

Lass, Roger. 1997. *Historical linguistics and language change*. Cambridge Studies in Linguistics 81. Cambridge: Cambridge University Press. (xxiii + 423). Paperback.

This is a major work on a major subject -and a major work in many senses. The first and more immediate, because it is “a kind of retrospective on nearly three decades of both being a historical linguist and worrying about the epistemic pretensions of what historical linguists do” (xiii). And if the author of such retrospective is Roger Lass, then, there are reasons to expect that his account of “how a specialist practitioner sees what he does” (xiii) will contain major insights, reflection and questioning on the subject. The work is, therefore, of theoretical nature and it came out at the right moment in the development of the discipline. After many years of expansion, growth, incursions in new fields (sociolinguistics or pragmatics, f.i.) and, above all, the arrival of the new technologies which have brought about unsuspected tools and possibilities for research, the need to stop and think again on the “big questions” regarding theory was being felt among the historical linguists. In this respect, Lass's work heralded in the conclusions of the 10th Conference on English Historical Linguistics held in Manchester almost a year after its publication. Most of the big names of the trade were there -Lass included, of course- and his lecture, just as most of the plenary lectures and sessions imparted by them clearly showed the shift towards theoretical thought in present and future work. *Historical linguistics and language change*, avidly read when it was first published, deserved revisiting and thinking it over in the light of this trend -particularly, when it is already present in the bibliographical references of most works on English historical linguistics recently published.

The book is not intended to be exhaustive, the author says, but deals with just a selection of issues. The list of the seven chapters covering those issues will show that hardly anything “important, interesting and tractable” (xiv) in historical linguistics has been left out: *The past, the present and the historian*; *Written records: evidence and argument*; *Relatedness, ancestry and comparison*; *Convergence and contact*; *The nature of reconstruction*;

Time and change: the shape(s) of history; Explanation and ontology. They are preceded by a Preface (far longer than the General Prologue is) where his position and main views are already stated, so that nobody can feel deceived afterwards. These are basically three:

- historical linguistics belongs “to a general science of historically evolved systems, whose principles are more or less the same, regardless of the kind of system evolved” (xvii)
- humans are not primarily “language builders” but end users (xviii)
- languages are populations of variants moving through time (xviii)

Here is a second reason why I have used the adjective “major” for HLALC - and thus attention will be paid in this review to each chapter individually, as many important subjects of our trade as historical linguists are contained in them. The first chapter goes in depth into the view of researchers of any kind (biologists, linguists, historians, philosophers) as mythmakers -i.e. “they use metaphors to structure their particular epistemic field in their cognitive or explanative aim” (6). Departing from the often forgotten truism that “all human knowledge is flawed, provisional & corrigible” (29) Lass discusses the nature of our object of study (language in the traditional sense) and the nature of our work. Interpretation happens in all cases, he says, and from his constructivist approach to cognition, denies (quite rightly in my view) the possibility of a “completely neutral account of facts” - even when studying “direct witnesses.”

The author proceeds immediately to support his thesis in his evaluation of the sources for historical linguistics in the second chapter *Written records: evidence and argument*. I must warn that this is one of my favourite chapters in the book (the other is the final one). The list of issues dealt with is illustrative: the nature of writing systems, the meaning of graphs in early texts, types of evidence such as the testimony of grammarians and phoneticians, metrics and rhyme, glosses and glossaries, and the representativeness of the textual evidence preserved. And typically in Lass, his argumentation and exemplification are extremely detailed -one learns as much from the conclusions as from the analyses themselves. The whole chapter is a brilliant and well-founded lesson on good practice for philologists, where the distance between writing and speech is carefully described and assessed in every field

relevant for linguistic change: orthography, writing systems, literary materials, modern editions of early texts... Particularly appealing part 2.5.2., devoted to glosses and translations and part 2.7: “Desperate remedies: interpreting vs. disappearing”. As for the first, the argumentation behind “*stangella* is not the OE word for *pelican* at all” (85) is a careful assessment of the nature and function of reference materials. The second challenges the validity of emendation for historical linguistics: “Trying to reconstruct an individual’s word choice is as fatuous as writing texts in a proto-language or reading ancient poems aloud in a reconstructed pronunciation” (100). Both (as any other point in the chapter, really) throw light on important theoretical issues regarding the diffraction which always happens when looking back to past stages of languages and its causes.

Chapter 3, “Relatedness, ancestry and comparison” revisits the basic Neogrammarian topics from perspectives, which take this work to multidisciplinary fields and ambitious theoretically risky waters: the mathematical theory of chaos, evolutionary biology, biosystematics. Lass makes linguistic families depend on genuine historicity, where systems are characterised by “sensitive dependence on initial conditions” and by having replicators (heritable items), variation and selection processes. As such, since the ontological bases are similar, Lass uses the cladistic terminology -which provides an updated and empowered description of genealogical connections among languages. He concludes that the Neogrammarian methods are still indispensable, when employed rightly, with all the necessary caveats and in the light of new developments in linguistic theory, in order to trace good linguistic history. It could not be otherwise, as structural relations are what define language histories, in so far as they are also the defining core of languages. This argument is pursued in the following chapters: Chapter 4 *Convergence and Contact* and Chapter 5 *The nature of reconstruction*.

“Endogenous change” is discussed in the first of these two chapters. The hypothesis that “even if a language could be totally isolated from any contact whatever ... it would change in just the same way ... as any other language” (208) seems to be a logical consequence of the theoretical assumption that languages are systems subject to imperfect self-replication. There are, however, two unclear points here: one, is it really the case with natural languages -i.e. can we find a language which has remained isolated long enough to test the hypothesis? Second, why is replication imperfect?

The classic explanation in “biology for beginners” points to combinatory reasons -plus various kinds of factors, which increase the frequency of certain basic combinations. What are those factors in the case of (supposedly) isolated languages? Social interaction within a closed community, certainly -plus a host of intrinsic forces of the languages themselves.

How are those forces favoured or handicapped in the case of non-isolated societies (the vast majority)? This, however, is not to deny that certain basic in-built properties of human languages cannot be neutralised by extra-linguistic circumstances. But I think that the above-mentioned question should have been addressed to in a clearer way.

Phonological change is dissected in Chapter 5 -*The nature of reconstruction*. After all, this book is by Roger Lass, and being of the kind it is, a long chapter on historical phonology seasoned by a re-consideration of the Great Vowel Shift was to be expected. His aim is to investigate how much and what can be reconstructed -and he applies the cladistic concepts explained in Chapter 3 to the methods both of comparative and internal reconstruction.

The general conclusion is that sound change continues to be, as always, the most fertile ground for reconstructive theory, followed by morphological evolution (particularly when connected directly with phonology). Of course, if what one wants to prove is that the Neogrammarian model is still valid. At first sight, this seems either a bit of a cheat or a circular argument, but what lurks here is really the old question: why should we be able to construct phonological histories so nicely whereas things become increasingly difficult when we proceed to morphosyntax? Why, if phonology evolves far more quickly than morphology or syntax, can we recognise *family likenesses* among languages there... but it is so hard to do the same in morphosyntax? Lass quite sharply points out that in one case we are dealing with individual items, and in the other, with paradigms. I would also consider the fact that phonological change is most directly connected with physical constraints -consequently, directionalities are, in this respect, far more predictable.

Additionally, we have to take into account that the kinds of evidence we need for our reconstruction are radically different: in the first case words, at most tone groups; in the second, texts. And as far as texts are concerned, Lass's remarks on proto-languages are indirectly a warning at the blind faith

which some tend to place in corpus linguistics. Locating the key difference between proto-languages and living languages in the possibility of producing native utterances and/ or texts and not in the *completeness* of the material evidence available clearly leads to the conclusion that collections of corpora are *per se* condemned to perennial incompleteness (in this sense whether corpora are made of living or dead languages is irrelevant). In my view, the new technologies have allowed the processing of overwhelming statistical data which are illuminating in many ways (confirming or rejecting hypotheses, forewarning new possibilities or tendencies...) But bad theoretical models provide bad interpretations of the best data available and emphasis on good epistemological tools is, then rightly made in HLALC.

More particular aspects are, to me, questionable: for example, his consideration of absolute constructions, dative of possession and non-accusative verb-objects in Indo-European languages as possible morphoclines, without clarifying whether some kind of linguistic area might be involved. More anecdotally, I partly agree with Lass in that many historical novels or new texts in extinct languages is *not a serious pursuit as a form of activity in linguistics* (274). But it is amusing, and can produce very illustrative allegories - very much, by the way, like many fierce defences of the real existence of the Great Vowel Shift.

This chapter and the following one (Chapter 6: *Time and change: the shape(s) of history*) are probably the densest and most difficult of the book. This is due partly to the nature of the themes discussed and partly to Lass's propensity to what he himself acknowledged once as *pomposity* (1987: xvii). This becomes particularly acute when facing major theoretical enterprises, as it is the case here, above all, in Chapter 6. After having identified the main perceptions of language change (change as loss, flux, *creatio ex nihilo*, degeneration, progress) and having placed them in their corresponding theoretical backgrounds. The author goes back to his re-consideration of linguistic change in the wider frame of general science. He distinguishes between classical Newtonian time and non-reversible of *thermodynamic* time as a key point in the identification of the possible directionality of changes. He places linguistic evolutions in the second type, in so far as they move through cycles: *the history of any dynamical system (= any evolving ensemble where variation of a parameter setting produces a change of state* (293). Other notions include *attractors* of various kinds and *chreods* or autocatalytic devel-

opmental patterns. This model is also employed to discuss in this light linguistic novelty, and more particularly, the origin of language (another of the *big questions*) and what he calls *junk*.

The problem of the origin of human language remains, for the most part unsolved (through recent anthropological research has provided suggesting hints) -but what is clear is that the kind of languages we can observe and reconstruct are already very sophisticated entities where real structural invention is, for the most part, not possible. What seems to happen, rather, is that languages produce *junk* (noise, redundancy, exceptions...) very much like biological systems, which needn't have any function in particular, but, may, if the occasion comes, be employed to serve other roles or integrate new systems.

This is explained by adapting again a term from palaeobiology: exaptation, which denotes "the co-optation during evolution of structures originally developed for other purposes" (316). The idea is certainly compatible with the more recent views on evolution as opportunistic and very much depending on the many possibilities of the very moment (and therefore, *chaotic*, cf. Arsuaga & Martinez: 1988). This is, doubtless, a daring view: the interpretation of linguistic history in the same terms of other evolutionary sciences (biology, basically) may, perhaps, be questioned from certain theoretical positions. Personally, I am not completely convinced that such a step can be given, since I am not sure either of up to what point living organisms, species, languages and language families can be equated. But in any case, I find it extraordinarily vivid as an image and as a metaphorical representation of what we know about the evolution of languages.

It also provides, in my opinion, more powerful and parsimonious insights into the subject than any other which simply assumes therapy or progress as reasons for change. In any case, my feeling is that considerable thought must be devoted to Lass's proposal in this line -even in the case that the concept had to be rejected in the end, much will be learnt from the whole research process.

Chapter 7 (*Explanation and ontology*) ought to be better described as a cocktail in which the issues of the whole work are revisited in generalisations regarding the nature of change, the logical structure of explanation, and a *vindication of the duties of historical linguists*. Change is of course a *sublu-*

nary axiom (325) and in order to reach any satisfactory knowledge about it we need to think about the logical structure of explanation. Lass distinguishes here between explanation as a technical term in academic discourse and loosely everyday uses -and states that rigorous positive types of explanations cannot be applied to the history of cultural artefacts. The huge temporal frame for most truly structural linguistic changes, and the fact that most languages show plenty of *undesirable configurations* cast suspicion on the idea that languages evolved either prophylactically or therapeutically in directions somehow embedded in the human ability to speak (whatever this may consist of). Lass typically labels this as *romantic nonsense* while he clearly distinguishes between *over-refined sensibility* and *lack of linguistic knowledge* (341). He advocates the need to remember that a language, as a means of communication is a shared system employed for a variety of purposes. The need to separate structure and function leads him to deny the possibility of speakers to assess the directionality of variation in their output, since for them languages are *by and large a non-focal historical given* (361).

I think Lass is right for the most part in his discussion of sociolinguistic approaches to change -but I'm still under the impression that speakers may, up to a point, make choices among variants because of social patterns of behaviour (i.e. prestige) rather than any kind of *linguistic feelings*. How many of these choices lead to structural changes must probably be further investigated. In this same line of argumentation, the notion of *lethal variants* should have been explained as well, for a basic issue lies behind: how can we decide which is lethal in language -impeding communication, perhaps?

The chapter -and the work- finishes with an *Envoi* in which the basic ideas advanced in the Preface are re-stated again in the light of everything he has written so far. He insists on the role of the linguistic historians as Model Builders -models valid till more powerful explanations and better evidence are produced. He also advocates a clear distinction between what should be the primary concern of any good description of language change (system, structure) and social, pragmatic approaches which, however interesting, and illuminating, and attention deserving, are complementary. I very much suspect that this is and will be the main trend of theoretical historical linguistic thought for years to come.

HLALC deserves, for me, the adjective *major* because all that have been said so far -but there are other reasons with which I will finish this review. In the first place, because Lass's work has always been interesting and stimulating -in the sense that it has always raised not only controversy, but also further questioning about new paths for exploration. Even if you dissent from his views (as I do, quite radically, about Mozart and Tchaikovsky) his points are very often challenging: you generally end up by digging up from them suggestive ideas, new paths or keys to face problems.

His work is also peculiar as he allows to percolate more from personal experience than many would: but who said this is wrong? In any case, in this work that experience allows us to see a true scholar at work, always ready to consider argumentation regarding his work and to revise not only conclusions, but also methods and theoretical models. Good examples are the beginning of footnote 1 to Chapter 7 (*I am grateful to all the reviewers and attackers of Lass (1980) for giving me important and often distressing things to think about*); his revision of drifts and major state changes in page 301 (*this has been interpreted, (mistakenly) as a kind of orthogenesis or directed evolution -notoriously in Lass 1974, which I regret, but was not smart enough then to avoid-*); and notably in point 7.2 (*In which the author revisits an earlier self, and is not entirely satisfied by what he sees, but not entirely repentant*).

The unmistakably *Lassian* touch appears in this work at its best: the customary detailed, often intricate argumentation furnished with all sorts of long examples, and all this topped with the usual ironical, unorthodox (and generally very much to the point) remarks of the type: *The velaric ingressive airstream used in clicks is the mechanism for biologically necessary operations like sucking or cigarette smoking* (318). I find this combination of scholarship and humour very nice, and not only because of the old chestnut that one should not trust people who take themselves too seriously. I like it above all because Roger Lass has always reminded me of Archpriest Ripamilán, a highly commendable character in *La Regenta*. He is notorious, among other things, because he adored dancing rigadoons privately, but for him Clarín has one of the very rare laudatory remarks of *La Regenta*: *he knew how to deal seriously with serious matters* (my translation).

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Trinidad Guzmán
University of León

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ADDENDA: A DIALOGUE BETWEEN THE REVIEWER [TG] & ROGER LASS
[RL]

[TG] Third: additional questions and comments I did not include in my review. Of course, I'm including them here simply because I'd like you to have them; don't answer them if you don't feel like it, after all most are silly:

1.- As far as “Quanta and phonetic gradualism: a few suggestions” (5.2.2) I wonder what the role of phonetic labs and their results can be in this “fineness of grid”?

[RL] A big role. I've been reading some very interesting computational phonetic stuff from a former student of mine at Oxford, and I think this is the direction real research in phonology ought to go.

[TG] 2.- I'm not sure that “syntax is not learned, but constructed -or generated” (p. 265) as opposed to phonology and morphology. By each generation? As you may imagine, I have first-hand evidence now about language acquisition by kids (poor chaps, two linguists as parents!) and I feel they also

learn syntactic structures in a similar way they learn morphological facts -but I may be wrong.

[RL] I agree, to a large extent. Did I really say that, or was I outlining other peoples' positions?

[TG] 3.- 6.3.3. p. 297: Would you consider mass media or social climbers as “attractors” or is it, perhaps, mixing theoretical approaches (sociolinguis-tics and the like) a bit too much?

[RL] Must think about this one. 'Attractors' are more abstract than people.

[TG] 4.- Same section, following page, long-term cycles of change. Since you like biology and following with your footnote 44 (p. 382): I have recently been reading about large genetic patterns in the evolution of living beings - something like, a fifth finger in human hands located in the same genetic map and arrangement as other anatomic features (appendix, maybe?) and therefore depending one on the other.

[RL] Yes. But there are different kinds of patterns. The 5th finger, etc. is a kind of leftover from the original bottleneck that allowed only the five-digit limb to get through, so the map itself is a survival. this isn't so much a pattern as a residue. Patterns of development are something different, but I don't want to say anything, as I'm still confused.

[TG] 5.- Polygenesis -monogenesis of languages. This is one of the points I'm most intrigued about at the moment. That's why I'm reading about anthropology (extremely popular in Spain, after Atapuerca's findings, by the way). They apparently make human language connected with the loss of the ability to breath and drink at the same time (they do not specify whether good wine or simply milk and water). If you feel like having a look into this there is a good web page in English:

<http://www.mncn.csic.es/atapuerca/bienve.htm>

since I don't think their books have been translated into English. They, by the way, also cherish the opinion that evolution and change tend to be rather haphazardous.

[RL] Well of course there's a relation between language as we now have it and restructuring of the velum and larynx. In newborn babies the larynx is in

the high position it is in adults of other primates, shoved up against the velum, and it drops later on in postnatal development. But though this was an enabling feature, surely the brain software, development of the left hemisphere, etc. had to be at least simultaneous. It wouldn't do too much good to have a nice speech apparatus without a brain to give it input. I'll look at the website. Thanks.

[TG] 6.- 6.4., p. 311. On the simplification of IE case-system. My Jaime says his friend the Indo-europeanist would ascribe himself to the thesis that there were fewer cases in IE -the evolution therefore being rather different. This view is becoming increasingly popular here.

[RL] I know there's a debate. Is primitive IE more like Hittite or Sanskrit? So far the evidence seems to me more in favour of the latter; though in some families like Uralic there's the opposite pattern. Finnish, the 'Lithuanian of Balto-Finnic', with 16 cases clearly developed from an ancestor with only 5.

Roger Lass, University of Cape Town
Trinidad Guzmán, University of León

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