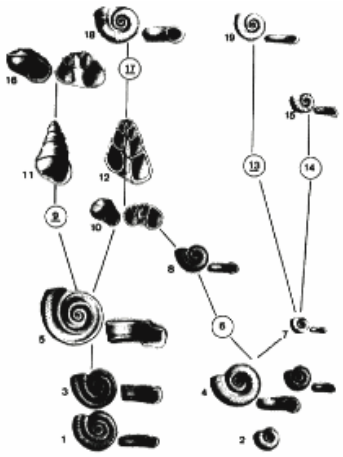
THE GENEALOGICAL WORLD OF PHYLOGENETIC NETWORKS



Statistical proof of language relatedness (Open problems in computational diversity linguistics 7)

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Statistical proof of language relatedness (Open problems in computational diversity linguistics 7)

The more I advance with the problems I want to present during this year, the more I have to admit to myself, sometimes, that the problem I planned to present is so difficult that I find it even hard to simply present the state-of-the-art. The problem of this month, problem number 7 in my list, is such an example — proving that two or more languages are "genetically related", as historical linguists (incorrectly) tend to say, is not only hard, it is also extremely difficult even to summarize the topic properly.

Typically, colleagues start with the famous but also not very helpful quote of Sir William Jones, who delivered a report to the British Indian Company, thereby mentioning that there might be a deeper relationship between Sanskrit and some European languages (like Greek and Latin). The article, titled *The third anniversary discourse, delivered 2 February, 1786, by the president* (published in 1798) has by now been quoted so many times that it is better to avoid quoting it another time (but you will find the full quote with references in my reference library.

In contrast to later scholars like Jacob Grimm and Rasmus Rask, however, Jones does not *prove* anything, he just states an opinion. The reason why scholars like to quote him, is that he seems to talk about probability, since he mentions the impossibility that the resemblances between the languages he observed could have arisen by *chance*. Since a great deal of the discussion about language relationship centers around the question how chance could be controlled for, it is a welcome quote from the olden times to be used when writing a paper on statistics or quantitative methods. But this does not necessarily mean that Jones really knew what he was writing about, as one can read in detail in the very interesting book by Campbell and Poser (2008), which deals at length with the supposedly overrated role that William Jones played in the early history of historical linguistics.

Macro Families

Returning to the topic at hand. The regularity of sound change and the possibility to prove language relationship in some cases was an unexpected detection of some linguists during the early 19th century, but what many linguists have been dreaming about since is to expand their methods to such a degree that even deeper relationships could be proven. While the evidence for the relationship of the core Indo-European languages was more or less convincing by itself (as rightfully pointed out by Nichols 1996), scholars have proposed many suggestions of relationship, many of which are no longer followed by the *communis opinio*. Among these *long-range proposals* for deep phylogenetic relations are theories that further unite fully established language families, proposing large *macro*-families — such as Nostratic (uniting Semitic, Indo-European, and many more, depending on the respective version), Altaic (uniting Turkic, Mongolic, Tungusic, Japanese, and Korean, etc.), or Dene-Caucasian (uniting Sino-Tibetan, North Caucasian, and Na-Dene), which span incredibly large areas on earth.

Given that it the majority of scholars mistrust these new and risky proposals, and that even scholars who work in the field of long-range comparison often disagree with each other, it is not surprising that at least some linguists became interested in the question of how long-range relationship could be proven in the end. One of the first attempts in this regard was presented by Aharon Dolgopolsky, a convinced Nostratic linguist, who presented a first, very interesting, heuristic procedure to determine deep cognates and

deep language relationships, by breaking sounds down to more abstract classes, in order to address the problem that words often do no longer look similar due to sound change (Dolgopolsky 1964).

Why it is hard to prove language relationship

Dolgopolsky did not use any statistics to prove his approach, but he emphasized the probabilistic aspect of his endeavor, and derived his "consonant classes" or "sound classes" as well as his very short list of stable concepts from the empirical investigation of a large corpus. The core of his approach, to *fix* a list of semantic items, presumably "stable" (i.e. slowly changing with respect to semantic shift), and to *reduce* the complexity of phonetic transcriptions to a core meta-alphabet, has been the basis of many follow-up studies that follow an explicitly quantitative (or statistic) approach.

As of now, most scholars, be they classical or computational, agree that the first stage of historical language comparison consists of the proof that the languages one wants to investigate are, indeed, historically related to each other (for the underlying workflow of historical language comparison, see Ross and Durie). In a blogpost published much earlier (Monogenesis, polygenesis, and militant agnosticism I have already pointed to this problem, as it is quite different from biology, where independent evolution of life is usually not assumed by scholars, while linguistic research can never really exclude it.

While proving language relationship of closely related languages is often a complete no-brainer, it becomes especially then hard, when exceeding some critical time depth. Where this time depth lies is not clear by now, but based on our observations regarding the paste in which languages replace existing words with new ones, borrow words, or loose and build grammatical structures, it is clear that it is theoretically possible that a language group could have lost all hints on its ancestry after 5,000 to 10,000 years. Luckily, what is theoretically possible for one language, does not necessarily happen with all languages in a given sample, and as a result, we find still enough signal for ancestral languages in quite a few language families of the world, that allows us to draw conclusions that go back about 10,000 years in the most cases, if not even deeper in some cases.

Traditional insights into the proof of language relationships

The difficulty of the task is probably obvious without further explanation — the more material a language acquires from its neighbors, and the more it loses or modifies the material it inherited from its ancestors, the more difficult it is for the experts to find the evidence that convinces their colleagues about the phylogenetic affiliation of such a language. While regular sound changes can easily convince people of phylogenetic relationship, the evidence that scholars propose for deeper linguistic groupings is rarely large enough to establish correspondences.

As a result, scholars often resort to other types of evidence, such as certain grammatical peculiarities, certain similarities in the pronunciation of certain words, or external findings (e.g.,from archaeology). As Handel (2008) points out, for example, a good indicator of a Sino-Tibetan language is that its words for *five*, *I*, and *fish* start with similar initial sounds and contain a similar vowel (compare Chinese *wǔ*, *wǒ*, and *yú*, going back to MC readings *ŋjuX*. *ŋaX*, and *ŋjo*). While these arguments are often intuitively very convincing (and may also be statistically convincing, as Nichols 1996 argues), this kind of evidence, as mentioned by Handel, is extremely difficult to detect, since the commonalities can be found in so many different regions of a human language system.

While linguists also use sound correspondences to prove and establish relationship, there

are no convincing cases known to me in which sound correspondences were employed to prove relationships beyond a certain time depth. One can compare this endeavor to some degree with the work of police commissars who have to find a murderer, and can do so easily if the person responsible left DNA at the spot, while they have to spend many nights in pubs, drinking cheap beer and smoking bad cigarettes, in order to wait for the spark of inspiration that delivers the ultimate proof not based on DNA.

Computational and statistical approaches

Up to now, no computational methods are available to find signals of the kind presented by Handel for Sino-Tibetan, i.e, a general-purpose heuristic to search for what Nichols (1996) calls *individual-identifying evidence*. So,computational and statistical methods have so far been based on very schematic approaches, which are almost exclusively based on wordlists. A wordlist can hereby be thought of as a simple table with a certain number of concepts (*arm*, *hand*, *stone*, *cinema*) in the first column, and translation equivalents for these concepts being listed for several different languages in the following columns (see List 2014: 22-24). This format can of course be enhanced (Forkel et al. 2018), but it represents the standard way in which many historical linguists still prepare and curate their data.

What scholars now try to do is to see if they can find some kind of signal in the data that they think would be unlikely to be detected by chance. In general, there are two ways that scholars have explored so far. In the approach proposed by Ringe (1992), the signalsthat are tested for in the wordlists are *sound correspondences*, and we can therefore call theses approaches *correspondence-based approaches to prove language relationship*. In the approach of Baxter and Manaster Ramer (2000), which follows the original idea of Dolgopolsky, the data are converted to sound classes first, and cognacy is assumed for words with identical sound classes. *Sound-class-based approaches* again try to illustrate that the matches that can be identified are unlikely to be due to chance.

Both approaches have been discussed in quite a range of different papers, and scholars have also tried to propose improvements to the methods. Ringe's correspondence-based approach showed that it can become difficult to prove the relationship of languages formally, although we have very good reasons to assume it based on our standard methods. Baxter and Manaster Ramer (2000) presented a more optimistic case study, in which they argue that their sound-class-based approach would allow them to argue in favor of the relationship of Hindi and English, even if the two languages are separated by at least 10,000 or even more years.

A general problem of Ringe's approach was that he tried to use combinatorics to arrive at his statistical evaluation. This is similar to the way in which Henikoff and Henikoff (1992) developed their BLOSUM matrices for biology, by assuming that the only factor that handles the combination of amino acids in biological sequences is their frequency. Ringe tried to estimate the likelihood of finding matches of word-initial consonants in his data by using a combinatorial approach based on the assumption of simple sound frequencies in the word lists he investigated. The general problem with linguistic sequences, however, is that they are not randomly arranged. Instead, every language has its own system of *phonotactic rules*, a rather simple grammar that restricts certain letter combinations and favors others. All spoken languages have these systems, and some vary greatly with respect to their phonotactics. As a result, due to the inherent structure of sequences, a *bag of symbols* approach, as used by Ringe, can have unwanted side effects and invoke misleading estimates regarding the probability of certain matches.

To avoid this problem, Kessler (2001) proposed the use of *permutation tests*, by which the random distribution, against which the attested distribution is compared, is generated

via the shuffling of the lists. Instead of comparing translations for "apple" in one language with translations for "apple" in another language, one compares now translations for *pear* with translations for "apple", hoping that this — if done often enough — better approximates the random distribution (i.e. the situation in which one compares several known unrelated languages with similar phoneme inventories).

Permutation is also the standard in all sound-correspondence-based approaches. In a recent paper, Kassian et al. (2015) used these approaches (first proposed by Turchin et al. 2010) to argue for the relationship of Indo-European and Uralic languages by comparing reconstructed word lists for Proto-Indo-European and Proto-Uralic. As can be seen from the discussion of these findings involving multiple authors, people are still not automatically convinced by a significance test, and scholars have criticized: their choice of test concepts (they used the classical 110-item list by Yakhontov and Starostin), their choice of reconstruction system (they did not use the mysterious laryngeals in their comparison), and the possibility that the findings were due to other factors (early borrowing).

While there have been some more attempts to improve the correspondence-based and the sound-class-based approaches (e.g., Kessler 2007, Kilani 2015, Mortarino 2009), it is unlikely that they will lead to the consolidation of contested proposals on macro families any time soon. Apart from the general problems of many of the current tests, there seem to be too many unknowns that prevent the community to accept findings, no matter "how" significant they appear. As can be nicely seen from the reaction to the paper by Kassian et al. 2015, a significant test will first raise the typical questions regarding the quality of the data and the initial judgments (which may also at times be biased). Even if all scholars would agree in this case, however, i.e. if one could not criticize anything in the initial test setting, there would still be the possibility to say that the findings reflect early language contact instead of phylogenetic relatedness.

Initial ideas for improvement

What I find unsatisfying about most existing tests is that they do not make exhaustive use of alignment methods. The sound-class-based approach is a shortcut for alignments, but it reduces words to two consonant classes only, and requires an extensive analysis of the words to compare only the root morpheme. It therefore also opens the possibility to bias the results (even if scholars may not intend that directly). While correspondence-based tests are much more elegant in general, they avoid alignments completely, and just pick the first letter in every word. The problem seems to be that — even when using permutations to generate the random distribution — nobody really knows how one should score the significance of sound correspondences in *aligned words*. I have to admit that I do not know it either. Although the tools for automated sequence comparison that my colleagues and I have been developing in the past (List 2014, List et al. 2018) seem like the best starting point to improve the correspondence-based approach, it is not clear how the test should be performed in the end.

Additionally, I assume also that expanded, fully fledged, tests will ultimately show what I reported back in my dissertation — if we work on limited wordlists, with only 200 items per language, the test will drastically lose its power when certain time depths have been reached. While we can easily prove the relationship of English and German, even with only 100 words, we have a hard time doing the same thing for English and Albanian (see List 2014: 200-203). But expanding the wordlists bears another risk for comparison (as pointed out to me by George Starostin): the more words we add, the more likely it is that they have been borrowed. Thus, we face a general dilemma in historical linguistics: that we are forced to deal with sparse data, since languages tend to lose their historical signal rather quickly.

Outlook

While there is no doubt that it would be attractive to have a test that would immediately tell one whether languages are related or not, I am becoming more and more skeptical about whether this test would actually help us, specifically when concentrating on pairwise tests alone. The challenge of this problem is not just to design a test that makes sense and does not overly simplify. The challenge is to propagate the test in such a way that it convinces our colleagues that it really works. This, however, is a challenge that is greater than any of the other open problems I have discussed so far in this year.

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