Book review: Kalevi Wiik. Eurooppalaisten juuret. Jyväskylä: Atena kustannus. 2002.[1]

Reviewed by Santeri Palviainen

Introduction

The origins of Finns have been debated hotly over the past couple of years in Finland. In his numerous publications in the popular press, Kalevi Wiik, professor emeritus of Phonetics from University of Turku, has made bold claims about the contacts between the Finno-Ugric and Indo-European languages. These claims have been met with unanimous criticism from Uralicists and Indo-Europeanists alike. The defenders of W's ideology have claimed that W's framework represents a new paradigm in historical linguistics, and the critics are merely old-fashioned. Indeed, W himself parades in the preface of the book that "he needs no more to apply for jobs" and "he does not have to be afraid of the attitudes of the Finnish traditionalists towards the thoughts that will change the traditional way of thinking and the traditional structures in our country in the near future." The book has also been nominated for the prestigious non-fiction book award Tieto-Finlandia.

The debate may seem rather bizarre to the outsider, and the nature of the debate is inevitably connected with the sociology of linguistic science in Finland. In this review, I will focus only on the linguistic side of W's argumentation and, more specifically, on the Indo-European data. I do not claim any competence either in archaeology or in genetics. However, W's arguments are *primarily* linguistic, contrary to his insistence that his research represents an interdisciplinary synthesis, and hence his book merits reviews specifically from linguists. The defenders of W's ideology might claim that such criticisms are invalid because they do not account for the whole analysis. My reply is that if the linguistic argumentation is fundamentally misguided, the model W is advocating cannot be true. To use analogy, a tripod with a rotten leg will inevitably collapse no matter how strong the other two legs are. Hence W's model is doomed to fail if it does not have support for its linguistic argumentation, and showing that to be the case is this review's intent. Furthermore, the validity of the particular

[1] All translations of passages and quotations are mine. The English translation of the book which is said to be forthcoming may have a different wording.

archaeological models advocated by W has been questioned in archaeological literature. I shall leave the specific questions of the Uralic linguistics to Uralicists.

Organization

W's book is divided into three parts which are subdivided into (unnumbered) chapters. There are a number of issue regarding the orientation of the book that deserve mention in this context. One of W's more peculiar mannerisms is to use formulae to "derive" languages. For example the formula for Late Proto-Germanic (pp. 139) is:

$$(((\mathrm{IE} + \mathrm{e\text{-}ba}) + \mathrm{e\text{-}lbk}) + \mathrm{su}_{\mathrm{bs}}) + \underline{\mathrm{bs}}^{\mathrm{hs}}$$

To put this in plain English, to get Late Proto-Germanic one has to take Proto-Indo-European (IE) and apply Pre-Balkan substratum (e-ba), apply Pre-Linear Pottery substratum (e-lbk), apply Uralic substratum (with a Basque substratum) ($\mathrm{su}_{\mathrm{bs}}$), and finally apply Basque superstratum (with a Hamito-Semitic superstratum) ($\underline{\mathrm{bs}}^{\mathrm{hs}}$).

W also makes very extensive use of maps with such a broad range of colors that even the manufacturers of Crayola should be jealous. The colors exemplify different languages and when the languages get mixed, the colors also get mixed correspondingly. Hence when the northwestern Indo-European language (blue) and Uralic language (yellow) mix, the outcome is Germanic (green) (simplified from fig. XV pp. 159); see later for more details. As W quite rightly points out in the preface, the colors prove absolutely nothing. The metaphor is, however, strong and should be used more responsibly in a book directed to laypeople. Despite the small print in the preface, it is simply not clear from the actual text that the mixing of colors does not constitute evidence, especially given the lack of actual linguistic examples in most cases therein.

Part I

The first part is boldly titled "General thoughts about searching for roots" (pp. 22–52). In this section W reviews the basics of the study of European prehistory illustrating the use of linguistics, archaeology, and genetics in this context. He emphasizes the importance of the synthesis of all these sciences, or rather in fact emphasizes archaeology and genetics over linguistics. W asserts that archaeological and genetic assumptions are closer to "facts" and linguistic assumptions are closer to "suppositions" (p. 23).

The role of genetics in prehistory is far from settled. The genetic data are very important and interesting, but it must be borne in mind that genetics deals only with the physical component of the people in question. Similarly pieces of pottery can tell only so much about the languages spoken by the people who used them. However, efforts can be made to discern which languages were spoken by which populations in a given archaeological framework. There is a huge literature on the topic, I refer to the references in Carpelan & Parpola (2001) and Mallory (1989) for further discussion in an Indo-European context.

The interdisciplinary approach actually makes practicing science doubly difficult: instead of mastering one science one has to master two sciences, or in our case, three sciences: linguistics, archaeology, and genetics. Interdisciplinary research does not mean that we give up the strictest standards followed in the individual sciences. To think otherwise would be a great error.

One of the central concepts W employs is *lingua franca.* A lingua franca is defined by him here as "the international language used by populations that do not naturally speak the same language" (pp. 38). To use W's two examples, English in the twentieth century and Latin during the Roman Empire served as linguae francae in their respective regions and, indeed, English may be regarded as the first truly global lingua franca. But to claim that Neolithic hunter-gatherers made extensive use of such languages spoken in vast areas is simply untenable (to take an example: W claims that Proto-Uralic was spoken in an area that reached from Rhein to the Urals, pp. 94–5). His claims that Proto-Uralic was the lingua franca of the hunter-gatherers in the Ukrainian refugium, Proto-Indo-European the lingua franca of the Balkan refugium, and Basque the lingua franca of the Iberian refugium are simply explaining *obscura per obscurissima.* The list could go on and on, but the bottom line is: W fails to present *any* evidence whatsoever for his hypothesis. The linguistic situation of the post-glacial Europe is simply not known and to claim otherwise is wild goose chase.

This is not to claim that there were neither linguistic contacts nor linguae francae in the prehistoric eras -of course there were-, but the research on prehistoric linguistic contacts has to proceed carefully given the dearth of evidence. It has long been known that the linguistic contacts have been very intense between Uralic and Indo-European languages since the reconstructable beginnings of those languages. Masses of loanwords and structural changes in various Uralic languages, especially the Baltic-Finnic languages, testify to this.

Part II: The most important phases of the Europeans

The second part is called "The most important phases of Europeans." The chapter reviews the basics of European prehistory. There were three refugia to which people retreated during the ice age: the Iberian, the Balkan, and the Ukrainian. The refugia play a pivotal role in W's argumentation.

According to W, the lingua franca in the Iberian refugium was Basque, and in the Ukrainian refugium the lingua franca was Finno-Ugric language. Proto-Indo-European was one of the languages spoken in the Balkan refugium. W cites the nonsensical theory of Darlington (1947) that there were phonetic features borrowed from an unknown substratum in the area that roughly corresponds to the area colonized by the people from the Iberian refugium. Of course the substratal features cited by Darlington are in any case secondary and very late, cf. the interdental fricatives in Castilian Spanish, Germanic, and Insular Celtic etc.

W is also happy to accept Vennemann's very controversial theory that the entire area of Western Europe north of the Alps was Vasconic-speaking (in effect, a preform of modern-day Basque). He also ventures as far as to suggest that there is a Basque substratum in Finno-Ugric, which, according to W, extended to Central Europe. This is explained by assuming that the lingua franca of all Northern Europe was Finno-Ugric after the Basque-speaking hunter-gatherers switched their language to Finno-Ugric. The Basques subsequently left a substratum in the Finno-Ugric language. W fails to present the slightest shred of evidence.

In W's view the Indo-European languages spread to Europe with the spread of agriculture. This view has been advocated by Renfrew (1987), but it has not been widely accepted, due to its problems for explaining the well-documented contacts between Proto-Indo-European and Proto-Uralic among other things. Chronology also presents major difficulties if one is to assume that Renfrew's theory is correct. The lifeline is indeed very thin: if the Indo-Europeans did not spread from the southeast (the Balkans and Anatolia), but rather from the east (Ukraine), W's archaeological scenario really collapses (for discussion see Mallory (1989: 177–181)).

Part III: Individual populations and languages

Part III is titled "Introduction to the Indo-European populations and languages." The part forms the bulk of the book and encompasses over 300 pages. W begins his overview with the Indo-European languages, and this is the focus of this review: does W present a coherent and tenable view of the development of the Indo-European languages in Europe? Does he back up his claims with acceptable data from the Indo-European languages?

Let us suppose for a moment that Renfrew's theory of the origins of the Indo-Europeans is correct. W claims that, as the people who used Proto-Indo-European as their lingua franca proceeded from Anatolia to the Balkans, they picked up a "Pre-Balkan substratum." W simply stipulates such a substratum without any evidence. This substratum caused the language to split into two: a Central European dialect and a Mediterranean dialect. The Central European dialect furthermore acquires a Pre-Linear Pottery substratum. The Central European dialect serves then as the precursor of Germanic and Balto-Slavic (and maybe Celtic).

The Mediterranean dialect acquires a Pre-Impressed Ware / Pre-Painted Ware substratum and serves as the precursor of Italic and Celtic. The careful reader now observes a discrepancy: which substrata do we expect to find in Celtic? W offers a particularly baffling account. From pp. 145–9 we are to understand that there in fact were two different Celtic proto-languages, Central European Celtic and Iberian Celtic. The former has substrata from Pre-Balkan, Pre-Linear Pottery, and furthermore from Basque and an unknown language Y (sic!!), whereas Iberian Celtic has Pre-Balkan, Pre-Impressed Ware, Iberian, Tartessian, and Basque substrata. It goes without saying that W's account is highly improbable. W fails to breathe a word on the properties of the donor languages (Pre-Balkan, Pre-Linear Pottery etc.) that might help explaining the features of the actual Celtic languages. Reconstructing two Proto-Celtic languages is clearly unnecessary, as Proto-Celtic is well reconstructable on the basis of the attested Celtic languages. Italic fares no better: Italic has substrata from Pre-Balkan, Pre-Impressed Ware, and Pre-Italic. No data whatsoever is offered supporting these scenarios.

W's theory of the Megalithic religion (!) is particularly puzzling (pp. 137–9). W assumes that Germanic and Balto-Slavic separated due to the language contact with the Megalithic culture and Megalithic language. He believes that Megalithic culture was a religion (!) which was spread by small elite groups. W also believes that the Megalithic culture was a prelayer to Christianity and the stone edifices were pre-stages of Christian churches. The "priests" and the other "men of the church" spoke the language of the "church" i.e. Megalithic language i.e. Hamito-Semitic. W asserts that Megalithic missionaries (!!) entered Basque-speaking areas and left a superstratum in the Basque language spoken in those areas. The missionaries then apparently acquired Basque which, however, had a Hamito-Semitic superstratum and then went on to spread the good word to the soon-to-be Germanic tribes. There is next to no evidence that the Megalithic culture (if there ever was any) enjoyed any status as a special cult, apart from the fact that many of the megalithic edifices are built in such a way that the sun shines on a particular spot inside the edifice on various solstices and equinoxes -one may interpret this as one wishes-, nor is there any proof of any missionary activity at the time (not to mention the gaping anachronism here), nor of any Basque or Hamito-Semitic language contacts with the Germanic speakers (*pace* Vennemann).

The darkest chapters of the book are the ones on the development of the Germanic languages. Late-Proto-Germanic in W's view had no less than three different substrata and one substratum. The first one was Pre-Balkan, the second Pre-Linear Pottery, and the third Finno-Ugric with a Basque substratum in itself. All these substrata are shared by Balto-Slavic as well. The superstratum allegedly comes from Basque. I will not treat here all the proposed substratal effects; for that I refer the reader to the debate in *Tieteessä Tapahtuu* (issues 7/97, 1/98, 3/98, and 5/98) between W, and Petri Kallio, Jorma Koivulehto and Asko Parpola. W's arguments are effectively torn apart by K,K&P, so I need not repeat the list here other than mention the most obvious errors from the book.

W makes the assertion that the vowel alternations in the English strong verb paradigms (W: *do-did-done* and *sit-sat-sut* (sic!!)) derive from the coloration effects caused by the socalled laryngeals (sic!). W does not seem to have grasped the nature of the laryngeals in Proto-Indo-European, which is understandable, given that the elementary textbooks that he uses as his sources for Indo-European data are grossly outdated or eccentric, and he seems to have misunderstood his only source with reliable reconstructions with laryngeals (Koivulehto 1988).

We owe the discovery of the laryngeals to the great Swiss linguist Ferdinand de Saussure whose findings were later confirmed by the decipherment of Hittite where the laryngeals were preserved in part. According to current mainstream theory, the laryngeals had two main effects: lengthening and coloration (Mayrhofer 1986: 121–50). We distinguish three different laryngeals with their respective effects:

*eh$_1$ > *ē   *h$_1$e > *e
*eh$_2$ > *ā   *h$_2$e > *a
*eh$_3$ > *ō   *h$_3$e > *o

The vowel alternations in the Germanic strong verbs, however, were not caused by the laryngeals but rather by the Indo-European ablaut. We need to distinguish two kinds of ablaut: quantitative (three grades: full grade, zero grade, and lenghtened grade) and qualitative (e- and o-grade) and combinations thereof (full e-grade, lengthened o-grade etc.). I illustrate this with Greek:

full e-grade: *leípō* 'I leave'
full o-grade: *le-loípa* 'I have left' (perfect)
zero grade: *é-lipon* 'I left' (aorist)

Similarly, the ablaut shows up in Germanic strong verbs:

PIE (full e-grade) *b^h**er**- > PGmc. *-ber- > NHG *ge-bären*
PIE (full o-grade) *b^h**or**- > PGmc. *-bar- > NHG *ge-bar*
PIE (zero grade) *b^h**8**- > PGmc. *-bur- > NHG *ge-boren*

Hence laryngeals have nothing to do with these particular English vowel alternations.

W's chapter on Grimm's law deserves special attention as it strikes one as an especially unlikely scenario. W seems to assume that Proto-Indo-European had four series of stops (voiceless unaspirated, voiceless aspirated, voiced unaspirated, voiced aspirated). Apparently in his view, voiceless unaspirated and aspirated stops were allophones of the same phoneme. W fails to cite the source of this view, but let it be noted that this non-standard theory of Indo-European stop system derives from Gamkrelidze & Ivanov (1995: 5–70, cited only in W's references). However, the theory does not strike one as very promising since the Glottalic Theory advocated in G & I (1995), among others, has its own problems and represents at best a minority view. Even if one is to accept the views represented by the Glottalic Theory, it does not follow from the Indo-European data that the voiceless stops were allophonically aspirated (let alone affricated) as advocated by G & I. Needless to say, W fails to breathe a word of these complications. For details, consult Mayrhofer's excellent survey of the phonology of Proto-Indo-European (Mayrhofer 1986). We can confidently discard the idea of allophonic aspiration of the Proto-Indo-European stops, and stick to the traditional view with the following VOT distinctions: [-vce, -asp], [+vce, -asp], and [+vce, +asp].

W's reconstructions of the Indo-European phonetics may come as a surprise even to Indo-Europeanists in favor of the Glottalic Theory. He claims that the Indo-European voiceless unaspirated stops were allophonically aspirated, or better yet, contained frication to the extent that they were in effect affricates (sic!); the aspiration of the traditionally voiced aspirated series was similarly realized as frication. Hence the series looked like this (pp. 168):

pϕ÷ tþ kχ
bβ dð gγ

Hence W explains Grimm's law as follows: the speakers of the Finno-Ugric language pronounced only the frication and not the stop, and thereby the stops shifted: *pϕ > *ϕ > *f and *bβ > *β > *v etc. The problem is that no Indo-Europeanist would ever reconstruct such sounds as no branch of Indo-European offers comparative support. As pointed out to me by Claire Bowern (personal communication), no language in the world has a system W would want to reconstruct for Proto-Indo-European, namely two affricated series + one voiced series which in Gamkrelidze & Ivanov's interpretation was glottalized. Hence we would have the following stop system for Proto-Indo-European in W's variation of Gamkrelidze & Ivanov's system:

pϕ tþ kχ[2]
p' t' k'
bβ dð gγ

No such system is attested.

The assumption that the devoicing of the voiced unaspirated stops could conceivably be considered Finno-Ugric substratum can be entertained, but then again it can be considered to stem from other kinds of substrata as well: it is well known that Etruscan did not make voicing distinctions. Why cannot we claim that Etruscan served as the substratum language? Etruscan furthermore has <f> /p^h/, <θ> /t^h/, and <χ> /k^h/ which

[2] I merge all the velar series under /k/

are generally thought to be the precursors of the fricatives produced by Grimm's law. Hence it would be much easier to claim that an Etruscan substratum caused Grimm's law than Finno-Ugric. Of course this idea is untenable for a number of reasons and should be rejected along with W's implausible account.

W reveals his true scholarly attitude on page 179: he states outright that it does not matter to him if some of the substratal phenomena he proposes are proven wrong, because it is enough if even **one** of the phenomena **provably** derives from a Finno-Ugric substratum (emphasis from original text). Where could this proof come from? Unless we are willing to wait for some divine pronouncement on this issue, there can be no absolute certainty. I have relatively little to say to W's treatment of the origin of West Germanic as he follows the standard assumptions concerning Celtic, Scandinavian, and Norman French influences on English. A Celtic substratum in High German is not as uncontroversial as W has us believe, but future research will no doubt shed more light on this issue.

However, W's theories regarding the genesis of East and North Germanic again warrants some attention. W is eager to assume Wend / Venetic substratum in East Germanic, and more specifically, East Slavic and Iranian substratum in Ostrogothic. In Visigothic there are Wend / Venetic + Dacian and Thracian substrata, according to W. In contrast to W's mysterious substratal languages (Pre-Linear Pottery and the like), we do know quite a bit about the East Slavic and Middle Iranian languages of the time, and I'd be curious to hear from W which features of Gothic exactly are attributable to East Slavic or Iranian substratum. Unfortunately, W is silent on this issue.

North Germanic is claimed by W to have an Early Proto-Finnic substratum (Frühurfinnisch, varhaiskantasuomi), termed Saami-Finnic by W (pp. 205). One of the important Saami-Finnic substratal features was the special development of unstressed vowels which underwent drastic changes (pp. 218–222). However, also West Germanic and East Germanic show similar developments, albeit not to the same extent.

For more recent contact phenomena between Saami and the Norwegian and Swedish dialects the research is not yet conclusive. I think, however, that from the phonetic substratum effects at least vowel balance, vowel reductions, isochrony, and metaphony can be connected with the general trend of the Germanic languages to emphasize the stressed syllable (Prokosch's law) and reduce the unstressed syllable (attested in all

Germanic languages). There is no doubt that this is ultimately connected with the stress shift to the first syllable of the stem. The unstressed syllables were also subject to their own phonological developments (the socalled Auslautgesetze). W is not the only one to connect the Germanic stress shift with Uralic; cf. also Salmons (1992). In this connection the initial stresses of Proto-Celtic and Proto-Italic also call for explanation and Uralic definitely cannot be used as one.

One tool employed by W to dismiss criticisms regarding the dating of linguistic changes is to claim that the phenomena were bubbling under for perhaps millenia before they actually surfaced. He claims that this was the case with i-umlaut which reflects Uralic vowel harmony, despite the large temporal gap between the end of Uralic substratum and the first instances of i-umlaut.

He goes on to Balto-Slavic. It has been long suspected that there are contact effects in Latvian and East Slavic, but W wants to see Finno-Ugric substratum in Proto-Balto-Slavic. Since he also assumes that there is a Finno-Ugric substratum in Germanic, it would be interesting to see to what extent the substrata in BSl. and Gmc. coincide. Unsurprisingly they do not. Let us take a few examples:

(1) Velar stops

Proto-Indo-European had three series of velar stops: plain, palatal, and labiovelar. Germanic merges the plain and the palatal series but retains the labiovelar series, but in contrast Balto-Slavic merges the plain and the labiovelar series and keeps the palatal series. According to W *both* can be argued to be Finno-Ugric substratum (pp. 171–2 and 265). What gives?

(2) Aspiration

Balto-Slavic merges the unaspirated and aspirated voiced stops, whereas Germanic retains three series separate, albeit in a different form than in PIE. W assumes that the loss of aspiration is the common denominator. This is true but it is also true that both branches kept voicing distinction—why voicing but no aspiration? Both seem to me equally un-Uralic. W offers no explanation why Germanic and Balto-Slavic went different ways here. It is a curious fact of latter-day Germanic that the voiceless stops are aspirated in many positions whereas the Baltic and Slavic voiceless stops generally are not.

(3) Large case inventory

W offers a further contradicting line of reasoning: the Finno-Ugric substratum helped the Baltic and Slavic languages to preserve their PIE cases. At the same time, he also claims that the Finno-Ugric substratum is also the cause of the reduction of the Germanic case system.

For Slavic W offers a potpourri of previous research. I shall not go into details at this point. For a thorough evaluation of potential Uralic substrata in Indo-European, see Kallio (2001) and (2002). For the rest of the Indo-European languages I cannot go into any detail here. On the other hand, this seems unnecessary as the chapters do not really contain any relevant linguistic information. I will take one further claim that even W views as hypothetical. This is the notoriously difficult case of Insular Celtic languages which differ radically from the Continental Celtic languages (Gaulish and Celtiberian among others). In W's view there are the following substrata in Insular Celtic: Pre-Balkan, Pre-Linear Pottery, Basque + Pictish and/or Finno-Ugric. W thinks that parts of Britain may have been Saami-speaking before the arrival of the Celts (p. 309). He considers the possibility that plural morpheme *-d* in Breton could be connected with one of the Uralic plural morphemes (*-t*), lack of verb 'to have' etc. He attributes these ideas to unnamed linguists. Furthermore W assumes that there is Hamito-Semitic adstratum in Insular Celtic. This is a century-old idea which has enjoyed renewed interest, for details see Gensler (1993) and Vennemann (2003). This is not totally inconceivable in contrast to other substrata proposed by W; for assessment of evidence see Gensler (1993).

I will also skip the remaining chapters relating to the Finno-Ugric peoples and languages as the linguistic evidence is few and far between, and the chapters consist mainly of archaeological name-dropping. The Basque substratum in Saami mentioned on p. 349 has been rejected by Ante and Aslak Aikio in Kaltio.[3]

Conclusion

W seems to have ignored all criticisms since he repeats mistakes from previous work that have been pointed out to him by other linguists. The mistakes that W has constantly repeated include the Greek ghost form *bait*

[3] Only in the online version: www.kaltio.fi

(glossed as a cognate with Gothic *paida* 'Leibrock', f. ō-stem) - no such form exists. The correct form is *baítē* (Doric *baíta*) 'shepherd's or peasant's coat (made from goat skin)'. Latin *kardía* is another mistake that has been around in several of W's publications, let us now then set the record straight: the Latin form is *cor*, *cordis* 'heart', and *kardía* 'id.' is Greek.

Some of the fonts did not print correctly in the book. For example Lithuanian <□> comes out as <˜>, schwas (?) have dropped out in the entire book (e.g. in pp. 170) and so on. Also numerous misprints and incorrect capital letters mar the book. The publishers have to be commended, however, for the layout of the book: it is eminently readable. The print quality of the maps and figures is very high. The book is also very well written.

I have given a very critical account of W's book. I feel that the linguistic community should be made aware of the numerous errors and erroneous interpretations found within. The sad part is that W has not learnt his lesson since his errors have been corrected in numerous fora by experts in their respective fields: Petri Kallio, Jorma Koivulehto, and Asko Parpola in *Tieteessä tapahtuu*; Ante and Aslak Aikio in Kaltio; Johanna Laakso, Cornelius Hasselblatt etc. They have all shown where the weaknesses of W's model lie, be it in general principles of linguistic science or in minute philological details. It is unfortunate that there are still linguists who feel persuaded by W's argumentation, despite the fact that all experts in the relevant fields have rejected W's ideas.

W's alternative "school of thought" is nothing but a will-o'-the-wisp. Referring to "schools of thought" as means to disregard the *communis opinio* is ascientific. W's careless and rash use of the linguistic terminology and his complete lack of evidence for his hypotheses deem this book unusable. If W's knowledge of his own field, linguistics, is of such a low standard, one may only wonder what the level of his archaeological and genetic knowledge is.

There are a number of excellent overviews of the study of Indo-European and Uralic linguistics, of European prehistory, and of IE and U homeland problems. The interested reader is better-served to turn to those than to this book.

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Contact information:

Santeri Palviainen
Dept. of Linguistics
Harvard University
Boylston Hall, 3rd floor
02138 Cambridge, MA
United States of America
palviain@fas.harvard.edu