Review of "Time Depth in Historical Linguistics", edited by Colin Renfrew, April

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This is a collection of 27 papers, mostly presentations at a symposium held at the

McDonald Institute in 1999. Contributions focus on two related issues: methods for

establishing absolute chronology, and linguistic knowledge about the remote past. Most

papers are restatements of the authors' well-known theories, but many contain innovations,

and some do describe new work. The ideological balance of the collection feels just left of

center. We do not find here wild multilateral phantasms, reconstructions of Proto-World

vocabulary, or idylls about pre-Indo-European matriarchal society. Or not much. These are

mostly sober academics pushing the envelope in attempts to reason under extreme

uncertainty.

One of the recurrent themes was that the development of agriculture may drive the

expansion of language families and therefore imply a date for the protolanguage. Colin

Renfrew describes his idea that that is what happened in the case of Indo-European (IE):

PIE was introduced into Europe at an early date, perhaps 8,000 BC. Peter Bellwood does

the same for the Austronesian dispersal, where there is actually a congruence of linguistic

and archaeological evidence to support the notion, then argues more boldly that

agriculture was the motive force for the breakup of language families throughout the world.

Another motif is that populations such as PIE speakers sat in place for millennia, which gave them enough time to develop many tree-confounding contact phenomena. Such beliefs may explain why this symposium had so many papers dealing with convergence. Renfrew writes about convergence within a PIE that has not quite broken up. Kalevi Wiik writes about a huge Uralic presence in prehistoric Europe, with contact phenomena explaining the emergence of branches like Germanic (Uralic substrate, Megalithic(?) superstrate). The rationale for these conclusions does not strike me as obvious. Raimo Anttila expresses a similar opinion about Wiik's theory as part of his own invaluable contribution highlighting recent developments in Uralic studies. Anttila's own contribution is on contact phenomena, specifically PIE loans into Uralic ca. 4000 BC. Other papers on convergence were perhaps less directly tied in to the theme of the conference. Yaron Matras and Peter Bakker each presents a typology and many fascinating examples of contact phenomena that can lead to highly mixed, even intertwined, languages. Reading Bakker's paper, I was struck by how fast languages can change in a contact situation; thousands of years of stewing is not required.

Most of the remaining papers focus more directly on methodological issues. This is the real strength of the collection, although I do not wish to give the impression that it covers the range of methodologies with any breadth or depth. Lyle Campbell gives a highly readable overview of techniques traditionally used to align linguistic developments with external events. T. V. Gamkrelidze's and Aharon Dolgopolsky's papers also describe some

traditional approaches to dating, such as linguistic palaeontology and loanword analysis. A couple of papers simply show how difficult data can be for any methodology. James Clackson claims that every single item ever adduced for the linguistic palaeontology of PIE is susceptible to another interpretation that provides no help at all in dating; for example, the all-important 'horse' could simply have been the wild horse. Larry Trask brings up some thorny chronological issues with examples from Basque, and James Matisoff pretty much succeeds in frightening readers away entirely from the very nearly intractable Southeast Asian languages. In general, the knowledgeable historical linguist will find this set of papers useful to the extent that she is interested in the excursus they take. For example, Campbell helpfully includes frank analyses of the weaknesses in Johanna Nichols' methodology (see below) as well as Dixon's (not Gould's) theory of punctuated equilibrium.

Several papers touched on the philosophy of science to varying degrees, and I found these discussions surprisingly useful. Sheila Embleton notes that historical linguists have traditionally had a bias for stating only what is virtually certain. But there is also value in different kinds of claims, such as identifying the most probable of several alternatives, or in quantifying the likelihood of an uncertain possibility. It is in this spirit that most modern users of glottochronology work: They know it gives at best rough estimates, but when such limitations are openly acknowledged, rough estimates are arguably better than no estimate at all or estimates based on what Marisa Lohr calls "intuitive glottochronology".

And it is glottochronology, in one form or another, that most of the methodological papers address. Embleton begins this set of papers with a condensed history of

lexicostatistic methods that have been used to establish dating. Several papers in this collection use a fairly traditional approach to lexicostatistics. Ilia Peiros performs a lexicostatistic subgrouping of seven Oceanic languages. Richard J. Hayward even tries to adapt it to an analysis of sprachbund convergence, with indifferent results. Christopher Ehret reports on 23 test cases from several different parts of Africa where archaeology and classical glottochronology give congruent results. Robert Blust, on the other hand, shows that the classical lexicostatistical subgrouping of Austronesian spectacularly fails to agree with the tree that has been constructed in accordance with the comparative method. He neatly demonstrates how lexicostatistics breaks down when different branches replace their vocabulary at different rates.

April McMahon and Robert McMahon's paper implicitly criticizes glottochronology from another direction. They discuss why dating can be reasonably reliable in biology: biologists have learned to focus not on phenotypes, whose rate of evolution is heavily constrained by functionality, but on those molecules whose exact structure does not make a big difference to the functioning of the organism. In such a subsystem, changes are essentially random and average out to a constant rate over long time spans. In linguistics, unfortunately, essentially all changes are functional, like phenotypes, so all hopes for constant rates of change are likely to be in vain.

Several papers suggest how glottochronology can be improved. Lohr reports the development of a new list of 128 concepts meant to replace the Swadesh list. Furthermore, she uses Fitch and Margoliash's least-squares method to derive "topologies" of languages

from lexicostatistic distances. An example with 18 IE languages suggests that it gives more felicitous results than traditional clustering methods. Lohr also illustrates that analyses using traditional word cognacy work better than those using phonostatistics. Unfortunately, as in most of these papers, details are sadly lacking: not even the new word list is given.

In an old paper recently translated, Sergei Starostin presents several modifications of his own, in addition to presenting a proposal for what he calls root glottochronology. His first modification is to square the time factor in Swadesh's formula, which would surely lead to paradoxes. Under Swadesh, we might expect English to replace a certain number of its basic words before this new millennium is out. Under Starostin, that number would vary depending on whether English is a Germanic language (with a small time factor) or an IE language (with a large time factor). The second proposal, which is more comprehensible, attempts to take into account that the less stable words in a list will be replaced more quickly than the stable ones. The formula gets quite complicated, but seems to fit known IE dates better. Will this extend to other verifiable dates, or is it a case of overfitting to a few data points? Ehret reports the modifications are unnecessary for his African languages. But Alexander Militarev applied the revised formula to the Afroasiatic languages. He reports an exact match with known Egyptian chronology and reports coherent results when building a subgrouping tree for 25 Afroasiatic languages. Against the trend, these papers give full data sets and details enabling replication.

Completely abandoning the mathematics of lexicostatistics, Mark Pagel shows how

maximum likelihood models can be used to subgroup languages, an adaptation of cladistic

methods used in biology. He illustrates the technique with a set of seven IE languages,

using 18 words, and reasonable results are obtained. Pagel's model is clearly a major

improvement over the classical Swadeshian mathematics, although computation time is not

mentioned.

Paul Heggarty describes a method for computing phonetic difference between

cognate words and thence entire cognate languages; he goes on, with evident reluctance, to

estimate the time of the IE breakup. This is thoughtful, new work with the potential for

being influential among linguists of a mathematical or computational inclination.

Unfortunately, of the many conceivable applications for whole-word phonetic comparison

algorithms, determining the historical distance between languages would seem to be the

least appropriate. If sound change is exceptionless, a single change should be treated as a

single historical event. But in Heggarty's method, the impact of two different changes can

vary by orders of magnitude, depending on how many words meet the condition for the

change and how dramatic the effect is per word.

William H. Baxter and Alexis Manaster Ramer point out that historical linguistics

lacks any tradition for significance testing. With great clarity and concision, they present a

way to test whether words in two languages—here, Hindi and English—are phonetically

more similar to each other than one would expect by chance, and therefore historically

connected. Most elements of their methodology are due to Oswalt, but this is the first time

that I have seen a completely accurate explication of how Monte Carlo methods can be

used to determine significance in such analyses. The only omission is of any discussion of the loanword problem, which could have been introduced by discussing why it was deemed appropriate to include Persian loans in the Hindi list and Scandinavian loans in the English list. Otherwise this article can serve as a standalone cookbook for how to do significance testing using phonological similarity metrics. Computer code is even given, although the choice of programming language—HyperTalk script—is bemusing.

The last section of the book is titled "Morphology, Spatial Patterning, and Beyond", by which is meant Nichols. She uses a combination of archaeological and linguistic evidence to argue for a very early (pre-glacial) peopling of the Americas. But Daniel Nettle neatly answers Nichols by showing how minor changes in her assumptions radically change the conclusions her model predicts. Parameters that are every bit as reasonable as Nichols' estimates can lead to the conclusion that the Americas were first settled around 14,000 BC—the communis opinio that Nichols attempts to overturn.

The book is published as two surprisingly heavy paperback volumes, glue bindings. The design is attractive. The footnotes, alas, are endnotes. The typography is pretty good; phonetic symbols, Greek, and Cyrillic (Russian) are all set competently. Consistent errors—using ess-zet (ß) for IPA beta (β) and misaligning superior diacritics with respect to slanted base characters—will not bother readers who are better adjusted than I am. And hopefully others will not be as credulous as I was in accepting that an awful lot of mixed languages have retroflex approximants as syllable nuclei, before realizing that every accented i in Bakker's contribution was systematically replaced by ɻ.