A rejoinder to A. Liberman’s review of *Old Frisian Etymological Dictionary*

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Anatoly Liberman (University of Minnesota; henceforth Lib.) gives here a rather extensive review of Boutkan-Siebinga, *Old Frisian Etymological Dictionary (OFED)*. Since Boutkan died several years ago, the editors of the journal have asked me as his former teacher to give comments on Lib.’s criticism.

Boutkan’s book was conceived as a contribution to a new etymological dictionary of Indo-European, a huge project which is being executed in Leiden. This explains the wealth of Germanic material, especially in the second part of the book. Boutkan started from the evidence of the first Riustring manuscript and hoped to extend the selection later.

Boutkan worked in a database format, so that every lemma had to contain the field “Origin” (PIE, Gmc., etc.). Generally, the given labels are well-substantiated (thus Boutkan provides ample considerations why *mar* is labeled “PIE [?]”), although of course one may sometimes evaluate the material differently. I agree with Lib., for instance, that *folgia* may have been “PIE [?]” and not “Gmc.”. Lib. concludes that the qualification is difficult, and that the reader should rather go by his own judgment.

It is regrettable that Boutkan has not introduced a label SUB (= ‘substrate’). Thus he notes (p. xviii) that “Gmc.” denotes substratum words and PIE words, while recognizing this difference is one of the main aims of the book. (See further below.) Therefore, one must read his comments carefully.

“The labeling ...breaks down completely when it comes to compounds” (Lib.). Boutkan assigns the label of the second element to the whole. This is an option, but I agree that it allows no conclusion whatsoever about the date (origin) of the compound. I further agree with Lib. that *wifstrewene* and *widuben* are probably only of Old Frisian date. In any case, the label is unfortunate, incomplete.

The references Boutkan gives are called by Lib. “unsystematic, sometimes counterproductive, and insufficient”. On the whole, this judgment is too harsh, in my opinion, although I agree that in the lemma *wifstrewene* it should have been mentioned that Krogmann rejected the interpretation adopted by Boutkan.

Sometimes I do not understand what Lib. precisely means. So he says: “Boutkan ends up knowing more about the protolanguage than about Old Frisian.” I take it that Lib. does not agree with the basic concepts of the Leiden school (he mentions “three laryngeals and neither /a/ nor initial /b/”). These are indeed starting points of the Leiden school, but they are widely accepted among Indo-European scholars.

In the remainder of his review Lib. pays much attention to the supposed substrate elements. He starts with saying that the number of substrate loans is (according to Boutkan) extremely high; he calculates that 80% of the Gmc. words are supposedly of substrate origin. Lib. then points out that “new words can be coined by the hundreds” (p. 8). If I understand the author well, he suggests that languages make wholly new words in great numbers. This, in my view, is simply not true. A language does not make new words. New words are either new derivatives (from existing words) or loanwords (from other languages). This can be easily understood. New derivations are of course a well known source for new words; and words from other languages are always available, especially from (adjacent) substratum languages. It is
always easier to take a ready-made word and adopt it than to make a new one. In the case of a
substrate, i.e. an indigenous language, there are at once thousands of words available, and the
advantage is that a great part of the population already knows the word. We also know well
enough that languages have no difficulty in adopting foreign words.
Lib. suggests that slang words originated as such spontaneous creations, but he admits
that he cannot demonstrate this. I think that words based on exclamations etc. would come
closest to what Lib. has in mind, but these are rather rare and form only a very small minority of
the vocabulary.
Lib. gets excited about the subject. Cf. (p. 9): “Why did the Proto-Indo-Europeans take
over unpronounceable vowels and consonants and acquire new speech habits? Judging by their
propensity to learn words and sounds from languages vastly different from their own, they must
have found “foreignisms” appealing ...We obtain a picture of conquerors who retained their
language but borrowed numerous phonological elements from the conquered and adopted alien
speech habits (...). they borrowed words ... right and left.... This picture is not realistic.” I totally
disagree with Lib. and think that what he describes is exactly what happened. A language has no
difficulty in adopting words from another language. One has to realize that a relatively small
group of people arrived in a foreign country with different, unknown plants, types of landscape,
instrumets, weights, games; in a country with a developed civilization where the much more
numerous population spoke another language the whole day, at every occasion, in the streets, on
the market.
The last three or four years I have been studying the substrate language of Greek (the so-
called ‘Pre-Greek’). This language had a quite deviating sound system from Indo-European, and
what you see is that Greek exhibits variants with a different rendering of these sounds; later the
variants were mostly eliminated so that one form remained (e.g. ophthalmós ‘eye’). Often
scholars are reluctant to accept substrate influence; this is very clear in the case of Greek. My
promotor, F.B.J. Kuiper, could not understand this reluctance (he supervised the magnificent
dissertation of Furnée on Pre-Greek, the book which was unjustly rejected by many scholars).
One reason for this is of course that we mostly cannot advance if no texts in the substrate
language are available. Research stops with the conclusion that a word comes from the substrate.
Further, substrate influence can often be assumed, but there is little or no evidence to check the
assumption, so that the argument involving substrate elements is often non-committal. All this is
true, but we must try to recognize substrate elements and analyze them, and try to figure out
what their influence may have been; there is no excuse to disregard them. Therefore I am glad
that the young scholar, who had a good eye for the substrate elements, gave us this book.
Lib. gives eight pages (pp. 10 - 19) with supposed substrate words and adds Boutkan’s
argumentation, to which he presumably does not object. Lib. concludes (p. 20): “among
the substrate words in OFED we find such as designate the most basic actions: help, bring, rise,
speak, need, etc. Very little human anatomy proved to be native [i.e. Indo-European]; borrowings
were presumably needed for the names of the beard, head, hand, hair, stomach, blood and flesh;
at least one word for the lip (lippa) is also said to be a borrowing. To the list above ben ‘bone,
leg’ can be added. It has no Indo-European etymology, and I do not know why the idea of a
substrate was not invoked in the entry. ... Even such foundational words as irthe, -lende and
himil (‘earth; land; heaven’) are presumed borrowings. If Boutkan’s findings in the area of social
relations are trustworthy, the conclusion is inevitable that, as far as the organization of society is
concerned, the Indo-European conquerors were in every respect inferior to the peoples they
subjugated, for they either did not have the institutions for which they borrowed new words [or
they had slightly different meanings] or considered native words inadequate. Here we face
substrate names for ‘people’, ‘lord’, some administrative units, ‘assembly’, ‘oath’, ‘pledge’, ‘servant’ ... Helich ‘holy’ and god were, it appears, also imported.” This discussion shows that we can draw important conclusions on the basis of (supposed) substrate words. These conclusions do not surprise me at all; rather it is exactly what I have expected.

I further noted the following remark. “Some reviewers assert that a Germanic etymologist of our time cannot ignore laryngeals (p. 21).” Though I am prepared to agree that for Germanic itself the laryngeals do not help much, the laryngeals are an established fact of Indo-European phonology and they can be relevant for deciding on cognate forms; so they must be recognized in Germanic as well.

On p. 22f. Lib. states that “Boutkan’s opinions, to the extent that they go beyond Frisian, are seldom worth considering.” As an example he ridicules Boutkan’s connection of the word for ‘honey’ with that for ‘bee’ (the etymology is Hilmarsson’s). I do not understand this; it is well known that the two words can have a common (etymological) element; cf. Greek méli ‘honey’ and mélissa ‘bee’ (in Vedic, gáv- means both ‘cow’ and ‘milk’).

On p. 23, Lib. wants more precision in the matter of phonetics and gives as an example that Boutkan ascribes the spellings auld, aud for ald, old to the velarizing effect of the l. I do not understand Lib.’s criticism; it is no doubt the character of the l which is responsible for it, as it is responsible for the development of l to u in Dutch (e.g. koud beside German kalt).

Lib. ends with some lemmata “in which he [Boutkan’s work] can be seen to advantage” (p. 23-25) and on pp. 25-27 he adds some remarks on individual words.

In conclusion (p. 21), Lib. states that Boutkan “died too early and was not equal to the task he undertook”. Boutkan stood of course at the beginning of his career, but he produced a wonderful and much-needed book. As far as the substrate elements of Germanic are concerned I think that Boutkan was essentially right, and that his book once for all demonstrates how much substrate is contained in Germanic.