

REVIEWS * ОБЗОРЫ И РЕЦЕНЗИИ

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CONVERGENT OR DIVERGENT LINGUISTIC DEVELOPMENT?

The comparative method of Indo-European linguists, which has become a widely accepted paradigm for historical linguistics in general, is based upon the principle that related languages diverge with passage of time. Robert M. W. Dixon has developed a general model of long-term linguistic equilibrium within an enduring linguistic area, followed by an episode of drastic change (Dixon 1997; 1999). He sees the punctuation episode, when the dispersal is actuated that triggers the divergence, underlying the family tree formation, as a rather special event. The more normal situation is one where convergence processes prevail. R. M. W. Dixon's standpoints are supported by Daniel Nettle (1999). D. Nettle argues that when Modern Man *Homo sapiens sapiens* moved initially from the eastern part of Africa into a new area would be a great variety of available geographical niches and the population would fission repeatedly and often. The newly split languages would go on changing until they were sufficiently different to be identified as different language families. But then he follows R. M. W. Dixon's convergence approach and predicts a decrease in number of apparent language families.

I am convinced that R. M. W. Dixon's model of punctuated equilibrium is the best for explaining emergence of the Uralic language family. Likewise, I see the only possible way for a further development of historical linguistics in its symbiosis with population genetics and archaeology which, in turn, support R. M. W. Dixon's model. The formation of intergrated Uralic languages by way of convergence has been supposed much earlier also by János Pusztyai, Kalevi Wiik and me. But in his book, deeply affecting upon the theoretical thought of the global historical linguistics, "The Rise and Fall of Languages" R. M. W. Dixon writes: "Work on Indo-European is, by and large, of the highest quality and has acquired a justified prestige. (Add to this the fact that the IE languages themselves include the major prestige languages in the world today.) As a result the family tree is the 'received model' of linguistic relationship which scholars working in other areas attempt to apply to their own groups of languages. Sometimes the family tree model is applicable, and a genetic relationship can be proved in the same way that has been for IE, e.g. Uralic, Semitic, Algonquian." (Dixon 1997 : 28; 1999 : 28).

But I must note that Angela Marcantonio begins her book "The Uralic Language Family: Facts, Myth and Statistics" (Marcantonio 2002) with the following words: "It is often claimed (for example Dixon 1997 : 28) that the family tree model of historical linguistics is realistically only applicable in a small number of language families, namely Indo-European, Semitic, Polynesian and Uralic. The purpose of this book is to examine closely the foundation of this claim for Uralic, by bringing together the main linguistic and non-linguistic evidence in one volume. My conclusions on the origin and nature of the Uralic languages will differ in several important respects from the conventional view. The detailed analysis is carried out in this book, uncovered a total absence of scientific evidence in favour of the notion that the Uralic languages form a language

family, that is, a genetically coherent group of related languages. Therefore, in short, I shall conclude that Uralic is not valid a node.” (c.f. also Marcantonio 2001; Marcantonio, Nummenaho, Salvagni 2001). And Lutz Edzard breaks to shivers the idea of the Semitic family tree model in his book “Polygenesis, Convergence, and Entropy: An Alternative Model of Linguistic Evolution Applied to Semitic Linguistics” (Edzard 1998; see also Edzard 2000). Unfortunately, I have no information about the state with language family tree conception concerning the Algonquian or Polynesian languages.

Colin Renfrew notes that the first linguist to have his doubts about the divergent origin of the Indo-European language family and presented a convergent model of the origin seems to have been N. S. Trubetzkoy (1939) (Renfrew 1999 : 261). C. Renfrew indicates that “the model of the gradual convergence of initially independent languages to form a new language family as conceived by Trubetzkoy ... does clearly anticipate aspects of Dixon’s work” (Renfrew 2000 : 13). The theory of convergence has had supporters already before and after N. S. Trubetzkoy, e.g. the Indo-Europeist Hugo Schuchardt and Finno-Ugrists Dmitrij Bubrich and Valter Tauli (see especially Honti 2000 : 135–138). For some time already in each of the three independent Finno-Ugric countries there have been one rather remarkably active converge-minded Finno-Ugrist (better known as contact-theoretician). In Hungary, since the 1980s already, János Pusztay is writing his diachronic papers in the spirit of the theory of convergence, in Estonia, since the beginning of the 1990s, I am doing it, since the middle of the 1990s, Kalevi Wiik is pursuing the ideas of contact theory (see about our gist of ideas in Künnap 1998; 2000). Johanna Nichols is known to have criticised the issue in addressing the diversity of world languages, it is the model of the Indo-European language family that is usually proceeded from, however, it need not suit the former at all (Nichols 1992). As J. Nichols sees it, the language tree model suits well for explaining the diversity on the Eurasian steppe area: from time to time new languages move in from east, spreading all over the area. Other areas — for instance the Caucasian mountainous region — maintain the diversity of various languages and a language from elsewhere has no chance of spreading over the whole area. Gösta Bågenholm writes (1998): “That social and ethnic integration has overcome the segregating forces during the millennia is illustrated by Fenno-Scandia having few languages and a slight pre-industrial split of dialects, while New Guinea over a long period of time has been and will remain the language-richest region of the world. [---] these converging forces over time prevailed in Scandinavia and Finland.”

During the last couple of years the work of the international multidisciplinary workteam “The Roots of Peoples and Languages of Northern Eurasia” has focused on quite new theoretical considerations concerning the origin of the Uralic languages. The workteam has grown out of the partnership that since 1997 organises closed symposiums with invited participants and publishes collection of papers read there (The Roots of Peoples and Languages of Northern Eurasia I (1998); The Roots of Peoples and Languages of Northern Eurasia II and III (2000); The Roots of Peoples and Languages of Northern Eurasia IV (2002)). At present the workteam consists of about thirty persons all over Europe, involving besides linguists also representatives of history, archaeology, population genetics, physical anthropology and other fields of science. The workteam supposes that there was no narrow Uralic proto-home with unitary Uralic proto-language, proto-race, proto-religion etc. in the east in the Volga or southern Urals area. There was a vast area of periglacial population in northern Europe from the Atlantic to the Ural Mountains. This population was the very first wave of spread of *Homo sapiens sapiens* to Europe. They could speak either relatively unitary Uralic languages or quite different languages. Groups of this population had linguistic and other contacts with each other in all respects. If there were quite different languages, these entered into a language union (*Sprachbund*) and by means of the intermediate *lingua franca* (or link language) these different languages unified up to a definite level but each one of them preserved some features of the initial language. If there were only relatively uni-

tary Uralic languages initially, they, too, have preserved some old features of their own (See particularly Künnap 1998; 2000).

Recently Johanna Laakso has expertly presented the difficulties confronting both convergence and divergence theories (or contact theory and traditional comparativistics) in clarifying the earlier history of the relationships between languages (interlinguistic relationships), particularly in the area of morphosyntax, at that (Laakso 1999). At the end of her paper she writes as follows: "The real challenge for future is now to find — after correcting the areal extremist exaggerations — a way to connect the traditionally self-dependent comparative method with studies of areal and contact phenomena. [---] Combining these two, the studies of internal vs. external explanation, sometimes seems hopelessly like mixing oil with water." (Laakso 1999 : 69). It becomes more explicit when we replace the opposition "internal vs. external explanation" by a question if the earlier history of interlinguistic relationships concerns mainly a convergent or divergent development. Traditional comparativists tend to regard it as an axiom that traditionally postulated families have developed by divergence, and so J. Laakso, too, proceeds from that axiom. When proceeding from that axiom the whole traditional comparativist arsenal will be set in play, including language trees and proto-languages, as well as preferably narrow proto-homes and departures from them.

A viewpoint by Uralic contact theorists was presented principally correctly by J. Laakso as follows: "There was not one proto-language but heterogeneous proto-Sprachbund with many centres, an area of constant convergence and divergence processes (this view has been propagated especially by Puszta 1995)" (Laakso 1999 : 39). However, J. Laakso considers this view as ontologically and methodologically impossible (p. 60). Methodologically, certainly, if we proceed from the methodology of traditional comparativistics, but why then ontologically impossible? There is no answer found to this question in J. Laakso's paper. Another standpoint of Uralic contact theorists is again principally correct as presented by J. Laakso in the following way: "There was no well-formed, complete proto-language because the proto-language was defective: it was (or: had originally been) a *lingua franca* used between speakers of different languages" (Laakso 1999 : 59–60). J. Laakso argues against this point of view as follows: "... we have numerous cases of language death as powerful evidence to the contrary: after a stage of bilinguality (with the two contacting languages living together but apart), the generation following the terminal speakers e.g. Gaelic in Ireland (or Sámi in Lapland) does not speak a mixture of Gaelic and English (Sámi and Finnish/Swedish/Norwegian/Russian) but a language which, despite some possible substrate features, is indisputably English (Finnish/Swedish etc.)." (Laakso 1999 : 60).

The evidence about language shift given by J. Laakso are certainly correct, however, she herself leaves two more options: "There are, of course, two exceptions to this axiom. First: for languages closely related (or dialects of the same language, or a pidgin and its lexifier language), it is so easy to borrow not only words but affixes, structures and other features that a massive influx of borrowings can almost or completely cover some original features. In borrowing between closely related languages, "contamination" is a frequent phenomenon. [---] Another exception are the real mixed languages now known in many parts of the world ... However, mixed languages only develop in exceptional circumstances, and they are typically in-group languages, not link languages." (Laakso 1999 : 60–61). Well, let it be that way. But when proceeding from the convergent development theory of interlinguistic relationships I fail to see the use of the traditional comparativist term "closely related languages", or another term "in-group language". This is the position of Uralic contact theorists that primarily different languages converged, among them some to the extent that traditional comparativists can regard them as closely related languages or interpret their mutual relationships as in-group relations.

J. Laakso does not seem to find it possible to observe Proto-Uralic as a form-scanty *lingua franca*, since "Proto-Uralic ... seems to have been a clearly agglutinative language with a rich morphology: at least five case endings, personal and possessive suffixes,

number markers, several derivational suffixes etc. [---] Pidgin languages typically have little morphology; in extreme cases (although by far not always ...) they are isolating languages." (Laakso 1999 : 66). J. Laakso seems to argue on behalf of a onetime actually existent Proto-Uralic, although she herself emphasises that "the proto-language reconstruction and the actual language spoken sometimes in the past are two different things" (Laakso 1999 : 63). What I have in mind is the reconstruction of Proto-Uralic, made by linguists. I think Juha Janhunen's version of Proto-Uralic (Janhunen 1981; 1982) is relatively good, reflecting minimal lexical and grammatical material, reconstructed into the supposed Proto-Uralic on the basis of maximally strict criteria. What is J. Janhunen's crop like? No more than 140 word stems, the phonetic system (based on the latter), about fifteen morphological suffixes and some syntactic features, actually rules of word order. However, it does not automatically mean that insisting on the traditional comparative divergence model, internal reconstruction, proto-languages and language trees would solve the problem, although holding fast to the habitual may appear safer than heading for yet little-known new tracks. But the tracks are really there and, unavoidably, they have to be covered.

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