in the cesspools . . . , garbage dumps, slag heaps, trash piles, and abandoned buildings of the world." The (non-linguistic) anthropologist Salwen (1973) has extended this trend to its logical conclusion by, for example, making his students of urban archeology aware of the parallels that exist between "the defacement of statues of gods and kings . . . follow[ing] . . . the conquest of one ancient state by another . . . and . . . [the] examples of vandalism [which] are a frequently visible part of the urban setting." Much as Rathje does, Salwen poses the question (p. 154) of whether "it might be argued that a site becomes the proper domain of the anthropological archeologist as soon as . . . [a] behavior stops and . . . the actors leave the scene!" (cf., as well, the "industrial archaeology" discussed in Hudson 1971: 1, who focuses on "material relating to yesterday's manufacturing and transport which has survived, more or less intact, on its original site"). This orientation is strongly parallel to one recommendation made here in section 3 below: that students of language change spend a substantial fraction of their time investigating ongoing linguistic developments occurring in the present.

33 The Vulgar Latin characteristics exemplified by Pompeiian graffiti and by the *Appendix Probi* presumably both reflect primarily urban speech-forms close to those found in Rome itself, while the Vulgar Latin traits found in the wood strips excavated near the Vindolanda fort in Britain are likely to include a greater number of rural forms. Still, Joseph and Wallace (1992: 105) have established connections between these two sorts of non-Classical Latin by presenting evidence that a "transformation of originally geographic variation into socially determined variation in an urban setting resulted from migrations into Rome and the expansion of Rome after the fourth century BC."

34 See Janda (1995) for a discussion of related problems which make it difficult not only to arrive at but also to organize and present a history of earlier English in a manner that does justice to those continuities between Old English and Middle English which can be established. Recall also the related dictum made famous by the British historian Lord Acton: "Study problems in preference to periods" (often quoted as "Study problems, not periods!"); see Dalberg-Acton (1895, quoted from 1930: 24).

35 Nonetheless, despite this lack of direct continuity in our records of English, it is common for linguists to make comparisons across the different periods of the language as if they were truly meaningful; this is a graphic instance of

Labov's characterization of historical linguistics cited above in section 1.2.1, since in doing so, one is simply making the most of the imperfect situation that the accidents of the attestation of English have provided, and letting an indirect ancestor stand in for the unattested direct ancestor. Although dialects can differ radically from one another, this step is based on the reasonable assumption that a given non-ancestral dialect is likely to be linguistically closer to the unattested ancestor than any other available point of comparison.

36 The "present" is a moving target, of course, since time continually – and continuously – keeps pace with change (which, it has been said, is the only constant; cf. Swift 1964: 251: "There is nothing in this World constant, but Inconstancy"). There is always a "present moment," yet, in virtually no time at all, one current instant yields to another (in a way often described as "slipping into the past"). Still, by "present," we here mean all moments within recent memory that remain potentially salient for speakers, or some reasoned extension of such a notion. Such an extended present may seem parallel, on a greatly enlarged scale, to the "specious present" (cf. Clay 1882: 167), also known (at least since Calebresi 1930) as the "psychological present" – a notion which has been adopted or discussed by psychologists and philosophers like James (1886: 374–9/1890/1918: 605–10); cf., for example, Mabbott (1951), Whitrow (1961: 71–7), Turetzky (1998: 125, 158), and their references. However, as Mellor (1998: 9) points out: "[I]f . . . [t]he present . . . [were] confine[d] to the present moment . . . [,t]hen many events . . . which last some time . . . would never be present. This problem has prompted the doctrine of the so-called 'specious present', which lets the present encroach a little on the past and the future. But by how much – a minute, a nanosecond? . . . [Here,] what is specious is the idea of a specious present, not the present itself." Therefore, Mellor (1998: 9) continues: "[t]he right way to define the present is this . . . [: i]n 1943, World War II stretched four years into the past and two years into the fuure. Yet it was certainly present then, as any combatant would then have testified. So its . . . time, a six-year . . . interval including the present moment, should, despite its length, count as present. Similarly, we should call any . . . time 'present', however long it is, if and only if it includes the present moment. That makes *this century* as present [a] . . . time as *today* or *this moment*. And so it should, since a centenarian whose . . . time it is will obviously be present throughout it." Thus, as long as we respect some such lifetime-length limit, we can argue that, for example, the 1950s are still "present" for many of us, even though they are over forty years removed from the time of this writing. It is this extended sense of "present" which allows us to discuss synchronic "slices" of a language that are broader than an instant, and which makes it meaningful to treat, say, "late-twentieth-century American English" or the like as a present (but *not*, for example, a 400-year period like "Middle English," often dated *c.*1100–1500). Perhaps the most revealing approach to the extension (i.e., extendedness) of the present moment was provided by Roman Jakobson (cf. Jakobson and Pomorska (Jakobson) 1988: 484): "[S]ynchrony['s being] . . . equated by Saussure . . . [,] both terminologically and theoretically . . . [, with] a static state . . . [can be] criticiz[ed by] . . . referr[ing] . . . to . . . cinematographic perception. If a spectator is asked a question of synchronic order (for example, 'What do you see at this instant on the movie screen?'), he will inevitably give a synchronic answer, but not a static one, for at that instant he sees horses running . . . [or] a clown turning somersaults."

37 At the very least, Ancient Greek dialects – such as East Ionic – which are "psilotic" (i.e., have lost Proto-Greek word-initial *h-*) constitute a counterexample to this claim, as Hock (1993b) has followed Allen (1976) in pointing out. Still, for the sake of argument, we nevertheless assume here that this claim could possibly be correct. A further part of Jakobson's claim here, namely that the presence of voiced aspirated consonants in a language implied the existence of voiceless aspirates, has been used by Gamkrelidze and Ivanov (1972, 1973, 1984 and elsewhere) and Hopper (1973) as an argument for their "glottalic" reinterpretation of the traditional reconstruction of the Proto-Indo-European obstruent system (touched on again in section 2.1). The fact that Jakobson was wrong about the one claim concerning aspiration makes us skeptical about basing too much on others of his putative universals. See Salmons (1993) for a useful summary of the "glottalic theory," and Hock (1993b) for an overview of various counter-arguments; see also Dunkel (1981), Garrett (1991), Job (1995), and Joseph and Wallace (1994) for some critiques of this "theory" and of the methodology.

38 The stage with [f θ x] but not [h] is in fact characteristic of what Ionic Greek ultimately developed into: namely, (standard) Modern Greek.

39 Indeed, given the existence of Ionic Greek (see n. 38 above), it is likely that this generalization is not an iron-clad one; instead, it may reflect a tendency rather than an absolute constraint on possible systems. Also, given what is known about the chronology of the change h → Ø vis-à-vis the fricativization of earlier voiceless aspirates in Greek, it seems clear that the loss of [h] occurred first. Hence, in the real-world analogue of the hypothetical case described here (in the main text), an earlier Greek system with both [p^h t^h k^h] and [h] passed first to a stage with [p^h t^h k^h] but not [h], and only then to a system with the relevant voiceless fricatives. The fact that Ionic Greek is not currently "in the present" is irrelevant; after all, it is a well-documented, attested language state, and thus in a sense it survives into the present via this documentation – and in any case, it existed at *some* "present."

40 Positing the putatively forbidden stage as a way-station – a transitory state that existed only briefly, between two "well-behaved" (i.e., typologically satisfactory) states – is extremely problematic. This is because, even if short lived, such a state would nonetheless constitute – for the entire duration of its existence, however evanescent – a possible human language. Presumably, therefore, nothing would require

the alteration of this stage (absent a substantive theory of markedness which would be able to demonstrate conclusively that certain elements or structures are measurably more difficult to acquire, retain, or use), and so the putative universal in question would have to be downgraded to a non-absolute constraint. Speakers living through a stage in "violation" of such a putative universal could not be expected to know – again, unless there existed some substantively worked-out notions of markedness (whether innate or acquired) – that they have to change their language state in order to conform to the universal at issue; for them, that state is simply what their language is! For further discussion of the problems besetting such "trigger/chain[-reaction]" theories, see Hawkins (1983) and earlier references there.

41 For further discussion of William of Ockham (or Occam) and his – or his predecessors' (as well as his successors') – relation to the razor-like principle of parsimony, see especially Boehner (1957: xx–xi), Adams (1987: 156–61), Beckmann (1990), and Maurer (1999), plus the bibliography in Beckmann (1992: 162) and the broad overview in Spade (1999). There somehow is something very fascinating, very winning, about this multifaceted figure from the late Middle Ages, who, though still a person of his time, penned volumes of writings ranging as far as the subject of politics (political science, one might even say) and encountered considerable risks and hardships due to the resoluteness of his own religious, philosophical, and political beliefs (e.g., he condemned the doctrine of papal supremacy over secular authorities outside of religious matters). In semiotician Umberto Eco's best-selling (1983) novel *The Name of the Rose* (*Il nome della rosa*, set in 1327), the fictional character called "William of Baskerville" ("Guglielmo da Baskerville") – likewise an English-born monk – arguably owes much not only to the fictional detective-hero of Sir Arthur Conan Doyle's (1902) *Hound of the Baskervilles* (i.e., to Sherlock Holmes) but also to the real William of Ockham. On the other hand, Baskerville sometimes mentions Ockham as one of his mentors and so must clearly be distinct from him; cf., for example, Haft *et al.* (1987) and the papers in Inge (1988). We mention fictional detectives here because, as Haft et al. (1987: 21) remind us, "historians . . . are Academe's quintessential sleuths," and historical linguists surely are no exception to this generalization.

42 For book-length studies on the new catastrophism, see the anthology by Berggren and Van Couvering (1984), as well as the following single-authored works: Albritton (1989), Huggett (1989), and Ager (1993), plus references there.

43 We have in mind here especially the French historical semanticist and general diachronician Bréal (1866: xxxviii–xxxix/1991a: 38–9) and the Danish classicist Madvig (1842: 56).

44 Drawing on suggestions made mainly in publications by Carozzi (1964), Mayr (1976: 343), Rudwick (1972), Burkhardt (1977), and von Rahden (1992), we provide below a list of European scholars who either advocated uniformitarian ideas or put them into practice before (sometimes long before) Whewell coined the term *uniformitarian(ism)* and basically credited Lyell with the corresponding concept. Instead of – (mostly) parenthesized – dates of publication, all of the years in this list are bracketed and indicate known or approximate lifespans. Among those deserving of honor as uniformitarians *avant la lettre* are, in chronological order according to birth year (and also in alphabetical order, in cases of shared birth years): Galileo Galilei [1564–1642], Marin Mersenne [1588–1648], René Descartes [1596–1650], John Wilkins [1614–72], Nicolaus Steno [1638–86], John Locke [1632–1704], Isaac Newton [1642–1727], Gottfried Wilhelm von Leibniz [1646–1716], Bernard Le Bovier, sieur de Fontenelle [1657–1757], César Chesneau Du Marsais [1676?–1756], Pierre(-)Louis Moreau de Maupertuis [1698–1759], Georges Louis Leclerc, comte de Buffon [1707–88], David Hume [1711–76], Jean Jacques Rousseau [1712–78], Étienne Bonnot de Mably, abbé de Condillac [1715–80], Georg Christian Füchsel [1722–73], Nicolas Desmarest [1725–1815], Anne-Robert-Jacques Turgot [1727–81], Horace-Bénédict de Saussure [1740–99], Peter Simon Pallas [1741–1811], Jean-Baptiste Lamarck [1744–1829], Jean-Guillaume Bruguière (1750–98], Déodat de Dolomieu [1750–1801], Alexandre Brongniart [1770–1847], Georges, Baron Cuvier [1769–1832], Karl von Hoff [1771–1837], George Poulett Scrope [1797–1876], and Heinrich Georg Bronn [1800–62].

45 Besides the authors mentioned in the main text, the following scholars had called attention to the non-monolithic (polylithic?) nature of Lyell's uniformitarianism before the 1980s (and the appearance of Christy 1983): Krynine (1956), Cannon (1960, 1961), Kitts (1963), Albritton (1967b), Goodman (1967), Hubbert (1967), Newell (1967), Wilson (1967) – the last five collected in Albritton (1967a) – Davies (1969), Hooykaas (1970b), Simpson (1970), Mayr (1972, 1976: 243, 248, 284–8), Rudwick (1972), Wilson (1972), Bartholomew (1973), Bowler (1976), and Ospovat (1977). As for during and after the 1980s, the corresponding list of scholars should include the following: Mayr (1982: 375–81, 875), Laudan (1987), and Le Grand (1988), among numerous others.

46 A directly related issue concerns the fact that, for times in the recent past, periods that are temporally closer to the present do not necessarily have more information available from (and about) them. Recall, for example, n. 25, where we cited the suspicions of Weinberg (1988) that, given the extremely poor quality of most paper used during World War II, it could happen that the greater survivability of the small, somewhat randomly distributed supplies of high-quality paper used during that conflict might give a skewed picture of major international events (e.g., if they reflected only the perspective of officials who managed the use of dogs and pigeons for military purposes).

47 An idea of the debates now actively raging about the nature of family life in earlier times can be gained by consulting the following: Shorter (1975), Stone (1977), Trumbach (1998, among other works), and Ozment (2001, among other works). Though reptilian monsters clearly have not always

had a major appeal for youngsters, modern-day children often display a strong interest in dinosaurs – whereby the amount of information now known about these creatures reflects increasing application of uniformitarian principles in paleontology. Thus, for example, research on dinosaurs has been little short of revolutionized by the incorporation of insights gained from the study of living creatures – as regards, for instance, their anatomy and physiology, as well as their behavior in mating, nesting, herd-travel, etc. For passionate advocacy of such reptilian uniformitarianism addressed to a general audience, cf. Bakker (1975, 1986), as well as Horner and Gorman (1988).

48 McLeish (1996: 14), for example, excoriates the strong non-uniformitarian trend in nineteenth- and twentieth-century classics education. He writes: "European universities became filled with magnificently reconstructed texts which . . . no one bothered to relate to the living beings who had created and enjoyed them in the first place. This miserable tradition persisted well into our own time. In the 1950s, some schoolmasters were still telling their pupils not to visit Athens in case its untidy charms spoiled appreciation of the true 'glory that was Greece'. . . . That there was life beyond the dative absolute, that the relevance of the ancient world was not a matter of texts and lists but involved the common human elements they contained, the flesh, blood, tears, lust, ambition, joy, despair, sweat, sperm – this was something that few self-respecting [Oxbridge] dominies ever thought to share with impressionable adolescents." For a discussion which threads its way skillfully around presentism, past antiquarianism, through immediatism, and near some of the many and varied other *-isms* which cluster around uniformitarianism (although the author in question does not actually use the latter term), see the short quotation from US historian Fischer (1989: ix) in n. 143.

49 It has been known for quite some time that, at the very least, Russell's (1903) definition of change has two rather problematic – or at least counterintuitive – consequences, one of which was recognized by Russell himself. The less serious of these (cf., e.g., Charlton 1995: 129) involves the fact that, while a transition from something to (virtually) nothing does indeed seem to constitute a change (e.g., an explosion that vaporizes a table clearly changes the table), a transition from (virtually) nothing to something does not obviously seem to change the latter entity (e.g., a carpenter who builds a table is not usually said to have changed the table). Yet both cases involve a situation where "A table exists" is true at one time but not at another time. More serious (cf. Crane 1995: 115, ultimately following Geach 1969: 91, 99) are instances where Russell's definition implies that one entity has changed solely because its relation to another object has been reversed by an alteration physically affecting only that other object. Thus, if "Our mothers are taller than we are" once held true at some time but now no longer holds true (because we have grown taller than our mothers), it counterintuitively follows from Russell's definition that our mothers have changed and that this is entirely due to growth on our part. Given such unusual characteristics of the definition for change which Russell published during his early years as a fellow of Cambridge University, Geach (1969: 71, 1979: 90–2) later initiated the still-current practice (cf., e.g., Strobach 1998: 132 et passim) of distinguishing between the "Cambridge (Conception of) change" – also known as "C-changes" – and genuine change; cf. also Cleland's (1990) paper "The difference between real change and *mere* Cambridge change."

50 On the impossible but endearing figure of McTaggart – who achieved something close to notoriety as a nihilist among philosophers for his above-mentioned denial that time exists, and who was known to be a convinced atheist – cf. Geach (1979: 6 et passim), who quotes from Dickinson (1931) this 1st-person statement by McTaggart: "The longer I live, the more I am convinced of the reality of three things – truth, love, and immortality."

51 Cf. Carlson (1977), whose distinction between "individual" and "stage-level" predicates obviously intersects with – but arguably is not identical to – the distinction discussed in the main text.

52 Here we implicitly echo Hoenigswald's remark (1960: 3n.5) that "any historical statement contains, avowedly or otherwise, at least two synchronic statements – one for each of two or more stages."

53 A striking parallel to Bynon's (1977) and Bloomfield's (1933) implied claim that the present has insufficient temporal length to permit insightful research on linguistic change can be found in Plog's (1973: 181–3) discussion of archeology as "diachronic anthropology" ("the study of temporal variability in human behavior and the products of that behavior"), as distinguished from "synchronic anthropology" ("the study of spatial variability in human behavior and its products"). Plog first asks: "If a scholar is interested in behavior and cultural processes, why would he [or she] not choose to work with these topics using the far richer sociocultural record of the present . . . [,] rather than the limited and elusive record of the prehistoric past?" He next mentions two possible reasons for preferring the study of non-contemporary culture and artifacts – because of "an intrinsic interest in the past," or because "there may be sociocultural phenomena . . . in the record of the past that do not occur in the modern record" – but then downplays these in favor of a third "justification for a science of past sociocultural phenomena," one that "focuses on change in time." Namely, argues Plog: "By and large, it is difficult and even impossible to study sociocultural change using modern data. Adequate event records that describe sequences of change cover longer periods of time than most ethnographers spend in the field . . . [,] periods . . . sometimes longer than the lifetime of a scholar. But such event records or sequences are the everyday concern of the archeologist." However, this conclusion totally overlooks the crucial difference between *diachronic correspondences*

and *changes* (and *innovations*) discussed here above in section 1.2.1: in terms of this distinction, it is archeologists who are usually in an inferior position when it comes to describing and explaining change(s). And, in any case, there is no law which prevents scholars – in anthropology or linguistics – from organizing studies of ongoing change in such a way that their window of data-gathering and analysis spans more than one lifetime (for further discussion of this and related issues, see section 3 below).

54 The spatial-orientation metaphor here derives from the standard "tree"-like schematization employed for showing language relationships.

55 That is, a critical part of the comparison process involves the interpretation of texts, whether or not these consist of direct testimony (such as inscriptions, manuscripts, personal letters, public documents, etc.) or indirect testimony (such as comments by travelers or grammarians about some first or second language). See n. 22 for references regarding philological methodology.

56 See n. 20 above.

57 More accurately, we should here say "between related speech-forms," since the comparison in question could be one across dialects or could even involve a comparison of variable realizations for some feature across (but firmly within) a given speech-community.

58 That is, if related language A and related language B disagree in some comparable feature, then either their immediate common ancestor proto-language was like A, so that B is innovative, or it was like B, so that A is innovative, or else it was like neither, so that both must have innovated.

59 And recall the problem with establishing lineal continuity in English (or any language, for that matter) discussed in section 1.2.1.6.

60 This formulation represents an unusually eclectic blend of approaches to grammar, reflecting (or at least intending to reflect) the work not only of Chomsky and other generativists, as well as of Labov and other variationists (who come more to the fore in the following main-text paragraph), but also of Coseriu (whose views have influenced many semiotically inclined linguists). For further discussion of norm, speech, system, and the additional notion of type, see Coseriu (1952, 1958, 1968, 1982).

61 Hoenigswald (1960: 2), for example, observes that "disappearing discourses may be *replaced*, in what must be called the 'same' life-situation, by new discourses . . . [; t]he study of the effects of loss, emergence, and, more properly, replacement of discourses . . . [–] that is, the study of linguistic *change* . . . [–] is the subject matter of *historical* (*diachronic*) linguistics."

62 And certainly earlier than its first documented occurrence in writing; see section 1.2.1 (and n. 21) for some relevant discussion.

63 And, for many proponents of grammaticalization (see, e.g., HEINE's chapter 18), change possesses a distinct directionality, which, it is claimed, is obvious and recoverable, at least for linguists. Even though, as documented by Janda (2001), they tend not to dwell on the role of speakers in change, advocates of grammaticalization presumably thus tend to see directionality as something which speakers, too, could be aware of, and from which they could then gain a sense of historical perspective on their language that is wholly derived from synchronic evidence available to them. However, ordinary speakers do not always do what linguists appear to believe they ought to do (see Joseph 1992 for some discussion of "opaque reanalyses"), so there is no reason in principle why speakers would infer historically correct directionality from synchronic evidence. Moreover, there in fact exist numerous cases of "counter-directionality" in the literature (see Janda 2001 for a list and discussion); that is, changes that run counter to the directions claimed by grammaticalization theorists to be natural or uniquely attested. The problem, as we see it, comes from linguists necessarily adopting a perspective on a language (e.g., through access to information about earlier stages, about related dialects and related languages, etc.) that is different from the perspective that any normal native speaker of that language, especially a preliterate speaker, could possibly take. The actual historical directionality for a change need not matter to speakers, as long as they can construct *some* mechanism to account for a particular alternation or relationship within their language. See, for example, Anttila (1972) on a speaker's synchronic relating of non-cognate tokens of *ear* (of corn and on the head).

64 Montelius studied the axes, clasps, knives, and swords of the Iron Age, and also extended some of his conclusions based on Scandinavian findings to other parts of Europe, but "the grand old man of Swedish archeology" is best known for his chronology of the Nordic Bronze Age, *c*.1800–500 BC, which – based on a typology of bronze objects – he partitioned into subdivisions still referred to as "Montelius Periods I–III" (Early Bronze Age) and "Montelius Periods IV–VI" (Late Bronze Age); cf. Sørensen (1996: 623). The particular typological method used by this "Linnaeus of archeology" involved establishing sequences of artifacts ordered according to the assumption that, to the extent that two objects are near to each other in shape, they must also have been near to each other in time. Despite his strong evolutionary bias, though, Montelius was interested in diffusion, too, arguing that the institutions and technologies of European society had originally come from Asia – a view dubbed the *ex oriente lux* ("light from the East") brand of Near Eastern diffusionism; cf. Klejn (1996: 286–7), McIntosh (1996: 283). On both the life and the work of Montelius, see the papers in Åström (1995); for a critical but fair assessment of Montelius's typological method (which seems to have been slightly anticipated by his colleague Hans Hildebrand), see Gräslund (1987: 56–120); on the general history of Scandinavian archeology, cf. Klindt-Jensen (1975).

65 Gräslund (1987: 5–12, 86–90) shows that Montelius avoided some of these ambiguities by using two strategies in tandem: (i) his "typological" method (focused on the serial development of one type of object across many find-sites), and (ii) the "find-combination method" (focused on the totality of objects found at each site). The kind of problem thereby avoided is

familiar to linguists. For example, when told that someone has found three texts with the respective schematic characteristics (i) ABC, (ii) AEI, and (iii) GHI, we cannot be sure whether these texts reflect a diachronic sequence (i) ABC > (ii) AEI > (iii) GHI (among other options) or a synchronic simultaneity that arose because these texts come from three adjacent languages which had the characteristics (i) ABC, (ii) DEF, and (iii) GHI until, via borrowing, language (ii) replaced its D with (i)'s A, and its F with (iii)'s I, yielding AEI. Such two-edged borrowing by a geographically intermediate group can happen in language or in material culture, and so cause not only linguistic but also archeological ambiguities – at least when a researcher uses only the "typology" of Montelius, as he himself sometimes seems to suggest that he did. Altenderfer (1996: 727), though, says in Montelius's defense that, before "the advent of absolute dating techniques, . . . typological analysis, . . . with stratigraphic excavation, was the only means by which archaeologists could develop cultural-historical sequences or otherwise measure the passage of time": that is, through "the systematic arrangement of material culture into types based on similarities of form, construction, decoration . . . [,] style, content, use, or some combination of these."

66 The only problem with using Montelius's (1899) developmental sequence of mid-nineteenth-century train-cars – also variously known as *railroad/railway car(riage)s* – to illuminate the parallel discontinuity of language transmission among humans is that the train-cars in question were the manufacturing products of three different countries: Britain, Austria, and Germany (for the Swedes). As such, they do not appear, at first glance, to represent a single line of development. Rather, these conveyances might collectively seem analogous to the situation of Old English (OE) versus Middle English (ME) discussed above in section 1.2.1.6 – that is, that the (documentarily) predominant dialect of Late OE is poorly attested in Early ME, and vice versa – which might support a claim that the discontinuity at issue is found not within one entity but across multiple entities. For our present purposes, however, this is a difficulty of practice, rather than principle. First, we assume that Montelius (1899) used train-cars from three different countries because a chronologically equivalent sequence of readily comparable drawings with train-cars from one country was not available to him (this is largely the basis for our own choice, at any rate). We therefore commit to securing a return ticket and coming back with a unified (i.e., intranational) set of drawings in the future – and we wager that these will exhibit the same characteristics. For instance, we have already found, pictured in von Röll et al. (1917: 17), a British train-car from 1838 that is virtually identical to Montelius's 1840 Austrian one; it is also probable that the German-made Swedish train-cars from *c.*1857 were based on British models (cf. von Röll et al. 1917: 18). Second, since the British train-car of 1825 was directly copied by most European railway systems, including that of Austria before 1840, and since there were contacts between German and Austrian train-car builders between then and the 1850s, an alternative case can be made that Montelius's sequence of train-cars does represent a single line of development (i.e., what we called "direct lineal descent" in section 1.2.3.8).

67 To prove that such transportation-based examples can literally get off the ground, we can cite identical developments among more modern conveyances, like jet airplanes: for example, the more recently introduced Boeing 767 jets have not yet crowded all the older-model Boeing 747s from the skies, though someday they may, just as the much earlier Boeing 707s and other jets eventually replaced most (but not all) propeller-driven airplanes from the business of transporting large numbers of passengers over long distances. Yet even supersonic air-travel in the twenty-first century, just like Montelius's (1899) sequence of mid-nineteenth-century trains, involves a remarkable carry-over from the latter's precursors – stagecoaches: English-speakers still commonly talk about "flying coach (class)" ("traveling by air while seated in a plane's economy-fare section").

68 Essentially this conclusion was expressed (much more memorably) by Collingwood (1946, here quoted from 1993: 482–3): "The whole of the present consists of traces or residues of the past, for the present is that into which the past has turned, and the past was that which has turned into the present. To speak, therefore, of the traces of the past in the present is to speak of the present and nothing but the present." Such argumentation is similar to that used by Thomason (1980: 419) in a book-review passage that ends by likewise addressing issues of language change: "If . . . [it] is correct . . . [to] repeat . . . that '"everything" in language is analogical' . . . , then . . . [it] is also correct – trivially – . . . [to] say . . . that analogy explains everything in language. But then it is equally correct to say that analogy explains nothing . . . [,] and we must re-invent traditional analogy, under other names, . . . to provide explanations for specific linguistic changes or types of changes."

69 The view of change and/or non-change that emerges here provides some insight into a matter of some concern to historical linguistics, namely whether a language such as Vulgar Latin (as attested in the Pompeiian graffiti, for instance – see section 1.2.1.5) is a dead language or not. On the one hand, one could argue that it is still alive, being continued, albeit in an altered form, in the various Romance languages of today. On the other hand, one could argue that that precise form as recorded in Pompeii and reflecting colloquial usage of the first century AD is no longer with us and thus is extinct. Biology again provides a useful concept and term that are applicable to such cases: Scott (1996: 457), in defining the term *extinction* as "the discontinuation of the existence of an animal or plant species or taxon," notes that "many animals and plants . . . do not become extinct in the true sense; they undergo *pseudo-E[xtinction]*, i.e. they disappear from the fossil record by evolving into something else (the genome is not lost but altered)." Thus, Latin could be said to be "pseudo-extinct," whereas

a language such as Hittite or any of the once hundreds of native languages of the Americas, which were not continued in any form since their speakers shifted to another language entirely or else died out without linguistic issue, would be truly extinct (dead) languages.

70 Colloquially, a *clone* is 'a virtually identical copy,' and so *cloning* can refer to the direct copying of a complete, full-sized (e.g., mature-adult) version of some entity. But a *clone* in the technical sense was originally – in the term's first English use, in 1903 – "the aggregate of the asexually reproduced progeny of an individual," later also "a group of replicas of (all or part of) a macromolecule (like DNA or an antibody)," and now most often "a genetically identical offspring grown from a single somatic cell of its parent." But one kind of cloning has existed for thousands of years: the cuttings used to create genetically identical copies of plants (note that English *clone* is based on Greek *klōn* 'slip, twig'); it is only so-called "higher organisms," especially mammals, that are difficult to clone. For the latter, cloning requires considerably more complicated steps, as shown by the 1997 cloning of the lamb "Dolly" by Ian Wilmut's team (after 277 unsuccessful tries!), discussed in Kaku (1997: 225–7, 379). Still, the "virtually identical copying" sense of cloning is now essentially an additional technical meaning of the term, because at least one biologist has extended cloning-related terms like *replicate* from genetics to cognitive domains. That is, the *replicators* first proposed by Dawkins (1976: 15–20, 191–3, 254, 269–74, 322–3, 1978, 1982a; cf. also Hull 1980, 1981) and since characterized (Dawkins 1982: 83) as "any entit[ies] . . . of which copies are made," including (Dawkins 1986: 128) "self-copying entities," have always included *memes* (from *mim(e)-eme-s*): units of information (ideas, styles, etc.) that reside in structures like brains, books, or computers. It is thus not surprising that several historical linguists have avidly promoted replication as a useful conceptual tool for dealing with language change (and especially with individual innovations, though this distinction is not always made): see, *inter alios*, particularly Ritt (1995), but also Janda (1994a, 2001: §5), Lass (1997: 111–13, 378–81), Johanson (2001) (who here, and elsewhere, characterizes borrowing as "copying"), and, with different terminology, Lightfoot (1999a: passim) and Croft (2000: passim). The notion of replication is especially useful for analyzing a phenomenon that results from the intersection of cross-linguistic (or cross-lectal) contact and hypercorrection: viz., the pseudo-loanwords that constitute hyperforeignism (and hyperdialectalism); cf. Janda et al. (1994). While the traditional term "borrowing" implies that something can never be borrowed into a language (or lect) A from a language (or lect) B unless it already exists in B, language contact surprisingly often yields "borrowed" words or phrases that are non-existent in the supposed source language. One such example is the English pseudo-Gallicism [ku də grɑ], which, as a pronunciation of supposed *coup de gras* 'stroke of grease,' is a failed copy – motivated by an overextended belief that "final consonants of French words are usually unpronounced" (as in *coup d'état* 'stroke of state') – of the true Gallicism *coup de grâce* "stroke of mercy." Such pseudo-loanwords can be seen to make eminent sense, however, if we give up the "borrowing" metaphor and instead realize that contact situations often involve attempts to create a replica, in one's native language (or lect), of a model found in another language (or lect) – whereby this replication may involve considerable distortion. Such an approach is not new; it goes back to Haugen (1950) and Weinreich (1953); for discussion, cf. Janda et al. (1994), plus, on related issues, Janda and Auger (1992).

71 As regards these criticisms of punctuated equilibrium, which range from the prosaically polite (as in a discussion of "Parallel gradualistic evolution of Ordovician trilobites") all the way to the polemical (as in Turner's 1986 characterization of punctuationism as "evolution by jerks"), it is not difficult to agree on a core set of references. Cf., for example, Gingerich (1974, 1976), Lande (1980, 1986), Levinton and Simon (1980), Stebbins and Ayala (1981), Charlesworth et al. (1982), Ayala (1983), Dawkins (1983), Maynard Smith (1983), Barton and Charlesworth (1984), Stenseth and Maynard Smith (1984), Turner (1986), Sheldon (1987), Kellogg (1988), Levinton (1988), Hoffman (1989), Dennett (1995), and Ruse (1999, 2000), plus more recent papers. It is worth noting that, in the case of several such critiques (especially Sheldon 1987), punctuationists have argued that a closer look at the relevant data supports rather than contradicts the central claims of punctuated equilibrium. At present, however, the most unassailable case of punctuated equilibrium in the biological literature remains that of the cheilostome bryozoans studied by Cheetham (1986) and Jackson and Cheetham (1990, 1994, 1999); to date, it has withstood all challenges.

72 Indeed, for a consideration of stasis from a linguistic standpoint, cf. chapter 5 by NICHOLS.

73 The other subtype of allopatric speciation (in addition to the peripatric variety, that is) has sometimes been said to involve a "dumbbell" model (since it typically involves the pinching-off of a comparatively narrow, bar-like space that once connected two bulbous lobes of population distribution; cf. Mayr 1963), although Bush (1975) speaks of "speciation by subdivision." A much more euphonious name for the same phenomenon is *dichopatric speciation*, in which (cf. Mayr 1997: 182–3) "a previously continuous range of population is disrupted by a newly arisen barrier (a mountain range, an arm of the sea, or a vegetational discontinuity)" in such a way that "the two separated populations . . . become genetically . . . different . . . [over] time and . . . acquire isolating mechanisms that . . . cause them to behave as different species when, later, they [again] come . . . into contact."

74 Discussion of this general topic can be found, for example, in Donovan and Paul (1998) and many references there. For a pessimistic assessment of the fossil record surprisingly in line with Darwin's (1859) views – one replete with implications not only for biological but also for

linguistic reconstruction – see Hennig (1969: 1–3). Further issues directly related to biological reconstruction, again useful as generators of heuristic comparisons with the reconstructive practices of historical linguists, are discussed in Scotland et al. (1994). Note also Eldredge's (1985: 69) judging of Cain's (1954) relief that the "fossil record is not complete" as "odd."

75 Mayr's own (1942/1982: 120) "biological species definition" is as follows: "Species are groups of actually or potentially interbreeding natural populations . . . which are reproductively isolated from other such groups" (a view which is both critically reviewed and compared with various alternative approaches in Wheeler and Meier 2000). Characterizations of this sort have sometimes moved linguists to suggest equivalences between the biologist's species and various linguistic constructs, such as *language*, *dialect*, *speech-community*, etc. (discussed herein in chapter 24 by WOLFRAM AND SCHILLING-ESTES). Although the intraspecies ability to interbreed might seem at first blush to match mutual intelligibility among (certain) speakers of different dialects within a single language, our own inclination is instead to match species with dialects, and biological "local populations" (or "demes") with speech communities (or communities of practice). This view receives support from the biological finding (cf., e.g., Mayr 1942/1982) that organisms which are in principle capable of interbreeding so as to produce viable offspring are nonetheless sometimes kept apart by factors that include acquired anatomical characteristics or behavioral tendencies. Thus, for example, a linguistic network is similar to a local population in consisting of members whose close proximity actually allows them to interact with one another, rather than organisms who could potentially interact (if they were brought together) but in fact do not do so.

76 Linnaeus' original (1750) statement of this principle arguably uses a plural form meaning 'leaps' as the object of ((*Natura*) *non*) *facit* . . . "(Nature) does (not) make . . . ," since *saltus* – more unambiguously *saltūs* – is indeed the acc.pl. of the Latin 4th-declension (masc.) noun in question. But other, later writers (e.g., Huxley 1859: 27) tend to follow Darwin's repeated use (1859: 171, 194, 206, 210, 243, 460, 471) of acc.sg. *saltum* in his invocations of "the canon of 'Natura non facit saltum', which every fresh addition to our knowledge tends to make more strictly correct" (p. 471). Hence we are entitled to suspect that some intermediary within the line of transmission between Linnaeus and Darwin wrongly believed the former's *saltus* to be the nom.sg. of a 2nd-declension masc. noun (one parallel to, e.g., *mūrus* 'wall') and so – wrongly – treated it as an incorrect case-form which needed to be replaced with "correct" acc.sg. *saltum*. Even linguists sometimes run afoul of the Latin 4th declension – as in Shibatani's (1976: xii) discussion of theoretical "*apparati*" (versus Latin *apparātūs*) – and this leads one to ponder whether the use of a plural-marking macron on *nexūs* in at least one philosophical work (*A Key to Whitehead* by Sherburne 1966: vi, 72–97 et passim) is perhaps not such an extreme solution, after all. We should note that it is not just non-native users of Latin who have been vexed by this problem: Roman writers themselves varied between 2nd and 4th declensions in, for example, using both *domī* and *domūs* as the gen.sg. of *domus* 'house.'

77 A linguistic analog of this scenario is unwittingly provided by Dawkins (1986: xvii), who devotes a brief complaint about instances of American English usage that have entered the United Kingdom to grumbling about the failure of young speakers in the United States to describe the prepublication evaluators of a book manuscript as its *referees*: these are, he writes, "not 'reviewers' . . . [,] *pace* many Americans under 40." Here, we can safely assume that an original situation in which a single main sense for *reviewer* reigned within a geographically unitary homeland (England) was later altered by a semantic change that expanded the sense of *reviewer* but occurred only in one peripheral, originally quite small set of British colonies (in North America) – whose citizens have now begun to spread their innovations (like *reviewer* as – also – "book-manuscript referee") back into the ancestral homeland. Thus a change via some form of cross-language or cross-dialect contact – cf. chapter 23 by THOMASON – is at issue here.

78 As pointed out in n. 21, this fact – that what change in documents most often reflects directly is the spread of an existing linguistic pattern into writing, rather than the spoken-language origin of that pattern in the first place – leads one to question the validity of Kroch's 1989 "Constant [or: Uniform] Rate Hypothesis [or: Effect]" (discussed here by PINTZUK in chapter 15, as well as by GUY in chapter 8).

79 Such timespans in geological terms take on particular interest in light of claims concerning possible temporal limits on the Comparative Method in the range of some 10,000 years; see, on this question, chapter 1 (section 11) by RANKIN and chapter 2 (section 3.3.1) by HARRISON.

80 In this connection, it should be mentioned that, as discussed more fully in section 2.3 below, grammaticalization is treated – to varying extents and degrees – by several chapters in this volume.

81 Interestingly, Lightfoot (1999a: 81–2) even describes the approach to grammar taken by a quantitative variationist sociolinguist like Labov as being consistently individual (and psychological) – "[a]s claim[ing] that speakers' grammars are psychological/biological entities existing in the minds of individual speakers" – despite Labov's own repeated insistence that understanding either the synchrony or the diachrony of a language requires the formulation of community grammars. For Labov (1994: 45n.2), after all, the conspicuous locus of regularity is the community, not the individual: "a language . . . [i]s a property of . . . [a] speech community," and so we must "avoid a focus on the individual, since the language has not in effect changed unless the change is accepted as part of the language by other speakers." The community-level focus of Labov (1972a, 1994, etc.) is thus indeed much closer to the species-level orientation of Eldredge, Gould et al. than to Lighfoot's concentration

on individual speakers. Another linguistic study with difficulties in the match-up between linguistic units and purported biological counterparts is Goodenough (1992).

82 One reflection of this fact is the principle of comparative reconstruction such that, especially when the change in question seems relatively unnatural (e.g., uncommon) and when the total number of sister languages involved is great, any change which is reflected in all the daughters of a given linguistic ancestor should be analyzed as having occurred once, in that ancestor, rather than individually in each sister. (Of course, considerations of parsimony are involved here, as well.)

83 While the heated debate and vigorous controversy that surround punctuationism show no signs of cooling off or quieting down, there appears to have emerged a tentative consensus that at least some speciation events are relatively punctual, while others are relatively gradual (cf., e.g., Geary 1990). Erwin and Anstey (1995a, 1995b), for instance, reviewed 58 previous studies that had been designed and carried out to verify the principal claims of punctuated equilibrium – a sample which not only included analyses representing a wide variety of taxa and periods but also, by its sheer size, tended to overcome deviations of individual studies from the strict criteria which have been advocated as necessary for any true test of punctuationism. Erwin and Anstey (1995b: 7) concluded that "paleontological evidence overwhelmingly supports . . . [the] view that speciation is sometimes gradual . . . [and without stasis, but] sometimes punctuated . . . [between periods of stasis; overall, then,] no one mode characterizes this very complicated process in the history of life"; it should further be noted that a quarter of the studies at issue reported a third pattern: gradualism with stasis. More or less the same divided conclusion regarding punctuationism (versus gradualism) is presented to college students of biology, evolution, and/or paleontology in such introductory textbooks as Futuyma (1979: 701), Strickberger (1990: 273–4), Ridley (1996: 562), Benton and Harper (1997: 52–3), Freeman and Herron (2001: 527), and Stearns and Hoekstra (2000: 274–5). Thus, for example, Strickberger ends his discussion of punctuationism as follows: "This dispute has generated many arguments and counterarguments . . . [;] all evolutionists agree that both gradual and rapid changes occur during evolution. What we have not yet resolved is the relative importance of these changes in explaining speciation and the evolution of higher taxonomic categories" (1990: 273–4). In this regard, one particularly significant finding concerns the fact that, where it exists, stasis does not seem to result from a lack of genetic variability. Avise et al. (1994) addressed this question by sequencing several genes in the mitochrondrial DNA of horseshoe crabs (the best known of the so-called "living fossils") and then comparing the amount of genetic divergence that they found within this clade to a previous study of genetic distances within another arthropod clade – the king crabs and hermit crabs – carried out by Cunningham et al. (1992). The

results were striking: Avise et al. found that horseshoe crabs show just as much internal genetic divergence as the king-/hermit-crab clade, even though the former have undergone far less morphological change over time than the latter.

84 As a parallel botanical example of stasis, Stebbins (1982: 21–2) cites the case of the plane tree, or sycamore, whose American species have quite recently been able to hybridize successfully with their (locally) introduced Mediterranean relatives in parks throughout the northern hemisphere and in the California foothills. This means that, "during the past 20 million years, plane trees that were separated from each other by a distance of 4,000 miles and grew in distinctly different climates have not evolved differences greater than those that distinguish breeds of cattle." In a nutshell, the visible differences distinguishing them are more extreme than their internal genetic differences.

85 We should note at this juncture that McMahon (2000b) likewise concludes another linguistic work (and one having biological and historical implications, as well) by quoting the last sentence from Voltaire's *Candide*. This is perhaps also an appropriate place to note that Croft's (2000) attempt to explain language change on the basis of an evolutionary approach was published recently enough that there has not yet appeared a sufficient critical reaction in the biological, paleontological, or (historical) linguistic literature which would allow us to quantify Croft's relative success or failure – to date – in his avowed goal of improving historical linguistics through the admixture of biological terms and concepts. On the other hand, we can already greet with approval Labov's (2001: 3–34) lengthy discussion of "The Darwinian Paradox" in the second volume (*Social Factors*) of his two-part investigation into *Principles of Linguistic Change*, where we take the author's increased attention to parallels between biology and linguistics as a positive sign because it represents a convergence with a similar development in our evolving plan for this introduction. Yet Labov (2001), too, has appeared so recently that it has not yet provoked a detectable groundswell of critical reactions in the current literature on biology, paleontology, and (historical) linguistics, and so – for the present – we will forbear from commenting further on the biology-related material in Labov's book, as well. Finally, we should here issue a blanket statement (covering all of both this and the previous section) that, although we have not always consistently maintained a terminological distinction between talking about change in language(s) and talking about change in grammar(s), we believe that our conclusions here do not depend on the individual choices between these sorts of terms that have been made at particular points in the main text.

86 Recall from n. 75, however, that (local) biological populations – or "demes" – are relatively small-scale units which thus seem to correspond more closely to linguistic networks or speech-communities, rather than to entire languages.

87 Thus, for example, Labov (1994: 98–112) discusses the "stability of individual phonological systems over time."

88 See Butters (1988) for documentation of this item, where it is said to be a "new" form. We regret citing a term of disparagement here (or anywhere), and do so only because it provides such a perfect example of the point that we are trying to make. Fortunately, many epithets of this type are of relatively short currency.

89 This usage was overheard by one of the authors (Joseph) at that camp in the summer of 1961.

90 Of course, one cannot rule out the possibility of there being some direct conduit for the spread of this usage, or some long-distance medium, such as radio, television, telephones, or the Internet. However, with processes which, like the clipping typical of slang, are quite common, we feel that the burden of proof would be on anyone claiming that there must be a direct connection between the two occurrences at issue. After all, an obvious play on words, for example, can be spontaneously created by several speakers (either in the same or in different locales, and either at the same time or at different times); it need not be the case that one speaker heard it from another. The experiences of Warren Peace, assistant principal at a high school attended by one of us (Janda), are instructive in this regard. Mr Peace reported that, whenever he moved to a new place, he always seemed to meet someone who, without any apparent influence from others, wanted to bestow on him the nickname *Tolstoy*, given the homophony of *Warren Peace* with the Russian author's famous novel *War and Peace*.

91 Such an assumption is parallel to what Gould and Wells are cited as saying in section 1.2.2.2 above regarding "nature's laws" being "invariant in space and time" (cf. also more generally Braithwaite 1953: passim). The trick, of course, lies in determining just what those laws in fact are – that is, for language, in figuring out what the universals are.

92 Of course, the history here ultimately involves a borrowing (since *homo-* is from a Greek form meaning "same"), but, as far as many "average" speakers of synchronic late-twentieth-century English are concerned, the connection between the form [hóumòu] and its referent(s) is purely arbitrary. The appearance of *m-* in a slang form of the word, or in two independent slang forms, thus ultimately has a long-term "historical" – that is, a polysynchronic – explanation (involving Ancient Greek, Renaissance-era humanistic borrowings of Greek morphemes into English, etc.), even if the absence of the fuller form's first syllable from the clipped slang form in the two relevant speech-communities does not.

93 Admittedly, Posner's later discussions (on p. 106 and especially pp. 419–22) tend to contradict this impression.

94 Of course, in such a situation, if alterations in one or the other language system occurred due to this external change in sphere of usage, or due to speakers' changing degree of familiarity with the languages at issue, this would not be surprising, since we would then be dealing with contact-induced language change (see chapter 23 by THOMASON), which is very different from the language replacement described here.

95 The ambiguity of *historical* (and *historic*) seems to represent a derivational continuation of the ambiguity inherent in *history*, which is often defined both as 'a branch of knowledge that records and analyzes past events' and as 'a chronological record of significant events, especially those affecting a people or institution.' These two senses are respectively given as (part of) the second and third meanings of *history* by Mish et al. (1997: 550), which is quite expected, since the practice of *Merriam-Webster's Collegiate Dictionary* is to provide first those senses which are etymologically older in English; thus, the first meaning that this work lists for *history* (attested starting in the fourteenth century) is 'tale, story.' Surprisingly, however, Pickett et al. (2000) list roughly the above meanings in essentially the same order, although this contradicts the usual non-etymological sequencing criteria of their *American Heritage Dictionary of the English Language*. Still, the latter work spells out a much more revealing pre-English etymology for *history*, whose roots extend back first from Middle English *histoire* to (borrowed) Old French *histoire* and thence, via Latin *historia*, to Greek *historía*, meaning primarily 'inquiry, research, or result thereof' (a sense still preserved in the phrase *natural history*) and derived via *historeîn* 'to inquire' from *(h)ístōr* 'knowing, learned, wise (person).' The last of these, in turn, has the reconstructed etymon *wid-tor- (compare English *wit*), a suffixed zero-grade form of the PIE root *weid- 'see,' and so is also related to Greek *eidénai* 'to know.'

96 We have ourselves sometimes wondered (usually in a whisper) whether there is not a need for some label like (antepenultimately stressed) *glossallagology*, from the Greek for 'language,' 'change' (*allagē*), and 'study,' or even *language-change-ology* (since the other major Ancient Greek word for 'change,' *metabolē*, would yield the hopelessly misleading expression *metabolic linguistics*). Unfortunately, we fear that, in a manner reminiscent of Jespersen's notorious characterization of Danish (his mother tongue), such terms – especially the former – might sound more like a throat disease than a serious attempt at conceptual clarification via terminological innovation.

97 As far as we know, a claim of momentous historic status for Templeton would be justified only if the above-mentioned sign at issue were intended to invoke the fact that actor James Dean had his fatal car-crash 25 miles east of nearby Paso Robles, in the even smaller town of Cholame, California, on State Route 46. But this is really quite a stretch as a fact about Templeton, since the crash in question took place at a site located two towns away. The alternative tack of claiming Templeton to be historic on the grounds that it has momentously arrogated that quality to itself solely by assertion (i.e., claiming that historic status can be gained just by making chutzpah-filled claims about history) is an intriguing notion, but it is not likely to be what the Templetonians themselves had in mind when they posted their sign. For Templeton's (or at least its Chamber of Commerce's) own views on the town's degree of historicity, see http://www.templetonchamber.com (but also http://www.ridenbaugh.com/travel/crv7.htm).

98 A relatively recent example of this phenomenon is provided by McCrum et al.'s (1986) *The Story of English* (not to be confused with Pei's 1952 book of the same name), which grew out of a very successful BBC documentary-like series originally made for television but now available in video format. Though this production belongs mainly to the domain of popular media, the public has come to view not only the book but also the filmed series as an extremely scholarly effort – which is especially unfortunate given that, in our opinion, the writers and producers involved in the project failed to provide an adequate overview across the history of English, due to their excessive focus on the putatively colossal contributions to the development of the language made by famous writers like Shakespeare. That is, what got lost in the alternating shuffle between literary luminaries, on the one hand, and sympathetically portrayed, less well-known varieties (like Irish English), on the other, was the pivotal role played over the centuries by the day-to-day conversational interactions and language use of "the English speaker in the street" – in, say, London or Philadelphia.

99 Shortly after writing this paragraph, we learned that Seamus Heaney's (2000) *Beowulf: A New Verse Translation*, had just become a bestseller in Britain. We take this as strong confirmation of our claim that public knowledge of earlier periods in the history of English is essentially limited to the name, or at most a bilingual translation of, only one text per period. Thus, if another famous writer were to make a vivid Modern English version of the long travelogue by the Norsemen Ohtere and Wulfstan (cf. Lund 1984) that was interpolated into the Old English translation of Paulus Orosius' Latin histories – a translation thought to have been personally supervised by King Alfred (cf. Bately 1980) – it would be unlikely to achieve even moderate sales, although the work in question is generally regarded as one of the most representative specimens of Old English prose.

100 In fact, any accounts that may have been written by historians concerning a sparrow's fall are likely to be more accurate than the majority of historical references to the end of the Roman Empire. The view most commonly encountered (cf., e.g., Benét and Murphy 1996: 883) holds that the last emperor – reigning from AD 475 – was (Flavius Momyllus) Romulus August(u)lus, who in 476 was forced to abdicate by the German general Flavius Odoacer, with the latter then exercising a short-lived rule over a German kingdom of Italy until 492. Grant (1990: 158–60, 215, 238), however, shows that Julius Nepos, Romulus' immediate predecessor as emperor (reigning 474–5), was imperially reinstated in 476 and – as indicated by his appearance on coins minted by Odoacer during this time – was officially recognized as Western Emperor until he was murdered four years later. The little-known truth is thus that the Roman Empire (in the West) did not end until AD 480, and that its last imperial ruler was Julius Nepos (the Grover Cleveland of Roman Emperors, since his tenure in office was interrupted by another's, just like the twenty-second (1884–8) and twenty-fourth (1892–6) president of the United States). Hence history proper greatly resembles linguistics (including diachronic linguistics) in that both fields are characterized by the unfortunate situation that most non-specialists and even some specialists "know" many "facts" about them which are not true. (A second, music-historical case of the same sort has to do with the nearly universal belief that Wolfgang A. Mozart regularly used a Latin form as his second name, *Amadeus* – whereas actually he always used the French equivalent, *Amadé*, for more than 13 years of his life, starting when he was 21; see Greither 1962: 7, 9, 49, 63.) Both history and linguistics (as well as their intersection, historical linguistics) thus confirm the wisdom of a comment once made by the Yankee humorist Josh Billings (pen name of Henry Wheeler Shaw): "It is better to know nothing [about a subject] than to know what ain't so" (cf. Billings 1874).

101 However, we must add the caveat that, given the number and complexity of the temporal issues discussed in most of the works just listed (solo as well as anthological), one can only rarely – even less often than in linguistics, we feel – give a blanket endorsement of all the claims or arguments in any individual study. Hence reading through the literature on time produces a kaleidoscopic picture continually altered by the adoption and rejection of relevant notions – some of which, in Augustinian fashion, seem (so to speak) alternately to fade in and out on the edge of cogency and comprehensibility.

102 As a concrete example indicative of the literally astronomical number of entities that exist in the universe, consider Dobzhansky's (1970: 1) report that a single human being consists of "about ten trillion . . . cells," together containing "some seven octillion . . . atoms" (i.e., seven times ten to the twenty-seventh power).

103 Lass (1997: 25) gives an example that makes this point in rather graphic terms that are far more concrete than Hockett's. Noting that neither the personal existence of the author Charles Dickens during the nineteenth century (1812–70) nor his birthdate (February 29, 1812) is subject to any dispute, Lass states that one reasonably secure inference to make is that Dickens's (biological) parents engaged in sexual intercourse at some point roughly nine months before Dickens's birth. While we ourselves do not deny that this inference is entirely reasonable, we note – as does Lass – that its absolute validity is only as solid as such beliefs as that Dickens was not an extraterrestrial and that human parthenogenesis was not possible in Dickens's parents' time. (Lass points out that matters would have been much different if Dickens had been an aphid.)

104 This also holds for Lass's Dickensian example (see n. 103): even if a specific event involving Dickens's parents might not be in question, much is unknown and probably forever unknowable about it, such as the exact moment of the author's conception, the ambient temperature at that moment, and so on. Collingwood (1928/1993: 484) makes roughly the same point in discussing historical scholars' tendency "to think that we know 'all about' something . . .

[,] possess a complete knowledge of it, when we know all *that is known* about it" (original emphasis). Collingwood goes on to conclude that, "[o]nce this confusion is cleared up, no historian would hesitate to say that, even in the period that he [or she] knows best, there are infinities of things he [or she] does not know for every one that he [or she] does." Collingwood is extensively cited by Lloyd (1998), whose insightful views we commend to the reader.

105 Thus, Sanskrit *nāma* and Latin *nōmen* agree on the length of the first syllable; Greek *onoma*, though, besides adding the problem of its initial *o-* (possibly from a laryngeal consonant), has a short vowel corresponding to the long vowels of Sanskrit and Latin, and bringing in forms for this word from other languages only muddies the waters further as regards the precise shape of the PIE etymon. But no one (it seems) would doubt that the evidence points to there being some PIE form for this word. We can thus contrast this case with the situation which – following Bloomfield (1946) – Hockett (1958: 524–5) describes for Proto-Central-Algonquian (PCA), where the relevant languages "show apparently cognate words for 'gun' and 'whisky'," but, since these are European "contributions" (so to speak) to the North American cultural scene, and since "Proto-Central-Algonquian . . . antedated the arrival of the Europeans," there can have been no word for 'gun' or 'whisky' in PCA.

106 It was Schleicher himself who initiated the systematic (though not the absolute) use of starred forms; cf. Koerner (1975, 1978a: xviii). That is, Schleicher was not the earliest asterisker among historical linguists, but he was the first consistent one.

107 We do not really know how Watkins (or anyone other than ourselves) would "vote," so to speak, in this case; however, our suggestion that Watkins might assign a zero to the reconstruction *patis is based on the assumption that the approach at issue here might tempt scholars to treat particular reconstructions in an all-or-nothing fashion, as it were – that is, by assigning zero (0 percent) to any reconstructed form that is unviable in some way (as, e.g., with the vocalism of *patis). More generally, though, it is not clear in every instance how such calculations of relative (un)certainty should be made and expressed. Still, the point remains valid that *some* index of (un)certainty would much more accurately reflect the comparative reality of any given reconstruction than asterisks now do. It is thus heartening that probabilistic approaches to reconstruction have recently been gaining greater application in historical linguistics and can now be found in such works as, for example, Renfrew et al. (2000).

108 Trask (1996: 208), for example, goes far beyond stating that "the existence of systematic correspondences" allows us to make "at least educated guesses about the sounds that must have been present in particular words in . . . proto-languages." Rather, Trask exuberantly suggests, "we can often . . . work out" (and here he surely means more than "speculate about") all of the following for a purely reconstructed language: (i) "*all* the ancestral sounds in individual words" (original emphasis), (ii) "roughly what whole words *must* have sounded like . . . , and (iii) "what the entire phonological system . . . *must* have been like" (emphasis here twice added to *must*). Trask also earlier (p. 202) speaks of the "methods which linguists have developed in order to . . . recover the histories of individual languages and language families." On the other hand, Trask (1996: 216–24) deserves considerable credit for devoting a lengthy section to the "[p]itfalls and limitations" of comparative reconstruction – a section whose warnings outnumber by far the few brief caveats provided by most historical linguistics textbook writers.

109 Some authors, however, use the time-as-measurement approach as a practical expedient in introductory discussions, and so do not even shy away from the attendant circularities. See, for example, Greene (1999: 37): "It is difficult to give an abstract definition of time – attempts to do so often wind up invoking the word 'time' itself, or else go through linguistic contortions simply to avoid doing so . . . [. B]ut we can take a pragmatic viewpoint and define time to be that which is measured by clocks . . . , device[s] that undergo . . . perfectly regular cycles of motion." However, Greene later adds: "Of course, the meaning of 'perfectly regular cycles of motion' implicitly involves a notion of time, since 'regular' refers to equal time durations elapsing for each cycle."

110 It should be mentioned, though, that British physicist Julian B. Barbour's views of time lie precisely in this direction, with all times existing simultaneously – but, as it were, in different places.

The fullest explication of his ideas, accompanied by some discussion of earlier scholars' arguments for and against, is given in Barbour (2000), but a quite brief though very general overview (simultaneously more focused and less technical) is available in Folger (2000), an interview in which most of the statements are by Barbour. In the latter, he describes his attempt to unify quantum mechanics with general relativity (a submicroscopic scale with a cosmic one) as yielding a theory where "[e]ach instant we live . . . is, in essence, immortal" (p. 58); Barbour calls each such still-life-like configuration a "Now." Rather than analyzing time as omnipresent, however, Barbour concludes that "there is no time": "the Nows are not on one timeline . . . , [but] just there," and, since "[n]othing really moves," "there is nothing corresponding to motion" (p. 60).

111 Quite apart from the question of their (in)validity, we should mention (for completeness' sake and because this section tends to provoke questions about them) that séances likewise fail to qualify even as potential sources of support for particular linguistic reconstructions, because it cannot be ruled out that the speech of groups and especially communities of spirits would continue to reflect changes vis-à-vis their earlier use of language.

112 Even with this substantial list and with those that appear later in the present section, it is obvious that we can here present only a small fraction of the huge literature – pro as well as con, scientific as well as philosophical, scholarly as well as fictional, and serious as well as fanciful – which has so far

accumulated on the subject of time travel. Hence we cannot pretend to do more here than diffidently follow our own leanings as to how many and which works to describe as representative, and which approaches to present in a more or a less favorable light. We trust that these in part externally and in part self-imposed limitations will meet with the reader's understanding, especially given our strong skeptical conviction that, both for the present and for the immediate future, it is practical considerations (such as the extreme difficulties which currently face all attempts to achieve and survive travel at the speed of light) that will prevent any time travel related to the study of (or, heaven forfend, the manipulation of) language variation and change. We are also well aware that there must be, within linguistics, many diachronicians as well as synchronicians who see time travel as inherently impossible – especially "backwards" travel into even the recent past – due to, for example, the entropy-related consequences of the so-called Second Law of Thermodynamics (tacitly invoked with our mention above of Boltzmann 1872, 1898), behind which there is always, as it were, a certain temptation to hide. We, too, return to at least indirectly entropy-related considerations, once we have finished briefly assessing what, if any, the practical implications of CTCs (= time-related curves; cf below) are for historical linguistics.

113 Of course, one might want to redefine "100%" in this context to mean "as certain as one could be," a realistic step to be sure but not the same as absolute certainty.

114 As for unearthly possibilities, we have heard it said that, for an Indo-Europeanist, heaven would involve having a speaker of PIE within earshot all the time – one who is talkative and speaks clearly – while hell would also involve having a speaker of PIE around all the time, except that this time it would be a taciturn mumbler with a perverse delight in talking just out of earshot.

115 On this topic, cf. both Wolfram and Christian (1976) and the discussion in Crystal (1995: 315).

116 Of course, diachronicians – of language or otherwise – sometimes get lucky (to be frank about it), as in the famous case of de Saussure's (1879) bold hypothesis (when he was barely out of his teens) positing for PIE a set of effect-laden but essentially abstract placeholders (accordingly called by him *coéfficients sonantiques* "sonantic [= sound] coefficients") which have come to be discussed under the rubric of "laryngeals" (for a number of general references, cf. n. 5). That is, de Saussure's conjectures and the reconstructed entities on which they rested were confirmed nearly fifty years later (unfortunately, after the great Swiss linguist's death) through the discovery and interpretation of certain consonants in Hittite, especially after the deciphering achievements of the Czech linguist Bedřich Hrozný (1917, 1919) came to the responsive attention of Kuryłowicz (1927). Discussions of this particularly striking and even dramatic affirmation of how great the value of internal reconstruction can be are available in most standard textbooks on historical linguistics; see, for example, Arlotto (1972), Anttila (1972), Hock (1991b), and Trask (1996: 256–60), among many others. It must be noted, however, that even successfully establishing the correctness of certain aspects of an internally arrived-at reconstruction virtually always leaves unknown many finer details (as we emphasize more strongly above, in the next paragraph of the main text).

117 Our own preference, however, is to characterize this approach as involving polysynchronic – rather than diachronic or "historical" – explanation; cf. the discussion above in section 1.2.3.2 (more precisely, see n. 68) and especially section 1.2.3.8.

118 We say "reconstructed proto-languages" in order to exclude situations like the occasionally encountered practice of referring to Latin as (equivalent to) "Proto-Romance," which would make the latter an attested proto-language. But, in any case, it is well known that the ("Vulgar"/Popular) Latin vernacular(s) from which the Romance languages arose are only very sparsely attested, and that the overwhelmingly more richly attested variety of Classical Latin does not represent the language state from which most Romance linguistic phenomena are descended. We should also exclude instances of what can be called "intermediate proto-languages," like reconstructions of Proto-Germanic which draw both on evidence relating to Common Germanic and on comparative evidence from elsewhere in Indo-European, since examples of this sort do seem to have a(n Indo-European) past, although not exactly an intermediate one. Hence we are here mainly focusing on ultimate proto-languages, like PIE itself.

119 The strength of the common belief that certain old-looking objects actually belong to the past rather than to the present is backhandedly proven by the vehemence with which present-day people are often tempted to deny the authenticity of historical relics that do not accord with their intuitive notions of what objects were like in the past. For instance, mock-ups which freshen up the remaining traces of paint applied in ancient times to the reliefs on the Parthenon in Athens or to carved rune stones in Scandinavia strike most modern viewers as so gaudy (even if eye-catchingly vivid) that they are automatically assumed to be completely modern inventions – since historically sensitive people "know," after all, that the dignified ancient Greeks and Scandinavians would never have daubed childishly bright colors on pristine stones. Lowenthal (1985) devotes considerable attention to this point; his book is in fact entitled *The Past Is a Foreign Country* (after a line from a play) as an expression of how we tend to assume that what is associated with a "foreign" time must also have a foreign look different from everything that we are used to in our everyday experience. Still, Lowenthal observes (p. 145): "For valued antiquities to look new is standard practice in the United States. . . . Shabbiness seldom brings history to life; the only way the past can seem real is if its relics are in their prime." Thus, he points out, the restored and replica buildings in Colonial Williamsburg are, according to Boorstin (1960: 93–4), "as neat and as well painted as the houses in a new suburb . . . [and] will never have the shabbiness that

many of them must have shown in the colonial era."

120 Santayana's dictum is often "quoted" (i.e., misquoted) as "Those who *refuse to learn from* the past are *doomed* to repeat it" (where italics mark the garbled parts); it has also been parodied by college students as "Those who cannot remember (the lectures from Intro[duction] to) History are doomed to repeat it." Such levity is perhaps not inappropriate for a quote which is so predominantly – and so frequently – taken out of context: that is, Santayana's point was not that history is cyclic, but that knowledge and skills cannot accumulate without a recollected history of memories and traditions. Thus, his preceding clause is (the very ethnocentric): "[W]hen experience is not retained, as among savages, infancy is perpetual" (p. 284). Still, what Santayana is usually (mis)interpreted as having meant was in fact explicitly stated in 1982 by the late Georges Duby, French historian of the Middle Ages, in the course of an interview with journalist André Burguière that was first published in the Paris weekly newspaper *Le Nouvel Observateur* and soon reprinted, in translation, by *World Press Review*: "Knowledge of history is a prerequisite to understanding the present. I concentrate on understanding the 10th to the 13th centuries because, within that period, the information seems rich enough to explore social relations comprehensively. I am convinced that what happened then wrought the mold for our ways of thinking, our behavior, our world view." On the other hand, the earliest major statement along these lines seems to have been made by Niccolò Machiavelli, writing in the early sixteenth century, who boldly asserted (from the edition by Walker et al. 1970: 517): "[H]e who would foresee what has to be . . . should reflect on what has been, for everything that happens in the world at any time has a genuine resemblance to what happened in ancient times." Still, consider the critical reaction to this by Crick (1970: 50): "[That ']human events ever resemble those of preceding times . . . ['] is common sense, if one allows 'resemble' to mean what is ordinarily meant by 'resemble' . . . [. But,] if one chooses to think that . . . [Machiavelli] meant by 'resemble' something like 'are ever determined by', then this is wrong . . . [–] and it is not his view . . . [,] either. . . . Choices can always be made, though they may not be the right ones."

121 Some omissions are due to practical space limitations which constrain the physical size of the volume. For instance, just as Spencer and Zwicky (1998) – in the same series as the present volume – provide sketches of various morphologically intriguing languages, our original plan was to include sketches of the main contributions to historical linguistics made by specialists in particular language families or linguistic areas: for example, the fact that, early on, research into the histories of Native American languages by Bloomfield, Sapir, and others convincingly demonstrated the possibility of doing historical linguistic research on non-literary languages. As Bloomfield (1925: 130n.1) put it: "I hope . . . to dispose of the notion that the usual processes of linguistic change are suspended on the American continent (. . . [cf.] Meillet and Cohen . . . 1924 . . . [:] 9). If there exists anywhere a language in which these processes do not occur (sound change independent of meaning, analogic change, etc.), then they will not explain the history of Indo-European or of any other language. A principle such as the regularity of phonetic change is not part of the specific tradition handed on to each new speaker of a given language, but is either a universal trait of human speech or nothing at all, an error." Here Bloomfield's views echo Sapir's famous dictum (1921: 219) that, "[w]hen it comes to linguistic form, Plato walks with the Macedonian swineherd, Confucius with the head-hunting savage of Assam." Alas, our going through with this plan would have entailed a much longer volume than would have been feasible. Also, some omissions are due to our having been incapable of finding specialists in certain areas willing or able to finish writing a particular chapter within the allotted editorial time-frame.

122 Joseph (2001a) – plus earlier presentations by him cited in Newmeyer (1998: ch. 5) – discusses the epiphenomenality of lexical diffusion and draws parallels between it and grammaticalization as similarly epiphenomenal phenomenon; on the latter, see also Janda (2001a) and Campbell (2001b).

123 But see Kiparsky (1976), Wescott (1976), and Kay (1976) for some discussion – relatively brief and somewhat inconclusive – of possible contributions from the field of historical linguistics to the resolution of this question.

124 It is not necessarily the case that this is possible. That is, since languages can differ in terms of the various conventionalized inferences that speakers draw from utterances, there can presumably also be corresponding diachronic variation in such inferences. But if, alternatively, pragmatic inferencing turns out to be just a part of some larger logical-inferencing ability possessed by humans in general, then such a system would perhaps not readily undergo or reflect change.

125 Despite our present characterization of the field as showing lacunae, we do not intend to downplay the start that a number of scholars have already made on studying various sorts of changes in language use. We would therefore draw the reader's attention both to the recent announcement (in late 1999) of a new *Journal of Historical Pragmatics* and to the somewhat earlier introductory essay in Jucker (1995) by Jacobs and Jucker (1995), which discusses what historical pragmatics in general might entail and what kind of work has so far been done in this area; see, as well, the other papers in that book (plus now also Arnovick 1999; Jucker et al. 1999). Still, many of the articles in the volume at issue are actually synchronic studies of the pragmatics of earlier language states (thus dealing with "old-time synchrony"; see section 1.2.3.10) and so do not really address changes in pragmatics per se. There is also a somewhat older literature on the pragmatic issue of alterations of address systems: see, for example, Brown and Gilman (1960) on the politeness-marking use (with potentially singular reference) of originally plural pronouns in European languages, or Friedrich

(1972) and Scotton and Zhu (1983) on the varying vicissitudes faced by terms meaning "comrade" in, respectively, Russian and Chinese. It is worth noting, though, that some seemingly pragmatic changes do not necessarily represent a qualitatively unique kind of development, but instead appear to be in some sense entirely unexceptional. Thus, for instance, changes in the nature or use of honorifics and other terms of address normally correlate with changes in social customs. For example, many speakers of American English now sometimes employ first names even in encounters with total strangers, as when telemarketing solicitors begin a call by using a first name to address someone with whom they are not on a so-called "first-name basis"! And, at least to some extent, changes in honorification behavior may represent just one type of lexical change.

126 Given (i) the major role played in many languages by intonation as a way to distinguish dislocation constructions (like (*As for*) *The neighbors, they left*) from resumptive-pronoun or even apparent agreement-marking constructions (like *The neighbors they left* or *The neighbors they-left*) and (ii) the fact that specific intonational curves tend to go unrecorded by writing systems, we speculate that such unwritten changes in intonation are at once criterial and yet invisible determinants for the chronology of reanalyses by which dislocation structures yield to agreement-marking ones. For example, a change like this has been discussed as characterizing certain varieties of Colloquial French; see Auger (1994), who focuses on Québécois but also provides general references. In fact, given our hunch that documentarily invisible intonational shifts like this are frequently and complicitly involved in the demise of particular dislocation constructions, we are tempted to speak of "intonation(al change) – the silent killer," since it involves a serious sort of change in grammatical blood pressure, so to speak (though perhaps in the direction of hypo- rather than hypertension). Occasionally, though, there exist rare exceptions to the generalization that intonation and related phenomena (like phonological phrasing) tend not to be indicated in written texts. Thus, for example, Fliegelman (1993) discusses the way in which a typographical gaffe by Philadelphia printers carried over into "broadside" copies of the US Declaration of Independence (1776) a reflex of Thomas Jefferson's private markings as to where he should pause for rhetorical effect if called upon to read the document aloud (since he knew of his reputation as a poor speaker).

127 The Balto-Slavic branch of Indo-European has proven to be an especially rich source for studies of historical accentology and prosody. See Collinge (1985: 271–9) for a summary of several major "laws" pertaining to this area, as well as such recent works as Bethin (1998) and Alexander (1993). On accentual systems in contact, see Salmons (1992) and the many references there.

128 The asymmetry at issue can best be illustrated with reference to tonogenesis – beginning with the fact that this term itself is still unfamiliar enough as a label that

we have overheard linguists exclaim, when they first encounter it in written form: "Look at this obviously metathesized misspelling of *ontogenesis*!" (we are not making this up). The same relative lack of attestations extends to the general referent of *tonogenesis*, as well. At one point in the writing of this essay, for example, we recalled that the 1970s and 1980s had seen a great upsurge of (especially phonetic) research surveying and comparing the origins of tone(s) in various languages; assuming that this trend must have continued up to the present, though beyond our immediate awareness, we considered offering an apology for this volume's lack of a specific chapter on historical tonology. But, when we looked for references to offer in lieu of such a chapter, we found that, in recent years, there has been no book- or even article-length study presenting a *general*, consensus-based overview of the various ways in which tones seem to arise, split, merge, shift (in quality), move (laterally within a word), and the like in the world's languages. Hence it is representative of the current literature on the topic that the chapters here by HALE (7), KIPARSKY (6), JANDA (9), and OHALA (22) only very briefly mention tonogenesis – the last of these, for example, focusing mainly on the relatively early results of Hombert et al. (1979) and on the revisions of its claims required by the later findings of Löfqvist et al. (1989) and of Ohala (1993a: 239–40, 269n.2), among others. In fact, one of the fullest treatments of tonogenesis remains that of Hock (1986: 97–106, 664) (with some references). It may also be noted, for example, that there is no entry for *tonogenesis* or any equivalent in Bright (1992) and Asher and Simpson (1994); rather, tonal origins are there discussed only in passing – see the respective indexes – and then mainly in connection not with phonology but with phonetics and particular linguistic groupings. Thus, diachronic tonological studies specific to one language or language family continue to appear not infrequently, but the dearth of recent comprehensive works on tonogenesis likewise continues, thereby sounding a low note within the general field of tonology. If any reader with expertise in tonological change is inspired by this non-optimal situation to write a survey article – or, preferably, a book – on tonogenesis, it will surely be met with a high-pitched cry of delight by all historical linguists.

129 See, for example, Swadesh (1950), Gudschinsky (1956), Hymes (1960), Dyen (1973), or Embleton (1986, 1991) for discussion and applications of this methodology. But, like Anttila (1989: 396–8), we here distinguish between glottochronology as a specific notion versus the much more general concept of lexicostatistics.

130 For example, when there is nothing else to go on, glottochronology might make available for further investigations a rough estimate of the time depth (i.e., centuries of separation) between two related language varieties. However, such a last resort would always have to be viewed as the weakest and least reliable source of information available, and so would come into question only under truly desperate circumstances.

131 The *locus classicus* disputing the foundations of glottochronology is Bergsland and Vogt (1962); see also the recent negative assessment in Dixon (1997).

132 See Benveniste (1969) for an insightful sifting of the linguistic evidence concerning early Indo-European society, all very ably summarized in Mallory and Adams (1997), where can be found (on pp. 290–9) a discussion of the Indo-European homeland issue. On the latter, see also such relatively recent works as Renfrew (1987) – to be read along with the important review by Jasanoff (1988), in which the linguistic side of the claims is addressed – and Mallory (1989). The many books and papers by the late Marija Gimbutas (e.g., Gimbutas 1970, 1985, among others) deserve mention here, too, as does Gamkrelidze and Ivanov (1984). Similarly, there is a long tradition within Indo-European linguistics of the study of early Indo-European poetics, summed up (and furthered) most recently by the masterful work by Calvert Watkins, especially Watkins (1995).

133 Two additional questions deserve fuller discussion but are only tangentially addressed in the chapters of this volume. First, given what is now known about individual differences in certain aspects of language acquisition (cf., e.g., Bates et al. 1995 and the relevant parts of Fillmore et al. 1979), is it really legitimate to talk about "the" language-learning child, as is especially common in generative syntax? We would argue that anyone discussing "the child's" behavior in language acquisition and change must first answer the question: which child? Nor is this just idle stone-throwing on our part, either; rather, what we have targeted here is arguably common practice – note, for instance, the title of Landau and Gleitman's 1985 book *Language and Experience: Evidence from the Blind Child* (emphasis added). But also, second: "when" is a child? That is, in light of the considerable evidence suggesting that substantially different linguistic behavior can be shown by the same individual at different ages between birth and age 18 (cf., e.g., Vihman 1996 on the concentration of consonant-harmony processes among younger children), is it not crucial to distinguish between and among some maturational equivalents of popular-culture divisions like infants, toddlers, kindergartners, elementary school students, and adolescents? We are hopeful that these matters will come much more saliently to the fore in subsequent collaborations between developmental psycholinguists and historical linguists. As Kerswill (1996: 178) notes, "People of all ages can (and do) modify and restructure their language – though exactly *what* they can change is to some extent age-related"; for a brief, older presentation of an actual case study involving documented change in an adult's language, see Robson (1975) (cf. also, more recently, Seliger and Vago 1991 on first-language attrition under conditions of contact and language shift).

134 It must be recognized, of course, that there may well be no such thing as a totally theory-neutral account, since decisions about categories and labels force one into a theoretical stance, even if only a weak one.

135 Works applying the tenets of Optimality Theory (OT) to language change are obviously a relatively recent phenomenon (since OT itself first came into prominence starting in 1993), but they already constitute a not inconsiderable literature (regarding which we thank Randall Gess for references to several articles in addition to his own). Cf., for example – among many others – Anttila and Cho (1998), Cho (2001), Gess (1996, 1999), Holt (1996, 1997), Kirchner (1998), McMahon (2000a, 2000b), Nagy (1996), Nagy and Reynolds (1997), Reynolds (1994), Zubritskaya (1995, 1997), and most of the papers in Hinskens et al. (1997), though see also the critiques in Guy (1997a) and subsequent works. Our own view is that, to date, applications of OT to historical linguistics have tended to demonstrate only that one *can* model diachronic correspondences in a constraint-based approach; they have not yet shown that OT allows many novel insights into language change which were not previously available, nor do they suggest that this new theory brings us appreciably closer to understanding why languages change. In a nutshell, "progress" is not a word that comes to mind when advocates of a theory which employs essentially only constraints and constraint rankings hail as a breakthrough the putative discovery that all language change consists in constraint rerankings. As the saying goes: it comes as no surprise that, to someone whose only tool is a hammer, everything looks like a nail. Still, we remain hopeful that this new century will be marked by OT-based diachronic linguistic studies which are less descriptive and more explanatory, especially as they begin to incorporate constraints referring more directly to psycho- and sociolinguistic considerations. For a rudimentary start in the latter direction, see Janda (1998a: 348–9), who advocates positing a family of EMULATE constraints in order to account for borrowing in dialect- and language-contact situations.

136 Hopper (1987: 148) expressed such matters as follows: "There is . . . no 'grammar' but only 'grammaticization' – movements toward structure."

137 In all honesty, we must note what Lass says about our position in Joseph and Janda (1988): "It is so beautifully explicit, and so wrong-headed, that it deserves quotation" (Lass 1997: 10). Needless to say, given our disagreement with Lass's rather strongly articulated – even extreme – and, for us, similarly wrong-headed views (e.g., on a pseudo-organicist approach to the nature of language; see section 1.1.2 above), we see this book as a whole – and especially this introductory essay – as an answer to his claims.

138 As for the alleged dichotomy in linguistics between synchrony and diachrony, Koerner (1974: v) points out that, "[a]s the result of a misunderstanding of Saussure's true intentions (. . . largely misrepresented by the editors of the *Cours [de linguistique générale* (1916)]), the idea . . . gained widespread currency . . . that synchronic linguistics . . . could . . . be dealt with quite separately from diachronic linguistics . . . [and] that the latter was little more than an accessory to the former which could easily be dispensed with." But "[c]omparison between the

Cours . . . as edited by Charles Bally and Albert Sèchehaye and the critical edition prepared by Rudolf Engler[(1967–8, 1974)] reveals that . . . [,] each time the 'vulgata' text speaks of an incommensurability between the synchronic and the diachronic viewpoint[s,] . . . Saussure had merely spoken of a (methodologically important) difference between the two in his Geneva lectures" (Koerner 1974: v.n.*). Nevertheless. "Bloomfield's *Language* of 1933 followed the model provided by the *Cours[. . .]* in separating these two 'points de vue', even to the extent that the historical portion of his book contains no[t a] . . . single cross-reference to anything mentioned in the preceding descriptive section, indeed as if there were two sciences of language entirely divorced from each other and as if one such field could operate satisfactorily without reference to the other" (p. v).

139 See also below (in the main text) regarding Japanese *rendaku*, as well as n. 140.

140 Other cases of this sort are readily available. For instance, it is well known that prescriptive grammarians can shape language use and hence linguistic form. This occurred in English with regard to, for example, the elimination of double negation among speakers of what is now the standard language. Something similar seems to have happened in German with the use of *ge-* versus Ø- in the formation of past participles: an experimental study by Wolff (1981) suggests that the prescriptive rule (requiring *ge-* before verbs having an accented initial syllable, but Ø- otherwise) is employed with greater consistency by speakers with more formal education than by those with less. Finally, in what is perhaps the most dramatic such case, since it hinges on the efforts of a single individual, Ehala (1998) has shown that the declining use of verb-final word order in Estonian subordinate clauses during the first third of the twentieth century, among speakers of all ages, can be traced to the influence of Johannes Aavik, a leading grammarian of the day who championed a "native Estonian" grammatical movement – with verb-final order being considered "an embarrassing German influence," as Ehala puts it (p. 77). Among other things, Ehala notes that this development seems to show parameter settings being changed in adulthood, an issue bearing directly on the claim that children are the primary instigators of change (especially if one adopts the views of Lightfoot 1991, according to whom change is a matter of resetting parameters; cf. here also LIGHTFOOT's chapter 14) – but, for a different general view, see AITCHISON's chapter 25, and the brief discussion in section 2.2 above, plus n. 133.

141 The reader must be the ultimate judge, but we believe the strategy of including a plurality of views on individual topics in this volume has given it not only a fullness but also a liveliness of voice. No attempt has been made to tone down what any of the authors have written – including the editors, who are themselves die-hard opponents of the school exemplified by the British diplomatic historian Sir Adolphus W. Ward, co-editor of the "good, gray, . . . excruciating" tomes (cf. Fischer 1970: 296) of the *Cambridge History of British Foreign Policy* in the 1920s. Sir Adolphus once complained (as reported by Roberts 1966: 112–13): "I've had a bit of trouble with Algernon Cecil's chapter . . . [; i]t's a bit lively."

142 For instance, besides revised and updated printings of earlier introductions (e.g., a third edition in 2001 of Aitchison 1981), several new introductory textbooks on historical linguistics have appeared in recent years, such as Hock and Joseph (1996), Trask (1996), Campbell (1999), and Sihler (2000), along with some specialized studies, like Nichols (1992a), Labov (1994, 2001), and Harris and Campbell (1995) – each of the latter being (encouragingly) the recipient of one or more book-prizes. Various other books aim at a more general audience of linguists but still have significant diachronic content, such as Dixon (1997), Newmeyer (1998), and Lightfoot (1999a). There have even been some general handbook-like surveys (although not as comprehensive as the present volume), like Jones (1993) and Polomé (1990), among others. For a listing of numerous earlier introductions to historical linguistics (including many works in languages other than English), along with some very brief discussion, see Janda (2001: §3) and references there.

143 Earlier book-length starts in this direction have been made in the more versus less distant past by, respectively, Barber (1964) and Bauer (1994). A list of article-length works pursuing roughly the same goals (and dealing with at least one other language besides English) is provided by Janda (2001; cf. especially §8). For discussion of a broadly similar (though by no means identical) trend in anthropology, see the papers in Fox (1991). And, in the field of history itself, Fischer (1989: ix) has provided one of the most eloquent statements of a position which we interpret as essentially identical to that espoused here: "In its temporal aspect, this inquiry seeks a new answer to an old problem about the relationship between the past and the present. Many working historians think of the past as fundamentally separate from the present – the antiquarian solution. Others study the past as the prologue to the present – the presentist solution. This work is organized around a third idea – that every period of the past, when understood in its own terms, is immediate to the present. Th[e] . . . 'immediatist' solution . . . in this volume is to explore the immediacy of the earliest period of American history without presentism, and at the same time to understand the cultures of early America in their own terms without antiquarianism." For more detailed discussion of presentism and antiquarianism – but primarily as fallacies, not "solutions" – see Fischer (1970: 135–42), who discusses numerous other fallacies, as well.

144 Foch's original (telegraphic) French words are discussed in Liddell Hart (1928: 162–3, 1932: 108); as that author concludes (1928: 162), regarding Foch's report: "If not true in fact, it was true in spirit."

145 Even if this statement strikes some as straddling the boundary between proselytizing and preaching, we at least have consistently tried to practice what we preach. As examples of works referring to both past changes and changes in progress, see Janda (1989, 1998a, 2001a) and Joseph (1981, 1992,

2001b); as examples of collaborative works on these and related topics, see (among others) Joseph and Schourup (1982–3), Janda and Varela-García (1991), Janda et al. (1994) and Joseph and Janda (1988), as well as (besides this introduction) the dedication for the entire present volume (within the preface which precedes this essay).

146 Taking Labov (1974/1978) as their reference point, at least two subsequent papers have started to ring the changes on his title "The use of the present to explain the past": thus, Hogg (1997) suggests "Using the future to predict the past" (e.g., by filling in earlier, unattested Old English structures on the basis of later, attested Middle English ones), while McMahon (1994b) proposes "The use of the past to explain the present." Cf. also the at least partly parallel titles of three purely historical or archeological (i.e., non-linguistic) works: Trigger's (1973) "The future of archaeology is the past", Koselleck's (1979) *Vergangene Zukunft/Futures Passed*, and Blackham's (1996) *The Future of Our Past*.

147 We intend "thick description" in its more literal sense ("richly textured"), as well as in the more contextualized and cognitivist sense adopted by Geertz (1973) from Ryle (1968a, 1968b).

148 For such an approach, an extra-linguistic model – worthy of emulation in all respects (not least as a warning as to the potential for external interference) – already exists in the work of the evolutionary biologist Henry Edward Crampton (1917, 1925, 1932), who "spent fifty years documenting the *current* geographic distribution and variation of [the land-snail genus] *Partula* on Tahiti, Moorea [(the inspiration for Rodgers and Hammerstein's "Bali Hai")], and nearby islands," in order to record, not just "a frozen snapshot, but . . . [a] moment in the future history of [the several species of] *Partula*"; cf. Gould's (1993) forebodingly titled "Unenchanted evening" (pp. 33–4; original emphasis). All told, Crampton personally measured more than 200,000 snails (with at least four length measurements just on each shell) and hand calculated all the statistics (in some cases, to eight decimal places), thereby ensuring that the "personal coefficient" was uniform throughout his research (Gould 1993: 32). "Crampton devoted this lifetime of effort . . . to *establish[ing] a baseline for future work* . . . [:] *Partula* would continue to evolve rapidly, and . . . [this] baseline would become a waystation of inestimable value . . . [, since f]uture changes have much more value than current impressions" (Gould 1993: 34; original emphasis). And, indeed, Murray and Clarke 1980 (respectively an American and a Britisher, working in collaboration with the Australian Michael Johnson) were later able to build on Crampton's start at making *Partula* into effectively a museum and a laboratory of speciation. Here is truly a lesson and an example to inspire all those who study innovation and change – linguists in particular! Alas, the end of this story provides an additional lesson: *Partula* has completely disappeared from Moorea, and almost completely from Tahiti, because the "killer" snail *Euglandina* from Florida – introduced on these islands by local authorities in an attempt to eliminate an adventitious snail – has instead devoured *Partula*, presumably ending forever its evolution there (cf. Gould 1993: 35–9). "Crampton's work is now undone," but "[w]hat is more noble than . . . intellectual dedication . . . [to] a lifetime of persever[ing] . . . through . . . field biology['s] . . . occasional danger and prolonged tedium" (Gould 1993: 40) in order to "establish . . . a starting point, with utmost care and precision, so that others . . . [can] move the work forward and continue to learn about evolution by tracing . . . future history"? By replicating (as closely as possible) the model provided by a scholar like Crampton (1875–1956), even linguists will be able not only to honor his memory but also to help turn his apparent defeat into vicarious triumph – though the spread of such a deliberate approach, and the gathering of such rich documentation, may at first seem to be advancing at a snail's pace.

149 Given the abysmal track record of attempts to predict change simultaneously on a large scale and over the long term (cf., e.g., Popper's devastating 1961 critique of Toynbee's 1935 proposed "laws" governing the "life cycle of civilizations"), what we advocate for historical linguistics is the formulation and testing of predictions regarding either (i) specific phenomena over longer periods of time or (ii) complex (or general) phenomena over shorter periods of time. We have already stuck out our own necks and made two distinct predictions of the first type (cf. Janda 1991 on the probable continuing spread of *-s* plurals on nouns in Modern High German, and Janda et al. 1994: 80 on predicted future developments involving (alveo)palatalization of English /s/ before clusters like /tr/, as in *stress* (pronounced as if *shtress* – see now Janda and Joseph 2001 for more discussion)). As regards the second prediction-type, one goal for linguists to aspire to is the current ability of meteorologists to make extremely accurate predictions regarding local weather for relatively short periods of time (e.g., up to five days in advance – whereby the linguistic parallel to this would more appropriately be five years or, better, five decades). In this regard, we are much more sanguine than Posner (1997: 107) – who, though "less pessimistic than . . . [Lass (1980a, 1997)] about the possibility of expla[i]n[ing] . . . linguistic change," still views "language . . . as a dynamic system . . . in the sense of an evolving ensemble where variation of a parameter produces a change of state, as in a meteorological or population system." "In such systems," Posner laments, "the number of variables is so large that accurate fine-tuned prediction is virtually impossible, although it is feasible to model the systems in such a way that some useful results can be obtained." We are likewise more hopeful than Lightfoot (1999a: 267–8); while agreeing that it is probably not productive now to attempt predictions regarding the "*distant* end results of language change" (emphasis added), we are convinced that historical linguists can succeed at more than "offer[ing] interesting explanations of changes as they take place, in the fashion of a weather forecaster . . . [,]

understand[ing] particular changes and explain[ing] them . . . as they happen."

150 To present this apparently anti-quotational quotation from Emerson without context is actually unfair to those who quote. Emerson precedes this remark with: "*Immortality* . . . [:] I notice that . . . [,] as soon as writers broach this question . . . [,] they begin to quote." This suggests that he was mainly criticizing authors who discuss the subject of immortality without having any real experience with it – and so are forced to cite other writers on the topic (who also lack the relevant experience . . .). We ourselves quote no one on the latter topic (since our lack of related background makes us subject to Emerson's dictum), but we have considerable experience in quoting, and so feel entitled to cite Emerson's opinion on the matter.

151 On the history of both quotation-sourcing and reference-free footnotes, especially in historiography proper, see Grafton (1997). Although the series in which the present handbook appears uniformly employs endnotes, rather than literal footnotes, the *style* of quotation is the usual linguistic one in which notes never contain only references, but always some content. The wisdom of the latter practice is shown by Hume's (1776, quoted from 1932: 313) reaction to the purely referential endnotes in the first volume of Gibbon's (1776) *History of the Decline and Fall of the Roman Empire*: "One is . . . plagued with . . . his Notes . . . [in] the present Method of printing. . . . When a note is announced, you turn to the End of the Volume . . . [,] and there you find nothing but the Reference to an Authority. . . . All these authorities ought only to be printed at the Margin or Bottom of the Page" (what Hume recommended is also the style of Grafton 1997).

Part II
Methods for Studying Language Change