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Table of Contents / Содержание

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New trends in European studies on the Altaic problem

The paper discusses several general problems of present-day historical Altaistics, taking as a reference point the critical evaluation of two large monographs by Martine Robbeets — one on the Altaic origins of the Japanese language (Robbeets, Martine. 2005. *Is Japanese related to Korean, Tungusic, Mongolic and Turkic?* Wiesbaden: Harrassowitz) and another on the evidence that comparative verbal morphology provides to validate the Altaic hypothesis (Robbeets, Martine. 2015. *Diachrony of verb morphology: Japanese and the Transeurasian languages*. Berlin: Mouton de Gruyter). Along with the analysis of the main methodological principles and some specific etymological decisions taken by the author, the paper also focuses on the critical discussion of certain assumptions that may be seen as typical of "anti-Altaic" researchers.

Keywords: Altaic languages, historical Turkology, verbal morphology, long-range comparison, history of the Japanese language, etymology.

After the *Etymological Dictionary of Altaic Languages* (EDAL) came out in 2003, accompanied by both positive (Blažek 2005; Miller 2004) and sharply negative (Vovin 2005; Stachowski 2005; Norman 2009; Georg 2004) reviews, it seems logical that the next step, instead of a *Sturm und Drang*-style gathering of additional material to confirm the updated reconstruction, should rather be a verification and cleanup of the already accomplished work. The author of these lines remains fully convinced (in fact, has always been convinced) that the first collection of Altaic etymologies with claims to a certain degree of completeness, published by S. Starostin, A. Dybo, and O. Mudrak more than ten years ago, should have not been called *Etymological Dictionary of Altaic Languages*, but rather something like *Versuch eines etymologischen Wörterbuchs der altaischen Sprachen* (the English language, to which etymologists are less accustomed, is possibly to blame; *An Attempt* or *An Essay* as the beginning of an English title is clearly better fit for a review of a voluminous dictionary than for the dictionary itself). Thus, as of this moment, reconstructions for both Proto-Turkic and Proto-Tungusic have been significantly corrected, and it is of crucial importance to incorporate these corrections within the general Altaic comparison. Likewise, it is also very important to include recent modifications done on Japonic (Japanese-Ryukyuan) and Koreanic reconstruction (of course, not without some critical reflections on such works as Miyake 2003; Bentley 2008; Vovin 2005–2009). And, obviously, in the light of the need for such updates, it is only natural that our attention should be drawn to two interconnected works by Martine Irma Robbeets, one of which came out very soon after the original publication of EDAL, and another one appeared only very recently. As a disciple of Sergei Starostin, on one hand, and a student of the Leiden school of comparative linguistics, on the other hand, M. Robbeets is trying to further develop Altaic studies after EDAL, bringing them to the general attention of European scholars and trying to overcome the mistrust with which "Altaic" is generally viewed today in Western scholarly communities, so that her work deserves serious attention (and critical evaluation) on the part of both anti- and pro-Altaicists.

The first of the books by Robbeets (2005: *Is Japanese related to Korean, Tungusic, Mongolic and Turkic?*) has already been reviewed twice, in Georg 2009 and Vovin 2009.[1] Georg's criticism of Robbeets 2005 often seems unfair and unrelated to the essence of the matter. A detailed disassembling of his arraignments would serve no purpose, since, in a significant number of cases, they are reduced to "juggling" the evidence — there is a clear impression that a certain presumptive ill will did not allow him to properly understand the text (as an example, consider this case: on p. 26, he exclaims, "…how a mention of Old Turkic (p. 17) can manage to only talk about the Orkhon inscriptions, sweeping the bulk of the Old Turkic literature under the rug, is incomprehensible…" — meanwhile, on p. 17 Robbeets simply writes that the earliest Turkic monuments are the Orkhon inscriptions; likewise, in the periodization table of the Turkic languages she correctly points out that the "ancient Turkic period" is the period of the pre-Mongol invasion texts). Therefore, I will only address here some of his more fundamental quibbles.

For one thing, Georg writes that Robbeets is wrong to not have examined G. Doerfer's reconstruction of Proto-Tungusic vowels, for the following reason:

> …both Proto-Tungusic and Proto-Mongolic had the pairs **ü* and **ö*. "Cognates" between both families show a surprisingly blurred picture of correspondences for these phonemes which can be remedied by the assumption that a great number of these (regarded as "old inheritance" by the Altaicists) were borrowed from Mongolic into Tungusic at a time when the original contrast had already been restructured (**ü* > **i* in Northern Tungusic, **ö* > **o*). It is not expected that a work like Robbeets' would accept this at face value, but that it is discussed, or at least known, is well within the range of what can be expected here.[2]

But in fact, there are virtually no pairs of Mongolic-Tungusic cognates in the Altaic dictionary where Mongolic would show *ü* and Northern Tungusic would have *i* (PTM **ü*) in the Altaic dictionary — if Georg tried to find a confirmation for his idea, he would have been seriously disappointed. Looking all over the database, I was able to find exactly zero cases of such a correlation in the case of PTM **i* (that is, where there are no reflexes in South Tung. or Manchurian, only in North Tung.) and only four cases of PMo **ü* : PTM **ü* which, as far as the phonetic shape of the morphemes is concerned, could probably be explained by borrowing, but at least in the first two of these cases, such an explanation would not agree with the semantics of Mongolic and Tungusic words, namely:

- PMo **üli-* 'to compare', *üliger* 'shape, form, model, story' (Mong. > Evk. *ulgur* 'tale, story' etc., see Doerfer MT 48) || PTM **ül*(*k*)*e-* 'to measure; to understand';

[1] A third review is Kara 2007. It is written in a more amicable and conservative manner and contains a number of specific corrections to etymologies, which should be accepted at least partially, even despite some visible inexperience of this first-rate specialist in Mongolian studies when it comes to applying the comparative-historical method. We have also a negative but not very informative review from Knüppel 2006 (containing no examples; cf. also Knüppel 2013 with similarly uninformative comments on Blažek 2007), and two positive reviews with minor corrections from Blažek (2007) and Miller (2007).

[2] In connection with this, the following quote from Vovin 1995 seems relevant: "The reconstruction of Proto-Tungusic **ö* (Benzing 1955, Doerfer 1978) is highly questionable: it seems to be based mostly on the Even vowel *ө*, used as a transcription sign, for example in Tsintsius 1975–77, which is in fact a back vowel, not a front one, as explicitely stated in Tsintsius (1947: 17) and Novikova (1960: 48), the two most comprehensive grammars of the Ewen language". Although the general doubt on the validity of Doerfer's reconstruction is legitimate, the actual reasoning is transparently wrong: if a Tungusic sound is *phonetically* a back vowel, this does not necessarily mean that it is *morphophonologically* not a front vowel. Cf. examples in Dudkin 1995: 9: *нөригэ* [*nөrigө*] 'grayling', *көӈгэлэ* [*kөŋgөlө*] 'pit, ditch', with morphophonologically front *э* in non-initial syllables.

- PMo **küse-* ‘to wish’ // PTM **xüse, *xüse-gdi* ‘hunter, man, male’, **xüse-gē-* ‘to be anxious, worry about smth.’;
- PMo **sür-* / **sur-* ‘to cry out; to sound, make noise (of wind)’ || PTM **sür-* ‘to creak, screak; to shout, cry’;
- PMo **türej* ‘boot-top’ || PTM **türē-*(*kse*) ‘boot-top’.

In my doctoral thesis (Dybo 1992, partly published as Dybo 1996) this Proto-Tungusic entity is interpreted as the diphthong **üj* and confirmed by the existence of a back-vowel correlate **uj*. An Altaic origin, with parallels in Mong., is offered: Alt. **öj* > PTM **üj*:

- PMo **möri* ‘shoulder’ — PTM **müjre* id.,

but the original height is preserved in such examples as PMo **tölgü* ‘prediction’ — PTM **tolkin* ‘dream’ (< PAlt **ö*), PTk **dūp* ‘bottom, root’ — PTM **dübe* ‘end’ (< PAlt **ü*). Cf. additional examples in EDAL (where traditional **ü* stands in PTM for my **üj*):

- PAlt **túj̯pè* (~ *d-*) ‘hill, top’, PMo **dobu* / **döbe,* PTM **dū-* (~ **düb-*), PJa **(d)ípà*;
- PAlt **i̯úse* ‘to grow, sprout’, PTk **ös-,* PMo **ös-,* PTM **üse-;*
- PAlt **i̯ǔt‘e* ‘thick liquid’, PTk **ōt,* PMo **öte-,* PTM **üt-;*
- PAlt **k‘i̯ŭrpe* ‘young (animal, fish)’, PTk **körpe,* PMo **körbe,* PTM **xürbe*;
- PAlt **k‘i̯ùle* ‘to exchange, trade, hire’, PTk **kōle,* PMo **kölü-sü,* PTM **xül-;*
- PAlt **ŋǒjču* ‘thin, small’, PTk **ōču-,* PMo **öčü-,* PTM **ŋüši-* (**ŋujši-*);
- PAlt **p‘i̯ùgV* ‘to flay, cut’, PMo **(h)öɣe-le-,* PTM **püg-;*
- PAlt **si̯ùŋe* ‘hoar-frost’, PMo **söŋ,* PTM **süŋü-;*
- PAlt **tǒj-* ‘four’, PTk **dōrt,* PMo **dör-ben*, **dö-čin,* PTM **dügin.*

For another thing, regarding the Turkic issue of zetacism / rhotacism and its importance for the Altaic hypothesis, Georg is trying to be much more radical than his predecessor Doerfer (who — in my opinion, quite correctly — used to state that “Z/S plays no role!” (Doerfer 1988). Actually, his main argument here is a reference to Georg 2003: 436, which examined an alleged case of **z* to *r* development in Chuvash *pir* ‘linen’ (cf. Common Turkic *böz* < Arabic *bazz*), but this example should be rejected: even if, ultimately, this is indeed a borrowing into Chuvash from Arabic, it still cannot be a reflex of Turkic **böz* because the vowels do not match — see SIGTYa 2006: 173–179 (Chuvash *erne* ‘Friday, week’, also cited there, generally reflects another consonant, namely, Common Turkic *-δ-*, see Fedotov 1996, 2: 480–481).

Another unjust criticism of Robbeets is encountered on p. 54:

> Regular correspondences for initial CVC sequences: we have not mentioned this before, but, frankly, the present reviewer has never seen anybody in historical linguistics explicitly defend a principle which confines the area where sound correspondences are to be sought to the initial CVC part of words and which, consequently, would represent carte blanche for ignoring any other part of any word involved in any “etymology” entirely as uninteresting. In fact, such a “principle” can only be interpreted as an attempt to lower the standards which etymologies have to pass before acceptance and, thus, to ease the task of justifying the proud “Yes” on which the whole edifice of this book was palpably erected in the first place.

In fact, however, Robbeets is entirely correct, since the “initial CVC part” in application to the Altaic languages should in all likelihood be considered a “root”. These languages are not prefixal, and their roots tend not to be lengthy, so the segmentation rests upon a reasonably

logical foundation. In Indo-European linguistics, for that matter, this principle was almost certainly taken as a basis (cf. Benveniste's "root determinatives"), and there is no reason why at least for the initial phase of work on Altaic etymology it could not be accepted, based on somewhat similar grounds.

This list of observations could easily be enlarged, but on the whole, all these anti-Altaic arguments can be summarized by referring to a Russian one: *на всякий чих не наздравствуешься* ("you won't have enough 'bless you'-s for every sneeze"). It is certainly true that anti-Altaicist comments on *specific* etymologies often contain valid points (although ultimately, my personal experience of evaluating them does not significantly decrease the total number of possible Altaic cognates). However, *general* complaints on various aspects of the reconstruction, such as the referral to Doerfer's views on PTM vocalism cited above, or the assumption that word-initial opposition of voiceless and voiced plosives in Proto-Oghuz is secondary (below I will refer to my own analysis; for dentals, see Dybo 2007 and a shorter version in Dybo, Starostin 2007), almost always turn out to be based on some systemic methodological error.

Concerning the review of Vovin (2009), I prefer to refrain from discussing it altogether. It is even more misrepresenting of Robbeets' achievements than Georg's, and, furthermore, is written in an unacceptable style, one that brings to mind the beginning of Vovin's own reply (Vovin 1995) to Karl Krippes' review of S. Starostin 1991 ("First of all, the tone of review can hardly be called academic. It rather reminds me a bazaar discourse with statements like 'data which he allegedly collected', 'Starostin did not have a good idea' etc.").

Instead, let us proceed to the actual discussion of Robbeets' work itself, beginning with the first monograph (2005). Although more than ten years old, this is where she provides her own system of phonetic correspondences, without which, of course, no further talk on language relationship is possible; the same analysis, for the most part, provides the foundation for the second, more recent, study.

As stated in the preface, the ideological position of the author seems to be almost perfect (including, among other things, her views on the problem of mixed languages, as well as the call for joint efforts between specialists in different languages). Naturally, archaeological (not to mention geographical) evidence can neither prove the relationship of the Altaic languages nor disprove it: language relationship is a purely linguistic concept, proven by purely linguistic methods — and it must be noted that (despite Georg's and Vovin's reproaches) Robbeets does not actually claim that she provides such a strict proof, but she does make it clear that, considering all available evidence, assumption of a Turkic-Japanese (or Tungusic-Japanese, etc.) relationship is more likely than, say, the assumption of a Turkic-Mayan (or Tungusic-Mayan) relationship.

I can agree with the critics of Robbeets that her trust in the ability of genetic and archaeological data to prove or disprove something in the field of language history is clearly excessive. To begin with, archaeological evidence is always incomplete (one cannot be sure that even within such a relatively small and well-explored territory as Japan every archaeological site has been discovered and excavated); the criteria according to which several archaeological findings are grouped together as parts of one archaeological culture are not always explicable and often quite impressionistic; finally, the parameters of ethnic/linguistic identification of archaeological cultures remain rather poorly developed (see, e. g., Roberts & Vander Linden 2011). As for genetics, it is well known that one should never *expect* any direct correlation between genes and language (as an example, cf. two cases when the correlation between genes, language, and geography is directly inverse, described in Balanovsky et al. 2011; Kushniarevich et al. 2015). Situations when genetic and archaeological data are in solid agreement within the framework of a simple historical / linguistic scenario are essentially due to random luck.

Consequently, the circuit scheme on p. 39 ("a working model for the relationship between Japanese, Korean and Tungusic") raises some questions — above all, concerning such entities as "Macro-Tungusic" and "South-Tungusic", denoted as the consecutive ancestors of Koguryo and Paekche. It cannot be assumed that archaeologists writing about the "South Tungusic" migration to the Korean peninsula have anything specifically linguistic in mind — just as archaeologists studying data on Western Siberia care very little about linguistic accuracy when writing about the "Ugric-Samoyed" cultures or about settlements of "Ugric-Samoyeds", a unity which never existed in the linguistic sense (cf. Borodovskiy 2001 etc.).

In regard to research methodology as it is explained by the author, I have an important remark on the warning against relying on the so-called "nursery words". It seems that assignment of any given word to that category should not automatically lead to its complete exclusion from comparison and reconstruction (see G. Starostin 2009: on a desirable diachronic approach to these phenomena); rather, one should keep in mind that this lexical group is subject to frequent borrowings from a special type of "infant-adult pidgin". The same approach is valid for cases of onomatopoeia, since different languages may have different secondary mechanisms of vocabulary onomatopoeization (see, e. g., Dybo 2004).

The assumption about phonological universals in pronominal morphemes, proposed by J. Nichols (1992: 261–62, 266–67; also referred to in Nichols 2014), is referred to in a neutral tone, even if the notion is transparently absurd (and the same applies to case markers as well). In synthetic languages grammatical markers usually have a simpler phonetic structure than stems, and a system of, for instance, monosyllabic CV-type markers is much easier to perceive as subject to phonological universals, just because the number of such combinations is automatically more limited than, e. g., CVCV. The point that Northwest Caucasian languages, despite their phonetic complexity, prefer short and simple phonemic sequences like *sa-*, *wa-*, *da-* for their pronouns, is well made, but much more important is the fact that the only other language family in Eurasia that shares a phonetically similar pronominal system is Northeast Caucasian — an argument that agrees far better with a scenario of their genetic relationship than with anything that has to do with a mystical system of "phonological universals". The same principle of observing systemic isomorphism in pronominal systems should naturally work for Altaic as well.

Turning now to issues of phonetic reconstruction, it must first of all be noted that Robbeets' discussion of the Proto-Japanese reconstruction shows that this field, unfortunately, still has not properly progressed from a methodologically primitive stage. Thus, reconstruction of a special series of Proto-Japanese voiced consonants (discussed on p. 53–54) is essentially based on Japanese "doublets", i. e. words that have similar meanings but differ phonetically by the presence or absence of a certain consonant in word-initial position. However, if there are no attempts to find at least some sort of complementary phonetic distribution of these variants (and, apparently, there is no such distribution), such a reconstruction is methodologically impossible; in my opinion, the author discusses this issue with unjustified seriousness.

Concerning the origin of the so-called "triangle sound" in Middle Korean from lenition: despite the fact that, as the author notes correctly, there is a significant number of exceptions to the lenition rule, her rejection of traditional solutions that interpret the sound as a palatal nasal **ń* looks strange. "Internal reconstruction", argued for in the works of S. Martin, is naturally important, but it can hardly be satisfactory with such a high percentage of unexplained exceptions. Verner's law, which Martin (1996: 58) mentions as an example of the importance of priority of internal over external reconstruction, is convenient precisely because it managed to explain multiple exceptions to older rules; in contrast, the "lenition law" has only served to multiply the number of such exceptions.

It does not seem productive to assume that all cases where some of the experts hold different views on some problem should automatically be considered uncertain and the respective material should be excluded from analysis — a somewhat apprentice-like approach, in my opinion. It is true that historical materials on Japanese and Korean are quite complex for an outsider; on the other hand, application of the comparative-historical method within these groups by Koreanists and Japanologists has often remained (at least, until recently) questionable. Sometimes, these works do not show a clear understanding of the difference between the data of a written monument and a reconstruction; sometimes, no adherence to the basic principle that phonetic laws should not have unexplained exceptions. And internal reconstruction, while an important addition to the method as a whole, should always be treated with caution — one should always remember that the deeper it is, the less material is there to confirm the rule and, consequently, the less reliable are the results that you end up with. This reliability may be enhanced if external check over a number of equiprobable alternatives is introduced, making external comparison a logical (and sometimes necessary) wrap-up for internal reconstruction. Overall, conflicting expert judgments on complex situations should necessarily be compared in terms of strength of their argumentation, before the situation is relegated to the unreliable "gray area" of the reconstruction.

The Tungusic reconstruction is presented with some inaccuracies. Most significantly, it is not true that V. Tsintsius really reconstructed palatal consonants (*m'*, *b'*, *s'*, *t'*, etc.), but did not reconstruct diphthongs (p. 68). Tsintsius did reconstruct diphthongs; and cf. Tsintsius 1949: 210–214, where it is only stated that consonants in the palatal affricate series — *č*, *ǯ* (also *n'*, *j*) — in some Tungusic dialects are, instead of affricates, realized as palatal explosives — *t'*, *d'*. Apparently, the author was misled by Benzing's statement (Benzing 1955: 40) that, following Tsintsius' logic in reconstructing palatal **n'*, it would also be possible to reconstruct other consonants as palatalized ("…aber dann müßte man wohl auch **b'*- (tg. **b'āga* ~ **biāga* 'Mond'), **g'*- (tg. **g'ā* ~ **giā* 'Gefaerte'), **s'*- (tg. **s'ā*- ~ **siā*- 'kauen'), **m'* … ansetzen"). Furthermore, Benzing indicates that the data are too scarce to agree upon a final decision; but today, with the publication of SSTMYa and other Tungusic dictionaries, this problem has largely been remedied, and it is now clear that regular reflexes of vocalic diphthongs do not allow us to interpret all cases of palatal **n'* as secondary developments before an old diphthong or **i*.

Regarding the RTR-harmony in Proto-Tungusic see below (p. 96 of this paper).

Robbeets' Mongolic reconstruction is taken directly from N. Poppe, without taking into consideration any amendments, including without distinction **-γ-* (or **-h-; > 0* in modern Mongolic languages) and **-g-* (> *-γ-/-g-* in modern Mongolic languages) and without account for Vladimirtsov's rule (**-γ-* > *-γ-/-g-* if a form contains another **-γ-* or a diphtongue). It should be noted that distinction between these two phonological entities, well reflected in the spelling of Sino-Mongolian documents, is now customary to Mongolian studies (cf. Janhunen 2003). The decision to reconstruct **p-* in Proto-Mongolic is a simplification; contrary to the opinion that S. Georg defended in his review, and in accordance with H. Nugteren (2013), we can be sure that actual Mongolic data calls for the reconstruction of **h-* rather than **p-;* the development from **h-* to *f-* (as well as to the palatal fricative) in South Mongolic languages is clearly due to vocalic context, cf.:

- **hunïn* 'smoke' > Middle Mongol *hunin,* Dagur *xɔny*, Monguor *funi* / *χuni*, Bao'an *fune* / *honɛ*, Dongxiang *funi*. (Nugteren 2013: 364).
- **hinie-* 'to laugh' > Dagur *xinəːd-,* Monguor *śine-*, Bao'an *śine-* / *xine-,* Dongxiang *śinie-*. (Nugteren 2013: 357).

- **hargal* ‘dung; dried cow dung (used as fuel)’ > Middle Mongol *harɣal,* Dagur *xarɣəl,* Eastern Yugur *harǧal,* Monguor *xarġal* / *χarġar*; Bao’an *χalġa,* Dongxiang *haŋġa*. (Nugteren 2013: 350).

Contrary to the author’s statement, Poppe’s reconstruction of PMo **β* (Poppe 1955: 99) is not due exclusively to external comparisons — it is primarily based on the alternation *b* ~ *ɣ*, observed in certain Mongolian stems. Finally, the description of Mongolian vowel harmony, for some reason, does not mention any interpretations of it in the spirit of RTR (see, for instance, Svantesson 1985).

The PTk table contains some transparent mistakes. If this is a zetacist reconstruction, then where is **l_2*? Johanson 1998 (as a source of data) is not to blame, because on p. 95 he does not present a complete table — his reconstructions are in the text itself (where, incidentally, there is also **h-*, reconstructed based on Khalaj data, but not mentioned in the table, even though it is discussed on the same page of the book under review); however, on pp. 104–105 he explicitly interprets the respective prototypes as r^i and l^i. [The explication of **l_2* < **-lC-* in Proto-Turkic, with a reference to Street 1980: 78–79, is not valid: **l_2* (~ **š*) is an entity reconstructed based on correspondences between Common Turkic and Chuvash, with **-lC-* as a probable Altaic prototype for it (the presumed **-lč-* is reflected in Chuvash as *-ś-*, which is a common reflex for Turkic **č* and not for **l_2*/**š*)]. The discussion of zetacism/rhotacism is presented in an oversimplified manner, without, for instance, any mention of “Helimski’s rule” (see Helimski 1986 a, b, Dybo 1995a) and O. Mudrak’s observations on the correspondences between Chuvash and Common Turkic (see, e. g., SIGTYa 2006: 27–40). Incidentally, Salar is by no means “a dialect of Uigur” (p. 75), since it is really an Oghuz language.

Concerning the problematic issue of whether initial voiced stops are to be reconstructed for Proto-Turkic, Robbeets resorts to the completely improper principle of “majority wins”, for which she has been repeatedly blamed by Georg, and in this case I am forced to agree with his criticism. Such a principle simply does not exist in comparative linguistics: if 20 languages that belong to a certain family do not show a particular opposition, but the 21st does, it has to be reconstructed for the protolanguage — unless one is able to formulate a special rule of secondary positional distribution.

Robbeets’ stance on this issue is mainly dependent on G. Doerfer’s works, in which he opposed the separate reconstruction of voiced stops because of considerable variation in reflexes as well as the innovative nature of Oghuz voicing, since, presumably, Persian borrowings into Oghuz languages had also undergone such voicing. Concerning the first argument (variation in reflexes), it would have been advisable to become acquainted with the analysis of Oghuz reflexes in Dybo 2005, as well as in SIGTYa 2002: 68–72, where it was shown that the alleged variation is actually much more rare than advertised, and that most of such cases can be regularly explained. (Incidentally, even some of Robbeets’ own examples of variations contain important omissions: thus, she writes “we find Tk *göm-* and Tkm. *göm-* ‘dig’ with a voiced reflex, while Azerbaijani *köm-* ‘dig’ has a voiceless reflex”, but cf. Azeri *gömmək* (rude, colloq.) ‘to dig earth’ in ARS 2006: 2, p. 275). As to voiced consonants in borrowings, here Robbeets cites, for example, Doerfer TMN: 3, 616, where it is supposed that PTk **gāne* ‘tick’ is a borrowing into Proto-Oghuz from Persian *kana* id.; however, in the same book Doerfer cites a Classical Persian text from the 16th century, where it is said that *qurād* (Arabic ‘ticks’) in Turkic are called *kene*, and notes that this citation speaks in favor of a Turkic origin of the word. In general, Doerfer’s conjectures about Iranisms in Turkic should be taken with a dose of caution: the origins of the Persian language are well studied, and a large number of Persian words have reliable etymologies that usually make it evident if the word is original or borrowed. In order to

state with confidence that a certain Turkic word was borrowed from Persian, it would be required to find a suitable Iranian etymology for this word. However, etymologization of 'tick' on Iranian grounds is somewhat problematic.

Thus, J. Edelman (ESIYa: 4, 208) decisively lists the Persian word among the derivates of the Proto-Iranian verb **kan-* 'to dig':

> The words for 'tick', 'mosquito' and other biting, penetrating, and clinging insects can be classified here (with the patterns **kana-ka-, *kana-ci-* etc.): Classic Persian *kana*, Tajik *kana* 'tick, bedbug' (< **kana-ka-*), Pashto *kunáy* (masc.) 'tick (on dogs, sheep)' (< **kana-ka*), but Pashto *kana* 'tick' — borrowed; Shugni *čangin* 'fly'; Sarikoli *kawa*, Yazgulami *kenj* 1) 'moth', 2) 'flour moth' < **kana-či-;* Wakhi *kəkʊ́ng* 'mosquito' — on the cognation of these words with Tajik *kana* see [ESWYa: 215]. Wakhi *kwənd* 'tick' also belongs here [ESWYa: 214]".

Steblin-Kamensky, in his *Etymological Dictionary of Wakhi Language* (ESWYa: 214) sticks to the same opinion:

> [Wakhi] *kwənd* 'tick'; Wakhi-Tajik *xamandák, kana*. Morgenstierne compares with Pashto *kunáy* 'tick (on dogs, sheep)', *kōn* 'big tick (on dogs, large cattle)' (IIFL II 527; EVP 1927: 33), Ashkun *kõ̃w*, Kati *kõ̃*, Bashgali *kõ̃* 'tick', 'louse' < Old Indian *kuṇa-* (CDIAL 3255); cf. also Wakhi *xəmənḍák* 'ovine tick', Sanglechi xam*áṇḍək* 'tick', Badakhshani, Kabuli *xamanduk*, Persian, Tajik *kana* 'tick' (Turk.? — Doerfer TMN III 1653). Buddruss 277: to Persian *káv-idan* 'to dig, excavate'.

Indeed, in Mgst. IIFL: II, 527 we find: "[Wakhi] *ku'wɛnd* L sheep tick. — Cf. Psht. *kūnai* (EVP, s.v. *kōṇ*)". But it is evident that if we stick to the comparison of Wakhi and Pashto words, they cannot be considered as a match for the Persian word because of phonetic reasons. In the old edition of the *Etymological Dictionary of Pashto* (Mgst. EVP 1927 : 33) we find: "*kōṇ, kūnai* 'a large species of tick or louse, infesting dogs and cattle'.— B. *kōṇyā'k.* — Etym. unknown. Cf. Ashkun *kõ̃w* id., Kati *kọ̃*". The reissue of the dictionary, edited by J. Elfenbein, D. N. MacKenzie and N. Sims-Williams, directly states: "*koṇ m.* a large tick infesting animals. The *-ṇ*, requiring old **-r(V)n* or **-shn-*, rules out connection with Prs. *kana*" (Mgst. EVP 2003: 23). On the other hand, we know of the Sanskrit word *kuṇa* 'a kind of insect living in clothes' (Monier-Williams: 289), clearly related to the Dardic words mentioned by Morgenstierne.

Thus, Persian *kana* 'tick' may theoretically be a verbal derivative, yet the word is properly recorded only in New Persian, with no earlier fixations. East Iranian forms that have been compared with it cannot be judged as proper etymological cognates — they either correspond to Skr. *kuṇa*, or represent borrowings from its Dardic relatives. However, all the problems connected with the tentative Iranian etymologization of the Persian word can be resolved by assuming instead that it is really a Turkism.

I have analyzed all three examples that Doerfer cites for the alleged voicing of **t-* in Persian borrowings in Proto-Oghuz in the introduction to Dybo 2007; here I will reproduce the same analysis for English readers.

In Doerfer 1971: 276 we find:

> … the development *t- > D- > d* surely is secondary in Oghuz… The above progression can be shown, among other things, by the development of old loanwords in Oghuz; e. g., Indian *tōbra* 'bag' (Turner 5972) became P. *tōbra*, the modern Osmanlı *dorbacık* (TM headword 947). Here *d-* is apparently secondary. The same holds true for P. *taġār* 'vessel' > Osmanlı *dağarcık* (TM headword № 905).

The weakness of such a conclusion from the comparative point of view is quite transparent; moreover, not being a competent etymologist of Iranian material (even brilliant knowledge of Classical Persian is not sufficient for this purpose), G. Doerfer finds himself helpless in determining such loanwords (which certainly could not be appreciated by his anti-Altaicist colleagues — specialists in individual language families that constitute Altaic). Thus, he mentions Pers. *tōbra* 'bag, bucket bag' (in early sources mainly 'horse bucket bag'), estimated as an unequivocal Turkism, e. g., by such an trustworthy etymologist of Iranian data as R. Tsabolov (see Tsabolov KES 2, 214) as an originally Iranian form. Curiously, however, in an earlier publication (Doerfer TMN: II, 594) Doerfer is much more cautious about the etymology of this word, speaking only of its Indo-Aryan connections, which can be confirmed by referring to Turner, № 5972. But it is important to note that Turner's dictionary was not conceived as a reconstruction of Indo-Aryan lexicon (only Mayrhofer IA comes close to actually realizing this idea, although, incidentally, these particular words are not found in that edition); rather, the idea was to represent the Indo-Aryan lexical data as completely as possible in a well-classified manner. Therefore, numerous Modern Indian innovations were included along with archaic forms, among which, undoubtedly, we find the data cited by Doerfer.

Under № 5972, in particular, we find a reconstructed Proto-Indo-Aryan form **tōba* 'bag' (a form certainly not attested in either Ancient or Middle Indian periods) with such Modern Indian reflexes as Bengali *to* 'pleat' and, supposedly a derivative noun with the diminutive suffix *-ḍa*, Lahnda *ṭoṛā* 'a bag hanged around a hand of bananas', Punjabi *toṛā* 'wallet', Kumaoni *toṛo* 'bag, especially for rupees', Bengali, Oriya, Hindi *toṛā* 'leather wallet' (> Nepali *toṛā*), Gujarati *toṛɔ*, Marathi *toḍā* 'bag'; Bengali *torā* 'wallet' (from Bihari?). In support of his hypothetical Indo-Aryan reconstruction, Turner cites:

a) under question: Skr. *ṭōpara-* 'little bag', which, first of all, does not phonetically correspond to Modern Indian forms, and, second, is an obviously late "Sanskritization" of a Modern Indian word whose only attestation is in the text of Dhūrtasamāgamana, a low-genre comedy ("Gigolo's Promenade"), written in the 14th — 15th centuries A. D., by which time Turkisms in Indo-Aryan were perfectly possible;

b) a hypothetical Proto-Iranian **tūbraka-*, reconstructed by himself and based on Persian *tōbra* and Pashto *tūbra* (where the Pashto form is a transparent borrowing from Persian), East Baluchi *thīraɣ* (borrowed into Brahui as *tūra*), Baxtiyari *turba*, Kurdish *tūrik.* These Iranian forms are clearly split into two groups: first, Persian *tōbra*, Baxtiyari *turba* and forms that could be borrowed from Persian, or directly from Turkic; second, East Baluchi *thīraɣ* and Kurdish *tūr*, *tūra* (m.) 'bag', 'tote bag' (*tūrik* — the form of the indirect case); in Tsabolov KES 2, 150 even more forms belonging to the same etymon are listed, such as Luri *tūra* 'bucket bag', 'pouch made of cloth', Semnani *tūra* 'bucket bag', 'pouch for pressing sesame oil', Lasgerdi, Sangisari *tūre*, Shamirzadi *türe* 'bucket bag', Sorxei *türî* 'bucket bag', 'pouch for straining (cottage) cheese'. It is fairly clear that this second group of forms goes back to Iranian (more precisely, Proto-West Iranian) **tūra*. Kurdish and Baluchi forms could be descended from **tūbra*, since in these languages *-b-* in clusters > *-w-* (Tsabolov OIF 91, 92, 81; Rastorgueva 1990: 184), but in other languages *-b-* would have been preserved, cf. the reflexes of Ir. **abra* 'cloud': Kurdish *awr*, Lasgerdi, Sangisari *abr*, Sorxei *obr*, Semnani *abr*, Baluchi West. *aur*, East. *haur* (Rastorgueva, Edelman: 2, 74).

For these reasons, regular etymological analysis does not allow to reliably reconstruct an Indo-Iranian entity in this case. Yet, astonishingly, Doerfer (TMN), referring to Turner's dictionary, even voices the idea that the Persian word may be derived from Indian *tōbra*, although Turner clearly states that, on the contrary, Punjabi and Hindi *tobra* (from which, in turn, are borrowed Bihari *tobṛā*, Gujarati *tobṛɔ*, Marathi *tobrā*) is a Persian loanword.

What is even more interesting is that in Proto-Oghuz this word may not have actually undergone voicing of the initial consonant. Turkic forms that are relevant for the reconstruction of the initial dental sound include: Turkish, Gagauz, Azeri *torba*, Turkmen *tōrba*; Old Osmanlı (since the 14th c.) *tobra, torva*, also *topra* in one text from the 15th c. (TS: 3, 3824–3826); PTk **tōrpa* or rather **tōpra* (rare cluster in a Turkic root that undergoes metathesis, cf. **topra-k* 'soil, dust', Western Yugur *durvaq*, Azeri *torpaG*, Khalaj *turpaq*; TMN: II, 592–6, VEWT: 490). Cf. also Hung. *turba* 'bucket bag', attested since 1528 (*thwrbam*); contrary to MNyTESz: III, 1002, hardly from Osmanlı *torba*, since the form had already been affected by the rule of transition from Old to Modern Hungarian, namely, *ō* borrowed as Old Hung. *ū* > mod. *u*; hardly of Cuman-Pecheneg origin (cf. CCum. *topra*, i. e. still without the voicing of *-p-*), but, perhaps, late Bulgar? If the Hung. word really originates from Danube Bulgar, then the original word is Proto-Turkic; otherwise, it should be considered Common Oghuz-Karluk-Kipchak. Khalaj *tọrba* (D–T: 207) is a borrowing from Azeri (cf. the absence of vowel length).

A possible Altaic etymology is Proto-Altaic **tʻòjrá* 'a kind of vesssel' (PMo **torku* 'tub, barrel; leather bucket' (Mong. > Turkmen *torka* 'bucket bag'), MKor. **tājá*, PJap. **tàrápì* 'trough' (Martin: 246, EDAL: p. 1391); PTk **TAr* 'pontoon, raft, boat', compared in the original EDAL etymology, should in this case be rather compared to PAlt **tʻiárko* 'a kind of carriage', PMo **terge*, PTM **turki* 'dog-sledge', MKor. **tǎrkó* 'light carriage'. The provenance of "Modern Osmanlı *dorbacık*", mentioned in Doerfer 1971, remains unclear. In contemporary dictionaries of Turkish, both literary and dialectal, this form is not attested. However, in TMN: II, 593 (which Doerfer refers to in the citation of this form in Doerfer 1971) we find "*dopracuq* 'ein kleiner Sack' (vielleicht 14. Jh.)" with reference to Vámbéry 1901: 162. This is, in fact, a form from an old Anatolian document (14th cenţury), and it should be noted that these texts show highly specific orthographic systems, which allow, in particular, substantial variation in the recording of initial dentals; cf. the distribution of forms with *d-* and *ţ-* (Arabic emphatic *t*, used in words with back vowels) in "Kalila and Dimna", also from the 14th century (Zajączkowski 1934):

- 'dust': *dopraq* — 5 times; *ţopraq* — 3 times (with Turkish *toprak*, Gagauz *toprak*, Azeri *torpag*, Turkmen, Khorezm-Oghuz *topraq*, Tuvinian *do'vuraq*, Tofa *to'praq*, PTk **topyrak* < PAlt **tʻăpʻo*(*rV*), PMo **toɣurag* id., PTM **tap-* 'to get dirty');
- 'to hold': *dut-* — 175 times, *ţut-* — 1 time (with Turkish *tut-*, Gagauz *tut-*, Azeri *tut-*, Turkmen *tut-*, Tuvinian *tu't-/du'dar*, Tofa *tu't-*, PTk **tut-* < PAlt **tʻ*[*u*]*tʻV*, PMo *todka-* 'to delay', PTM **tuta-* 'to stay').

Thus, one isolated case of a certain spelling cannot be accepted as relevant testimony of phonetic aberration until the exact manuscript is determined and the statistics of spellings with *d-* and *t-* for every morpheme in this manuscript is calculated — only in this manner can we restore the rules (or, more accurately, preferences) applied by individual scribes to the corresponding characters. After this, the forms are to be compared with contemporary (including dialectal) data, and only then we can proceed to meaningful hypotheses both on the phonetic meanings of the characters and on the dialectal identification of the scribe. Such philological research should undoubtedly be conducted for Old Anatolian and Old Osmanlı texts; until then, judgements based on individual forms extracted from particular manuscripts will remain unsubstantiated. This statement pertains to all examples from Doerfer 1969 (with respect to forms with *d-* in Old Osmanlı): instead of being based on a general analysis of graphic systems, they are simply drawn from the historical dictionary (TS), i. e. reflect individual spellings.

Doerfer's second example of an "Iranism with voicing" is 'bag': Turkish *dağar*, Gagauz *daar*, Azeri *dayar* (Turkmen *tayar-čı̈k* 'camel's foam alveole' — a Kipchakism), Tuvinian *taar*

(< *dh-*, or from Mong.); PTk **dagar*, with attestation in Common Turkic (Hakas *taar*; in documents with MK *tayar* 'bag for grains etc.'; the most widespread meanings are 'big bag', 'big stoneware vessel', and 'chopping block for cattle', see ESTYa 1980: 120–122). The word was borrowed into Persian as *taġar* (where it developed such meanings as 'bag /as a measure/' and 'food supply') and into Mong. as *tayar* 'bag'. The form *tagara* 'stoneware bowl' and similar forms, attested in Turkic languages since the 13th c. (Tafsir), as Doerfer had justly noted (in Doerfer TMN II 512–519), is a Persian diminuitive that was re-borrowed into some Turkic languages. The Turkic word has an Altaic etymology (< PAlt **t'agu*; cf. PMo **toγu-gan* 'caldron', PTM: Evenki *taγa* 'birch-bark basket'); PTk **d-* regularly becomes voiced < PAlt **t'* by assimilation with the word-medial voiced stop (see Dybo 2005: 53).

Again, in TMN: II, 512–519, Doerfer supposes (under question) that this word may have been borrowed from Turkic into Persian, although at the same time he assumes that the word is not Turkic in origin, but comes from an unknown language (based on a rather flimsy argument that there is no such Turkic root as **ta* or **tag*, from which this word could have been derived). All the forms listed there from "andere iran. Dialecte", including Yagnobi, Pashto, and Shugni, are obvious loanwords from Persian, but no Middle Persian form is attested, and Doerfer does not even try to provide an Iranian etymology for the Persian word. Shouldn't even a convinced follower of Doerfer's approach, given the presence of a rather elaborate etymology in TMN, remain skeptical towards a contradictory marginal remark in Doerfer 1971, with no arguments provided in its favor?

To these two examples of "voicing in Persianisms" one more example is added in the article Doerfer 1969: Turkish *denk* 'equal', ultimately a Sinitism = Chinese 等, contemp. *děng*, Middle Chinese *tɤŋ*, Old Chinese *tāŋʔ* 'rank, degree, grade, class' (Late Zhou), Western and Eastern Han, Early Post-Han Chinese *tǎŋ* 'rank, degree; class, sort; order; row, category, group; company; similar; such as; *a grammar word following lists*; equal; identical; to compare' (Karlgren 1923: 0961 i). However, it is easy to show that in Persian this is a relatively recent Turkism. The situation is as follows: the attested forms in Persian (according to Doerfer's examples) are *täng* 'horse-load, bag (of sugar)'[3] and *däng* 'half-load (for horse)' (Fazl-i-Ali 1979), 'equilibrium, balancing' (rarely) Rub., as well as *dängadäng* 'equal' (lit. 'däng against däng'). In Oghuz languages the forms are: Old Osmanlı *däng* 'correct weight' (P: III, 1660), *deng* 'half', *teng* 'one from a pair' (15th c., TS: II, 1062), Turkish *denk* 'equal, similar, pair; equilibrium; counterbalance; bale'; Gagauz *denk* 'equal, equivalent', Turkmen *deŋ* 'equal, identical; equally', *deŋ agramlɨ* 'equal by weight', Khorezm-Oghuz *dəŋ* 'equal' (Abdullaev 1961 I 36)[4]. The meaning 'counterbalance, half bale' is derived from the meanings 'equal weight, counterbalance on scales' (< 'equal'), attested already in Old Uigur (TT VIII; U II; from Old Uigur borrowed into Written Mongolian, MMo *teŋ* 'equal, straight, scales, counterbalance, bale' MA 346; Kow. 1691). It is evident that Oghuz forms demonstrate all the intermediate stages of semantic derivation, whereas Persian forms show only the final state (the expression *däng adäng* 'equal' is

[3] Persian *täng* 'girth', borrowed into some Iranian and Turkic languages, contrary to Doerfer, does not belong here; it is derived from the verbal root **tan-* 'to pull, tie, weave' or **tang-* / **θang-* 'to pull, weigh', see Horn 1893: 89.

[4] Azeri *tän* 'even, equal', as correctly supposed by Doerfer, is borrowed from Chagatai. Azeri dial. (Dmanisi) *taŋ* 'is equal', listed in ESTYa under the word in question — most probably, a Kipchakism, borrowed from Kumuk *taŋ* 'comfortable, suitable', connected with Kipchak *taŋla-* 'to choose', see Clauson EDT: 521. Old Osmanlı *dek* 'half bale' (13th–14th c.), *tek* 'one from a pair' (14th c.), Turkmen *tek* 'one from a pair', dial. *dek* 'equal; half bale' (DS: IV, 1406) et al. also do not belong here, contrary to Doerfer, who supposes a "dialectal phonetic" development *teng* > *teg*, upon which Karluk *teg* > *tek* and then this Karluk form was borrowed into all Turkic languages: in addition to the construction being extremely cumbersome, such a development as PTk **-ŋ* > Karluk *-k* is downright impossible.

clearly secondary relative to the meaning 'counterbalance'). Therefore, it makes more sense to suppose that Oghuz forms reflect the Common Turkic situation; that initial voicing reflects Old Chinese non-aspirated articulation; and that Modern Persian has in some cases borrowed forms with initial voicing from Oghuz languages, and in other cases voiceless forms from Kipchak or Karluk languages. Even if Modern Persian has borrowed the word with the meaning 'equilibrium, counterbalance' from Late Ancient Uighur (but if so, where does the Persian initial voicing come from?), eastern Oghuz forms (Turkmen, Khorezm) with initial voicing and with the meaning 'equal' cannot be Persianisms and should be traced back to Common Turkic. It is possible that Mod. Uigur *deŋ* 'equilibrium' is borrowed from Mod. Persian (Menges 1955).

New research on Proto-Turkic voiceless/voiced consonants (see, for instance, Dybo 2005) has revealed a number of regular phonetic innovations within the Turkic family that have only confirmed the original assumptions of V. M. Illich-Svitych, who was looking for patterns of phonetic correspondences here, rather than occasional irregular changes. (Of course, this does not mean that we should underestimate Doerfer's research as an enormous contribution to the historical phonetics of Turkic languages.)

Overall, the fact that Robbeets rejects certain innovations in the reconstruction of Proto-Turkic does little to increase the methodological rigour of her own reconstruction; and even from a purely theoretical standpoint, it is understandable that the introduction of additional phonological distinctions to the reconstructed system often allows for a more efficient fine-graining of suggested etymologies.

On p. 77, Robbeets writes that "the reconstruction of PTk **n'-* in initial position is still controversial". But nobody ever tried to reconstruct any initial sonorant consonants for PTk, it was even a problem for the Altaic comparison. The assumption that Hung. *nyár* 'summer' is a borrowing from Turkic **jār* 'spring; summer' is in itself beset with problems.[5] We cannot make

[5] See Dybo 2007: 166. If we consider Hung. *nyár* (*nyar-at*) 'summer' a loanword dating to the Proto-Ugric period, we would have to suppose an extremely archaic form for the Turkic source (PTk **jār* 'spring; summer'). Since the word begins with *ny-*, in order to explain it we must reconstruct such a situation in Proto-Turkic (or in early Bulgar) where the nasal quality of the Proto-Altaic consonant that normally developed into PT **j-* was preserved. This situation should be earlier than, for example, the one reflected in borrowings into Proto-Samoyed — cf. PSam **jemńə-* 'mend' < PTk **jama-* 'knit up' < PAlt **nĕmè* (EDAL). Apparently, this last argument decreases the credibility of the supposed borrowing, since (at least, according to contemporary opinion), the Proto-Ugric homeland was definitely to the west of the Samoyed ancestral home; consequently, the Turks, while advancing to the west, would only have encountered the Ugric people after the Samoyeds.

As to the etymology itself, we have the following difficulties here: (a) if it is a borrowing from Proto-Turkic into early Hungarian, the alternating length in Hungarian is hard to explain: Proto-Turkic long *ā* in early loanwords is reflected as a non-alternating long vowel (see Räsänen 1937, Dybo 2010); (b) the original as well as the Bulgar meaning of the Turkic word is most probably 'spring' rather than 'summer'; (c) although, contrary to MNyTESz, for phonetic reasons the Hungarian word cannot be traced back to Proto-Uralic **ńErV* 'Rute, junger Sproß' (UEW: 331; the phonetically correct Hungarian parallel to this root is *nyír*, pl. *nyírek* 'Birke, *betula*; dial. junger Schößling; (OHung) Birkenwald'), it is not isolated, since we can alternately compare it with PSam **nårå* 'Schneekruste; Frühling' (Nganasan *nóru* 'Schneekruste', *noru* 'spring', derivative *noruo* 'spring'; Enets *nářa, nára* 'Schneekruste', *nara* 'spring', derivative *nareo* 'das spätere Frühjahr'; Tundra Nenets *нара* 'spring before ice drift, the time of ice crust', *нарэй* 'spring before ice drift; spring (adj.)'; Forest Nenets *nārrɔ ə*; Karagas *nára* 'spring' Janhunen 1977: 98; Helimski 1997: 722; contrary to Janhunen, Mator *narha* 'new' hardly belongs here, but rather to PSam **ńarpɜ* 'new', Helimski 1997: 724). For Hungarian and PSam we can reconstruct a Proto-Uralic form **ńare* (*e*-base, judging by alternating length in Hungarian). In the light of all this evidence, it appears that a relationship exists between Uralic, Turkic and general Altaic forms. The Turkic form is traced back to PAlt **nə̯ə́ŕ*[*à*] 'young; spring, summer': PMo **nirai*, PTM **ńar-gu-*, MKor *njərɨ-m*, PJap **nátù* (Ramstedt EAS I 111; Vladimirtsov 1929:

any strong presumptions about PTk phonology based on one or two cases of dubious loanwords.

Regarding the issue of "rotacism / zetacism" in Proto-Turkic, Robbeets, while discussing the development of *z, s* from clusters, draws attention to Street's research (Street 1980) without mentioning the works by E. Helimski (1986a, 1986b) or my own paper (Dybo 1995 a), where some possible alternative cluster developments are suggested.

Finally, in her presentation of the reconstruction of Proto-Turkic vocalism, Robbeets mentions that the opposition of closed and open *e* is not directly reflected in any Turkic languages. This is not quite true, since in Azeri and in Turkmen the opposition between open and closed syllables with primary long vowels directly reflects this Proto-Turkic opposition (see the presentation of evidence in Dybo 2007, and still earlier in SIGTYa 2002).

The next part of the monograph largely concerns sifting through comparative lexical material in order to select convincing evidence in favor of Japanese-Altaic kinship. No other thesis is being proven: the existence of the Altaic family as such is not placed under doubt by the author. In other words, Robbeets' aims, on the basis of EDAL, to select those comparisons that are hard to deny on the surface level, to check if they conform to a system of phonetic correspondences, to verify if they are numerous enough to rule out accidental similarity, and to analyze the feasibility of interpreting the Japanese part of the data as borrowings from a certain Altaic language.

First of all, Robbeets studies those cases where a Japanese cognate has an internal etymology that contradicts the Altaic comparison. Here it should be noted that the absolute priority of internal etymology over external is hardly found among the basic postulates of comparative linguistics. Cf., for example, the amateur etymology of Russian *якорь* 'anchor' as a haplology from *яко-корь* 'like a root' (in reality, the word is borrowed from Greek) — looking quite plausible on the surface, but definitely not true. One should take into account that in restricted etymological systems (e. g. in the case of language isolates) researchers often attempt to stretch the limits of internal etymology until the explanation is no longer satisfactory or even realistic. Thus, for Proto-Japanese **əpə-mənə* 'food' (> AJP *op*(*w*)*omono*, Martin JLTT: 509), its internal analysis as 'big thing' is clearly a folk etymology: cf. the same root in the verbal derivative OJ *op*(*w*)*o-k-* 'to eat greedily'[6]. In EDAL, the term "folk etymology" for such cases was used euphemistically — implying lack of serious phonetic or semantic evidence in proposals published earlier by researchers.

Regarding those cases where Robbeets suggests internal etymologization through morphological segmentation, one should also remember that, on strictly formal grounds, we could even detach the deverbal affix *-ing* in Eng. *thing*, Germ. *Ding* etc., although it is actually part of the stem (**þenaz ~ *þenxaz* sb.n.: Goth *þeihs* 'occasion, time', ON *þing* 'assembly, thing', OE *þin* 'thing, meeting', OFris *thing* 'assembly; legal case; thing', OS *thing* id., OHG *ding* id., Orel 420). One should be particularly cautious when proposing such etymologies for compound words where their individual parts do not imply any regularity (such as MJ *fitume* 'tip of a hoof', where *tume* is explained as 'hoof', while *pi* is given a very dubious explanation). The argument where the author proposes the existence of an early *i* '5' based on comparison of *itutu* '5' and *iso* '50', *ipo* '100' seems unconvincing when viewed against the background of numerous examples of contracted compounds in Japanese. Subsequently, in the section on "Arbitrarily in-

145–146; Poppe VGAS: 38, 81; Martin 1966: 243; OSNYa II: 84; S. Starostin 1991: 74; SIGTYa 2000: 73–74) should best be investigated under the angle of an ancient genetic connection (Nostratic).

[6] Apparently, some attempts to find etymologies for Indo-European kinship terms are of the same nature; cf. **pətḗ*(*r*), gen. **pətr-és*, *-ós* "zu *pō*(*i*)- schützen?" (Pok. 829), etc.

serted morpheme boundaries" one finds many cases whose analysis directly contradicts the logic of the section on "Undetected morpheme boundaries".

Nonetheless, it should be acknowledged that in many cases, challenges to EDAL etymologies are based on quite serious grounds.

In section 6.2 ("Morphology") the author lists several hypotheses on the origin of a number of Japanese grammatical formants, mostly explaining them through grammaticalization of lexical nouns; this section is arguably one of the most useful in the entire book.

Concerning "Nursery words and Sound Symbolism" (section 7), I would like to note once more that, in general, words suspected of sound-symbolic properties can be expected to slightly deviate from strict phonetic correspondences, but it hardly makes sense to dismiss out of hand otherwise phonetically and semantically satisfactory cases on the grounds that, bypassing standard etymologization, they can be explained within the general framework of such phenomena. Especially in a language with such a limited phonological inventory and such harsh restrictions on syllabic structure as Japanese, considering how vague are the criteria to define sound symbolism, it may be possible to treat almost any word as "sound-symbolic".

In Chapter 8, the author filters out cases that can be allegedly explained as borrowings into Japanese from other sources. Here we can only note that unequivocal borrowings may only be postulated for languages whose history is very well understood; for this reason, most of the explanations that suggest ancient borrowing from Ainu seem invariably less convincing than the alternate hypotheses of Altaic origin for such words — for example, the hypothesis about *iruka* 'dolphin' being borrowed from Ainu *rika* 'whale' is hardly more plausible, semantically and phonetically, than the suggested Altaic etymology. Borrowing from an unknown language (incidentally, such borrowings are quite often supposed in Turcology), as in the case of Japanese *kuma* and Kor. *kwom*, is a speculation that hardly deserves attention, unless there is significant internal evidence for this, such as a serious violation of phonotactic rules within a particular morpheme. And some of the hypotheses just leave a weird impression — e. g., the attempt to explain Jap. *mara* 'penis' by means of Bdh. Skt. *ma:ra* 'evil'. Yet on the whole, once again, criticism of particular EDAL etymologies is often useful and should be definitely paid attention in order to improve etymological analysis.

In chapter 10, Robbeets is checking if the established phonetic correspondences survive sifting and are still valid when restricted to the remaining material. Here it should be noted that it may have been preferable to rely on sources outside of EDAL to analyze phonetic issues — in the case of language groups with well-developed etymological traditions, the corresponding parts of EDAL entries should rather be viewed as condensed references to internal etymological dictionaries. In general, one can sense insufficient command of data on continental Altaic language groups on the part of the author. Thus, on p. 288 she contests the EDAL reconstruction of PTk **(j)ēn-čik* 'shin' and modifies it to **īnčik*, based primarily on such forms as Turkish *incik* and Turkmen *īnžik*. However, first of all, if there really is a reflex that contradicts the reconstruction of initial **j-*, it would not be the Oghuz forms (transition **ji- > i-* is fairly common in Oghuz languages), but rather Yakut *inńiäx* (Pek. 1941; initial **j-* in Yakut is expected to yield *s-*). Second, such reconstruction does not explain such Turkic forms as Middle Turkic *jinžik* (IM), Siberian Tatar *jinžik* (Tumasheva 1961), Shor *enžik,* Chalkan *enčik,* Bashkort *jensek*. As shown in Dybo 2007: 55–57 (and, earlier, in SIGTYa 2002: 40–42), in such cases we can reconstruct a descending diphthong (Ptk **ējn-čik*), and such a Turkic protoform agrees with the Altaic reconstruction of the word **pʹèjńé*.

An interesting methodological innovation, introduced by Robbeets in this chapter, is matrix analysis of the correspondences. Despite its usefulness, however, the analysis contains cer-

tain inaccuracies. Thus, matrix 1 analyzes the comparisons that should support non-randomness of the correlation PJap **p-* : PKor **p-* : PTM **p-* : PMo **h-* (**p-* according to Robbeets) : PTk **b-*. At the same time, as I have already mentioned earlier, the author simplifies the PAlt reconstruction — but in this case, rather than rolling it back to the traditional version based on the "Ramstedt-Pelliot law" (PJ **p-* : PKor **p-* : PTM **p-* : PMo **h-* (**p-* according to Robbeets) : PTk **0-* (Khalaj *h-*, as supposed by Doerfer)), she selects as the only correct one the innovative correspondence series of Illich-Svitych, where Turkic **b-* corresponds to voiceless consonants in the other groups (for these cases, the EDAL model proposes to reconstruct non-aspirated **p-*). As for the cases that conform to the traditional row of correspondences, Robbeets simply removes the Turkic forms with *0-* as irregular (they are given in square brackets).

However, statistics seems to go against this decision. In matrix 1.1, there are 9 stems with Turk. *b-* (and at least one of them was placed there by mistake: Turkic *bir*, PM **bueri*, Jap **pito* should have been listed in matrix 1.2.), and 15 stems with Turk. *0-*/**y-* (cases with dipthong), plus 3 more in the list of "irregular" cases in matrix 1.3. This gives us 8 "regular" cases against 18 "irregular" ones (!), and after all the manipulations, we are still basically left with 2 rows of correspondences: (a) PJ **p-* : PKor **p-* : PTM **p-* : PMo **h-* (**p-* according to Robbeets) : PTk **0-* and (b) PJ **p-* : PKor **p-* : PTM **p-* : PMo **p-* : PTk **b-*. (The third row remains trivial: PJ **p-* : PKor **p-* : PTM **b-* : PMo **b-* : PTk **b-*).

The same applies to the matrix that lists correspondences for dental consonants. We can detect that the correlation "PJ **t-* : PKor **c-* : PTM **č-* : PMo **č-* : PTk **č-*", including a cognate in Turkic, was recorded 8 times (2.3) and is considered regular. But the correspondence "PJ **t-* : PKor **t-* : PTM **d-* : PMo **d-* : PTk **d-* (**t-*, according to Robbeets)", including a Turkic cognate, was attested 5 times (2.2), and in all these cases the Turkic cognate is placed in brackets, i.e. declared phonetically irregular. The correlation "PMo **d-* : PTk **d-*" is recorded another 5 times in (2.4), where its correlates in other branches are PTM **ǯ-*, PKor **c-*, PJap **t-*, and is declared regular, going back to PAlt **ǯ-* (pp. 297–300). At the same time, on p. 321–322 we see such matrices as "PJ **y-* : PKor **c-* : PTM **ǯ-* : PMo **ǯ-* : PTk **j-*" (also said to reflect PAlt **ǯ-*) and "PJ **y-* : PKor **c-* : PTM **d-* : PMo **d-* : PTk **j-*", said to reflect PAlt *d-. On the latter group of examples, the author comments: "It can be remarked that a number of entries have pTk **t-* as the Turkic reflex, but anticipating what follows the Turkic candidates do not stand the phonological test due to their problematic medial consonants and vowels". However, these candidates (**terpe-*, **tōrt*, **tāj-*, **taš* < **talC*, **teš* < **telC*, listed on pp. 321–322) generally demonstrate the same vocalic and consonantal composition as their Mongolian and Tungusic cognates (which have not been ruled out by the author), and if we add up the examples of "PTM **d-* : PMo **d-* : PTk **d-*" from matrix 2.2, the overall number becomes so impressive that it is hard to get rid of the feeling that the author discards a large number of perfectly valid etymologies simply because they do not fit into her "reductionist" theory, not properly founded upon the standard historical-comparative method.

Similar problems arise with the assumption, on p. 311, of the secondary character of PTM **x-* (even though the conditions of such a development remain unknown), where one of the arguments is the observation that word-medial **-x-* is strangely absent in the PTM system. Actually, serious arguments in favor of the reconstruction of this phoneme in word-medial as well as word-initial position were already proposed in Dybo 1990 (with the publication of SSTMYa, the amount of available data on TM languages grew considerably compared to earlier work by Benzing, which explains a large number of innovations in the reconstruction of PTM that were accepted in EDAL). The other argument against the reconstruction of the "guttural triad" for PTM and PAlt is purely structural, based on alleged parallelism with the system of binary oppositions for other occlusive consonants. However, since the potential third

series for labials, dentals, and fricatives was previously ruled out with serious violation of basic comparative-historical methodology, the argument is hardly acceptable.

The brief overview of the history of reconstruction of PAlt **ń-* (p. 315) has no mention of Dybo 1995b, where the correspondence "PJ **m-* : PTM **ń-* : PMo **ǯ-*", reflecting PAlt **ń-*, accepted in EDAL, was described in detail. On the other hand, the reconstruction of clusters as a possible origin for CTk **š* and **z* (pp. 330–332) seems to be promising and requires further consideration.

Overall, the phonetic table reconstructed in the monograph is seriously abridged compared to the version of EDAL. Although Robbeets mentions that the table only pertains to etymologies that include Japanese material and is not necessarily exhaustive for Altaic as a whole, in truth, the main reason for reducing the number of the reconstructed phonemes is the assumption of a series of unconditioned consonantal splits, with no explanations provided. This certainly does not improve upon the regularity of the model proposed in EDAL, and for that reason, the model of Robbeets cannot be considered as an advance on that model.

The last part of the monograph is given over to analysis of those etymologies from the core vocabulary that were selected as reliable. In the author's opinion, they constitute sufficient evidence to prove the Altaic affiliation of Japanese, and, in general, I agree with Robbeets' analysis. However, I do have certain objections to some odd methodological theses, proposed on p. 413 in the author's analysis of morphological parallels:

> (1) In agglutinative languages the morphemes are mainly suffixes or unbound postpositions. They are in a peripheral position, a position where phonological erosion is expected

— but let us note in passing that there are plenty of language families with agglutinative prefixation as well, e. g. Abkhaz-Adyghe or Central Saharan;

> (2) This is also true for a large number of Indo-European suffixes, like e. g. the proto-Germanic *-iz* plural that completely eroded in final position. However, the Germanic plural left a trace in the root due to the inflectional feature of Indo-European. Agglutinative word formation, on the contrary, tends to exact segmentation of root and morpheme. In Japanese, Korean and Altaic we do not expect inflectional fusion like the English *mouse — mice* in which a lost plural morpheme *-iz* can be traced in the phonology of the root.

In reality, however, no typological characteristic that is "immanent" to the language, be it flectivity or agglutinativity, can with complete predictability influence (or not influence) the phonetic processes in that language. Thus, the phenomenon of fusion is well attested for most Turkic and Tungus-Manchu languages. Moreover, such a phenomenon as "Uighur umlaut" is well known: in Modern Uighur, vowels of the stem change under the influence of vowels in subsequent syllables. For some words this phenomenon helps to determine which vowel (**U* or **I*) was present in the second syllable in Proto-Turkic (in most other languages, the difference has been erased because of labial vowel harmony): cf. Uig. *beliq* 'fish' < **balïk*, but Uig. *yoruq* 'light' < **yaruk.* In the same language, the degree of aperture in vowels of the non-first syllable depends on whether the syllable is open or closed, resulting in inflectional alternations (*yaš-lar* 'young person-Pl' — *yaš-lir-i* 'young person-Pl–3Prs'). If, over the course of subsequent changes in the language system, final narrow vowels get lost (and such events are known in the history of various Altaic languages), the forms would differ only by the alternating variants of the final vowel. Such phenomena are to be easily expected in the history of Altaic languages, which makes a precise reconstruction of Proto-Altaic vocalism an especially hard task.

All said, despite various deficiencies of the author's approach described in this section, there can be no doubt that over the course of this research, conducted already a decade ago, Martine Robbeets has emerged as a serious, ambitious, and independent researcher, initiating the extremely important task of revising and thoroughly evaluating the new Altaic reconstruction (as presented in EDAL) that no other researcher or reviewer had really set for him/herself before that (or, for that matter, ever since).

* * *

The second monograph by Robbeets, published last year (2015: *Diachrony of verb morphology: Japanese and the Transeurasian languages*), is primarily focused upon the possibilities of reconstructing elements of Proto-Altaic morphology, mainly the verbal system. The author's choice was most likely influenced by recent research, which indicates that borrowing of verbal lexicon and verbal morphology is quite rare (cf. *Loanword Typology Project*, especially Wohlgemuth 2009). Because of this primary emphasis on grammatical topics, the work is saturated with information on the general typology of grammar and grammaticalization, and the author demonstrates close familiarity with practically all the main publications in this area.

In the introduction, Robbeets lists the main factors that have motivated her to attempt a proof of Altaic relationship from this angle. On the whole, the presentation gives a fairly reasonable impression; for some reason, however, the fact that EDAL actually did contain a substantial number of satisfactory grammatical parallels between various branches of Altaic is not mentioned (for that matter, nor do the other critics of EDAL usually pay any attention to this section of the dictionary). It is an entirely different matter that these parallels do not show much paradigmaticity, due to the fact that Altaic morphology is positional rather than paradigmatic. As for the attempt to discard the term "Altaic languages" altogether (and substitute it with the newly-coined "Transeurasian"), it is somewhat amusing, but is probably due to the irrational antipathy that some researchers experience in relation to this term, more than any other factors.

In general, we agree with the presentation of the chronology of Altaic divergence and the main features of different Altaic languages; however, a few remarks should be made:

(a) there is no record of Bulgar presence in Kazakhstan;

(b) concerning the literature on Xiongnu, there is no mention of Dybo 2007, which contained certain arguments in favor of the current reconstruction of Proto-Turkic;

(c) the idea, proposed by Doerfer, that the Khalaj branch had split from Common Turkic before Yakut remains unfounded. The phonetic properties on which Doerfer bases his classificatory argumentation are not shared innovations, but rather preserved archaisms. These are *h-* < PAlt **p'-;* preservation of primary vowel length (where long and short vowels differ according to Oghuz model, but do not show Oghuz voicing); and preservation of **-δ-*. Final **-g* in polysyllabic words was not lost. Khalaj morphology in general resembles Oghuz; verbal affixes of the first series preserve the archaic 1Pers.Pl. *-UK*. There is one peculiarity of Khalaj nominal declension which brings it closer to Chuvash — preservation of pronominal declension that, without yielding to analogical influence, has maintained the postvocalic genitive affix **-ŋ > y*; although this feature could be considered archaic, in reality it is rather a secondary haplologic development, cf. a similar situation with the Genitive 2Pers. possessive form **-ŋ-Uŋ > -y* along with the presence of the postvocalic genitive variety *-yn*. Thus, with respect to nominal inflection, Khalaj is closer to Oghuz languages, although only through preservation of archaic properties: the language did not undergo the innovations that covered Southern Siberia, Karluk and Kipchak groups (see Doerfer 1988: 79);

(d) Eastern Old Turkic as an attested written language form is quite evidently NOT the ancestor of all attested Turkic languages with the exception of Chuvash and Khalaj. Specifically, it contains some innovations shared with some languages of the Northeastern group of Turkic (e. g. 1Pl *-myz* in finite verbal forms, etc.);

(e) The Northeastern group clearly does NOT constitute a genealogical branch. Yakut-Dolgan and Tuva-Tofa are different branches and presumably split from CT at the same time as Oghuz. The splitting of the Oghuz branch is not related to the spread of the Mongol empire, having taken place much earlier. As was shown in many works of the Moscow school (mentioned above), the "voicing" of initial **k, *t* is not an innovation, but an archaism; if the author does not accept this, she should explicitly dispute it. The actual common innovations of Oghuz are: the restructuring of the opposition *ä ~ e*; the so-called Oghuz voicing of medial **-k-, *-t-, *-p-* after primarily long vowels; and the development of consonantal ~ vocalic declension types;

(f) there is no substantial Bashkir presence in Uzbekistan and Kazakhstan, nor is there any substantial presence of Kazakh in Asia Minor;

(g) on Mongolic languages: it is not true that all contemporary Mongolic languages can be traced to the language spoken by Genghis Khan (p. 12). The Southern Mongolic languages of the Kukunor group (Mongghuer, Dunxiang, Baoan) presumably split from Proto-Mongolic much earlier, around the 5th century (this may have been related to the migration of the Xianbi clan Muyung into the region of Gansu in 313 A.D. (see Bichurin 1833: 844; cf. glotto-chronological data — 85–88% common matches on the 110-item wordlist, as per Gruntov, Mazo 2015);

(h) the following passage, with reference to Nugteren 2011 (p. 14), is probably mistaken: "Whereas word-medial palatal breaking is still in progress in Mongolian proper, it has been completed in peripheral languages, for instance…a front vowel has been preserved in … Kalmuck *nüdn* 'eye', whereas it resulted in a back vowel in … Mongghuer *nudu*". However, palatal breaking has no relation to this, see Nugteren 2011: 36–37: "Common Mongolic vowel harmony involved two classes of vowels. The distinction between the two may have been an opposition between front and back vowels or may have been based on tongue root position. The QG languages do not provide additional evidence to resolve this matter". The surface back quality of Gansu *u* (< **ü*)-vowels is almost identical with those of Khalkha. "In Monguor vowel harmony has broken down both in stems and in suffixation. The front rounded vowels **ö* and **ü* merged with their back counterparts **o* and **u*. Nevertheless, the former harmonic constraints are clearly visible in many existing primary and derived stems… In Baoan and Dungxiang there are also only two rounded vowels left, but as Kangjia preserves four, generally corresponding to the four rounded vowels of CM";

(i) Tungusic: the term "Tungus-Manchu" remains preferable, since, as it has been shown in a number of works (Sunik 1962: 16–17; Vasilevich 1960; Avrorin 1957: 473; Avrorin 1959: 3–4), based on phonetic, morphological, and lexical arguments, the split of the Manchu branch was the first one to take place within the family, and it would be logical to reserve the term "Tungusic" for all the languages that remained. Should it be stressed that nothing is known about the linguistic affiliation of Sushen? Also, Kili, or the Kur-Urmian dialect of Nanai, by no means belongs to the Southern group — it is actually Northern Tungusic, close to Negidal;

(j) On Japonic and Korean, p. 20: how is the fact that Korean and Japanese families at some point coexisted on the Korean peninsula documented? Do we bring the Koguryo language as a representation of Japonic into discussion? And how should we interpret the statement: "their coexistence was discontinued when Japonic relocated to the Japanese Islands in the first millenium B.C.?" This suggests that their coexistence was somehow documented before the first millenium B.C., but in what sources?

Additionally, it seems that the Farming/Language dispersal hypothesis (to which the author refers) contradicts historically attested facts. In reality, all cases of historically documented migrations and language spread have been due to the dispersal of nomadic stock-raising peoples.

On pp. 45–88 we find an account of the main methodological principles of comparative-historical research adopted by the author. We can agree with almost all of its points — this is a sound methodological chapter, providing basic information on morphological reconstruction as it is usually described in introductory courses (some of them are cited by the author). In particular, the following approach to internal reconstruction is introduced (pp. 47–48):

> In internal reconstruction, alternations within a single synchronic stage of a language are "undone" as it were, and an earlier state is reconstructed. In this process, it must be ensured that a plausible developmental pathway can be traced from the earlier reconstructed form and function to the attested ones. In Middle Korean, for instance, there is a causative-passive marker that has numerous allophones MK *-Ac?-, -Gi-, -hi-, -i-, -y-,* and also has various functions: it either derives causatives from transitive and intransitive verbs or passives from transitive verbs. Combining phonological knowledge about velar lenition with insights into the general typology of the development from causatives into passives, allows us to undo the changes and reconstruct an original causative marker of the shape pK **-ki-* (cf. Section 6.7.2).

Some minor comments are, however, necessary.

P. 48: "Theoretically, it follows that morphological reconstruction should always be preceded by phonological reconstruction. This is especially true for the Transeurasian languages, which are agglutinative and thus tend to share fewer idiosyncrasies useful for the establishment of fusional families like Indo-European. Shared irregularities such as the suppletive *ego* / *me* pronominal stems can demonstrate the correspondence between morpheme shapes without reference to regular sound correspondences". However, (1) pronominal stems *are* irregular in Altaic languages (cf. *ol ~ an-* in Turkic, *bi ~ na-* in Mongolic); (2) the congruence of Lat. *ego*, Skt. *aham* and Slav. *azъ* still can be shown only by means of the regular correspondences.

P. 55: "A genetic relationship can be demonstrated on the basis of regular correspondences in form and function. It should be kept in mind, however, that identifying correspondences does not require reconstruction. The reconstruction of Proto-Transeurasian morphemes is a by-product, rather than the primary goal, of the comparative method. As Harrison (2003: 225) puts it: "One can use the comparative method to draw genetic conclusions without reconstructing a thing." Nevertheless, the present work will propose concrete reconstructions for ancestral morphemes because they make the posited set of changes between the daughter languages and the ancestral language more visible and because they serve as the basic units of the overall ancestral morphological system".

I would not agree with the last point. To demonstrate genetic relationship, it is necessary to do more than simply show regular correspondences: it is also very important to show that it is possible to reconstruct a specific fragment of the protolanguage and to formulate historically realistic transition rules between the protolanguage and its descendant languages.

P. 58: "Some of these forms have even led to the reconstruction of a causative-reflexive in proto-Nostratic *t'V- by Kaiser and Shevoroshkin (1988: 313)." — The reconstruction of this morpheme was done by V.M. Illich-Svitych (OSNYa 1971: 13); Kaiser and Shevoroshkin gave an account of it for English readers.

P. 59: "… the probability that a certain correspondence in verb morphology is due to coincidence will be lower than that for a similar correspondence within the lexicon, because the body of elements open to comparison is much smaller" — this argument is quite dubious,

since any assessment of the probability of chance coincidences should be carried out on joint lists of grammatical and lexical morphemes. Also, we must not ignore that decreasing the size of the sample automatically decreases the size of the confidence interval for it, meaning that the overall statistical reliability of the results is lower in the case of a smaller inventory of elements.

P. 61: "Metaphorically, the term 'copy' is obviously more correct than the term 'borrowing' because the model language does not give anything up, and the copying language does not give a borrowed item back. The main point, however, is that a copy is never identical with the model". Metaphoric notions of losing a "borrowing" by the donor language are resolved by the fact that we speak of information units, which do not get lost by the donor during transfer (cf. a similar situation when we still use the term 'borrowing': "Talent borrows, genius steals"). Additionally, the term "copy" does not seem to account for the very typical fact that, while at the time of borrowing the borrowed unit looks maximally close to the source unit, it then gradually adjusts to the constraints of the new language; cf. the difference between "adapted" / "non-adapted" borrowing (should the term "copy" be applied only to the latter?).

P. 62: "Their description of copiability as a relative tendency suggests that bound verb morphemes belong to the most stable parts of linguistic substance and provide fairly reliable evidence to demonstrate common ancestorship. Even though I believe that no single part of language structure is conclusive by itself, my decision to limit the scope of this book to bound verb morphology is based on this assumption". As nice as it looks, there are also known cases like Copper Island Aleut, where what we observe is precisely the borrowing of morphemes from bound verb morphology! (However, not the *complete* bound verb morphology system).

P. 64: "An indication of morphological borrowing is the restriction of shared morphemes to shared roots. This criterion is valid for derivational as well as for inflectional morphology. The borrowing of derivational morphology is a gradual process: first, the morphemes are borrowed along with lexical items; later, they become extracted and productive on other foreign bases and finally, on native bases. Matras (2009: 209) distinguishes between the term "forward diffusion" for the former case and "backwards diffusion" for the latter. The denominal verbalizers *-ize* and *-ify*, for instance, entered English in the 12th century through borrowings of Old French verbs ending in *-iser* and *-efier /-ifier* (e. g. *baptize, stupefy, sanctify*). From the 16th century onwards new verbs were derived, first, from Latinate (e. g. *equalize, objectify*), then from other foreign bases such as Greek (e. g. *chondrify* 'turn into cartilage (Greek *chondros*)') and, finally, from some native bases (e. g. *womanize, ladify*), but even in contemporary English *-ize* and *-ify* combine more frequently with foreign than with native bases (Marchand 1960, 238–240, 255–259; Gottfurcht 2007: 84–85)".

This reasoning is by all means fair, but, theoretically, I could easily imagine some fervent anti-Indo-Europeanist who might try to debunk one of the most transparent proofs of Indo-European genetic relationship — common inheritance of two conjugation types, thematic and athematic, with partial preservation of lexical distribution — in the following manner: we can suppose that affixes of athematic conjugation were borrowed into Ancient Greek from, for example, an ancestor of Sanskrit (in its oldest state, still with the distinction of *e, o, a*, which later converged to *a* in Indo-Iranian languages), first, for specific verbal roots (**esmi* > *eimi* 'I am' etc.), after which they spread to some proper Greek verbs (*ollumi* 'I kill', etc.). In such cases, it is really only our general experience, suggesting that such verbs as 'to be' and 'to eat' are not easily borrowed, that prevents us from setting up this scenario as at least equiprobable with the scenario of genetic inheritance.

P. 64: "Similarly, Wutun (Sinitic) has borrowed from Bao'an (Mongolic) the interrogative marker *-mu*, e. g. Wutun *qe-lio-mu* [eat-PFV-INTER] 'have (you) eaten?' (Janhunen 2012c: 25). The

Wutun interrogative contains the Bao'an finite narrative marker *-m-* and the interrogative *-u*, e. g. Bao'an *ode-m-u* [go-FIN-INTER] 'do (you) go?'. However, Wutun reinterpreted the morpheme without taking into account the tense-aspect marking".

Actually, it might be more efficient to explain this particle as a borrowing from Bao'an *-mbu*, a combination of an affirmative word (focus particle) and an interrogative particle, borrowed into Wutun as a whole. Cf. Bao'an *та јуджі ідэрсанг мбу?* "Aren't you tired from the journey?", "*нэ мэнэ мэсго мбу?* "Are these my clothes?" (Todaeva 1964: 106).

P. 68: "The repeated marking of an inflectional category that has already been expressed is an indication of code-copying". Multiple marking of categories is a frequent phenomenon in agglutinative languages, especially in Siberia; in verbal forms it is traced to synthesized constructions with auxiliary verbs. In general, grammatical categories in agglutinative languages are very different from those in inflectional languages: they are not obligatory, nor do they always have a unique expression within a certain wordform. An example is cited: "For example, the verb forms *kimumisti* 'we are sleeping' and *kimasti* 'you (PL) are sleeping' in other Greek dialects correspond to the Silli forms *kimumisti-niz* and *kimasti-niz*, in which *-iniz* is copied from Turkish as a general marker of plurality without regard to person. We can thus say that the forms are double-marked for plurality". But cf. an indigenous Khakas form: *Палыхтап иирзер пар-ар-быс-тар* 'We will go fishing in the evening' (*пар-ар-быс-тар* 'go-FUT-1.PL-PL), generated in a similar manner. Therefore, the criterion is not very reliable.

P. 69: "The semantic mismatch between "infinitive" and "verbalizer", occuring in this example (French *traiter* 'to treat' < Latin *tracta:re*) is a counter argument against inheritance".

This is also not a very good criterion. Such a semantic shift is, undoubtedly, due to the fact that Latin conjugation in *-are* includes a large number of denominal verbs, and it is a productive type for the formation of denominal verbs; therefore *-are*, even in Latin, functions as a verbalizer. Here (as in the following example with Yakut and Tungus) we can say that in borrowings the functions of the marker can sometimes be narrowed; but narrowing of a marker's functions can also occur in the course of historic development (cf. the development of the *-l-* participle in Russian — from marking past tense in both primary and secondary predications, it shifted to exclusively primary predications).

P. 71: "Copper Island Aleut, Michif, Gurindji Kriol and Ma'a can be regarded as 'mixed' languages because different parts of grammar and lexicon come from different languages, to such an extent that it is impossible to assign them unequivocally to a single genealogical ancestor. The question arises whether in these cases 'mixed' refers to the nature of the languages having double ancestry or to the perception of the linguist, who may no longer be able to clearly distinguish the inherited from the copied subsystems. In my view, these "mixed" languages may represent instances of code-copying taken to an extreme".

It can hardly be doubted that at least for both Copper Island Aleut and Michif we can easily determine the genetic ancestor. In Copper Island Aleut it is clearly seen that verbal morphology is borrowed from Russian rather than Russian inherited. Only the most productive paradigmatic class of Russian conjugation is used, neglecting the base joining rules and with complete loss of lexical distribution. Two forms are borrowed for pronouns (Nom., Acc.) due to their relative infrequency in Aleut, since the corresponding meanings are usually expressed within the verbal wordform. In Michif, on the contrary, truly complex Cree verbal morphology is preserved, while French nominal morphology is so structurally simple that its borrowing can be easily explained.

P. 73: The example of "the contact-induced grammaticalization of the verb 'to make, do' to a causative auxiliary" is not very convincing, since such grammaticalization is typologically frequent (cf. French, Azerbaijani).

As to the other criteria for borrowing (2.4.2.1 — productivity restricted to shared roots; 2.4.2.2 — unilateral morphological complexity; 2.4.2.3 — mismatch of morpheme boundaries; 2.4.2.6 — phonological mismatch; 2.4.2.7 — distribution limited to contact zones), these are well-described and quite convincing.

P. 74: "2.4.3 Indications of genealogical retention → 2.4.3.1 Globally shared grammaticalization": it would seem that "globally shared grammaticalization" cannot be taken as a criterion of inheritance, if it all amounts to a common morpheme which was grammaticalized after a typologically frequent model. In such cases it can hardly matter if this morpheme was commonly shared through inheritance or through contact.

P. 75: "The globally shared grammaticalization should be spread over more than two (proto-) languages." Everything that was said above on the low probability of the same morpheme being borrowed into a number of related languages is correct; however, if the grammaticalization pattern is sufficiently frequent (e. g. "go" or "want" for 'FUT', "do" for 'CAUS' etc.), then we can only speak of such probabilities for borrowing *lexemes*, rather than morphemes. On the other hand, a contradictory example may be found in the privative affix *-sI/Uz*, borrowed from Azeri into Budukh (Talibov 2007: 109), Kryz (Authier 2009: 70), and some other North Caucasian languages in Azerbaijan.

P. 76: Concerning criterion 2.4.3.3 ("Shared cumulation"), I must stress that *any* statements on the etymology of inflectional morphemes in languages for which there are no general comparative grammars or etymological dictionaries (e. g. the languages of Australia) must, by definition, be regarded as highly unreliable. Among such cases is the situation with case copying in Arnhem land, and the same holds for the alleged borrowing of denominals from Ritharnu to Ngandi: if the morpheme *-ti-* does not have the same meaning in Ngandi as it has in Ritharnu, it could simply represent a different affix, phonetically similar through sheer coincidence. This is a general flaw in contemporary typology of areal contact — as if the process of borrowing is not in itself an object of comparative linguistics and does not need to be subjected to strict etymological analysis, so that it becomes sufficient to merely *state* that "A is borrowed from B" without presenting actual historical evidence for this statement.

On the other hand, shared cumulation is not an absolute criterion for relatedness either. We know some cases of borrowing of cumulative affixes, e. g. Copper Island Aleut shows copying of Russian portmanteau person-number flexions in verbs (at the same time, Russian nominal flexions are not copied with the same degree of cumulativity). The example of borrowing from Yakut to Evenki, cited earlier by the author (pp. 67–68: "copying of the Yakut presumptive-assertive paradigm as presumptive in Uchur Evenki and as assertive in Lamunkhin Even. The copied suffix strings require specific accommodation with the marker *-r-* in Evenki and with the connective glide *-j-* in Even, which is not needed for the attachment of native suffixes") refers to cumulative borrowing, without morphemic analysis. And insertion of *-r-* and *-j-* does not mark borrowings — it is typical of verbal stems that are incorporated into composite words (see Boldyrev 2007: 639).

Another disputable statement is: "When the semantic correspondence… concerns a meaning that is demonstrably secondary to one of the participating morphemes, we are probably dealing with a copy". What if this is merely a semantic change? Let us suppose that Genitive often develops from Ablative. In Sanskrit, the flexion *-ad* conveys the semantic roles of Ablative and Instrumental; its cognate in archaic Latin is similar. Let us further make an etymologically reasonable supposition that in Slavic its cognate conveys the meaning of Genitive. Since this is a secondary meaning in relation to Ablative, should we consider the Slavic morpheme a borrowing?

As for the first part of the cited observation ("when the semantic correspondence is so divergent that it cannot be explained by referring to cross-linguistically attested pathways of grammaticalization…, we are probably dealing with a copy"), here I would rather assume

that, if the semantic development cannot be explained by a regular pattern of grammatical change, it is not even a borrowing — rather just a spontaneous coincidence.

Table 7, with the verbal paradigm of Copper Island Aleut, has some misprints: the root of the Russian verb "to speak" should be spelled *govor-* when transliterated and *gavar-* when transcribed. The example itself is not particularly successful: allomorphism in the paradigm of Russian verbs is reduced in Russian mutual common language when the flexion is unstressed [*-iš, -it, -im, -itə, -'ut*], so in this case it is not "copying" that caused this reduction.

P. 81: Section 2.5.2, "One cannot demonstrate unrelatedness", seems very well written and detailed. Only one important point should be added here: there is a very good way to prove unrelatedness of a certain language to a certain language group — that is, to prove its relatedness to *another* language group, i. e. to show that it belongs to a node on a totally different genealogic tree. No sooner do we have reliable proof of, say, common ancestry between Chukchee-Kamchatkan and Tungus-Manchu, Finno-Ugric and Mongolian, Austronesian and Japanese-Ryukyuan, Sino-Tibetan (or Hmong-Mien) and Turkic, etc., the issue of the Altaic family will be automatically taken off the agenda. Incidentally, none of the anti-Altaicists have so far succeeded in anything of the kind.

In general, the author's conclusions on the importance/necessity of morphological evidence do not raise any serious objections. It might only be added that the outstanding conclusiveness of Indo-European morphological parallels is also due to the uniform distribution of verbal paradigmatic types among groups of lexical cognates in different IE languages (when many verbs can be reconstructed for the protolanguage as specifically belonging to the athematic verbal class, etc.). Such a situation cannot obviously be expected of agglutinating languages where absence of lexically distributed paradigmatic classes is one of the main features, and, consequently, it is unreasonable to demand that comparative Altaic morphology should comply to the exact same requirements as comparative IE morphology.

The section on "Verb roots" (pp. 89–173) opens with a discussion of a particularly interesting problem. It is well-known that Altaic languages behave differently in respect to the coding of attributive words: Japanese and Korean code them as predicative (within the grammatical class of verbs), but continental languages treat them as term-words (belonging to the grammatical class of nouns). Consequently, the author sees herself obliged to establish the original coding, one that could be projected onto the Proto-Altaic stage.

In dealing with the issue of the formal definition of parts of speech, I cannot fully accept the position of Robbeets from the point of view of the contemporary state of theoretical linguistics and typology (although her assessment of the problem of syncretistic verbal-nominal stems is correct — namely, that the number of such stems in Altaic languages is vastly exaggerated by certain authors). The assertion that there are no languages without the distinction of "nouns", i. e. mainly term-words, and "verbs", i. e. mainly predicative words, should not be as simple as that: a strict, perfectly defined border often cannot be drawn between these categories, since different languages employ different sets of criteria to draw it, and in some cases it cannot be precisely defined as a certain feature that may be prescribed for lexemes in a vocabulary. It may be difficult to define such lexical classes (outside of purely semantic criteria) in a dictionary of isolating or analytical (such as, e. g., Polynesian) languages. Cf. a particularly complex case for the situation in Ancient Chinese (S. Starostin 1994).

In the place of certain terms I would have preferred more traditional ones, for example, *attributives* instead of *property words* and *stative verbs* instead of *verbal adjectives*.

P. 91: "Syntactically, they [adjectives] … can enter comparative constructions. Morphologically, adjectives make use of specific derivation patterns, such as intensifying and deintensi-

fying elements or partial emphatic reduplication". It should, however, be remembered that only *qualitative* attributives take part in comparative constructions and intensifying reduplications.

Some inaccuracies may be observed in the analysis of various coding types of certain attributives. Cf., on p. 98: "Note that some of these deverbal adnominalizers have denominal counterparts with the same form. Compare, for instance, WMo. *-KAi* in WMo. *butara-* 'fall to pieces' → *butarqai* 'dismembered' vs. WMo. *qongqor* 'hole' > *qongqorqai* 'uneven'. The observation that verbal and nominal bases can be turned into a nominally coded property word using one and the same morphological means, suggests that the concept "adjective" was originally perceived as a single category, distinct from nouns and verbs". However, *butara-* can also be analyzed as a denominal (deadverbial) verb with the suffix *-a*, and *butarqai* can be regarded as derived from the original name (adverb) *butar* 'in pieces'. Unfortunately, existing descriptions of derivational morphology in Mongolian languages often confuse denominal and deverbal models (not to mention additional semantic confusion because of imprecise English translations).

On p. 99, we have the following paragraph on Mongolian attributives: "Switched encoding. Middle Mongolian and Written Mongolian retain traces of switching, whereby the same property word can have both nominal and verbal encoding, e. g. Mmo *bulqa* 'hostile; hostility' and MMo. *bulqa-* 'to be hostile', WMo. *boγus* 'pregnant (of animals); fetus' and WMo. *boγus-* 'to be(come) pregnant', WMo. *qarsi* 'contrary, opposed; obstacle' and WMo. *qarsi-* 'to be contrary, to be opposed' (Kara 1997: 158, 160), WMo. *tasi* 'slanting' (in *tasi zam* 'slanting, uphill road') and Wmo. *tasi-* 'to deviate, slant, slope, incline (intr.)'". It deserves to be mentioned that, out of four examples, for three there can be no doubt that the original coding was nominal, since they are all Turkisms, based on borrowed nouns (*boguz* 'pregnant, in calf', *qarši* 'opposite, contrary', and *taši* 'mountain pass').

Some remarks on the analysis of the Turkic situation (3.2.5): the fact that many attributive words are derivationally deverbal does not in any way indicate proximity to verbs — many words that function as syntactic arguments are also deverbal, but that does not lead us to claim that nouns were originally verbs. Likewise, many verbs are also derived from adjectives and nouns, so this cannot serve as an argument. Formerly, N. K. Dmitriev (Dmitriev 1962: 34) proposed to distinguish in Turkic languages a separate lexico-grammatical class of qualitative adjectives which, characteristically, can be substantivized not only as an object possessing a certain quality, but also as a name for the quality itself — cf. the remarkable property of Turkic participles which can function not only as attributives but also as *Nomina actionis*. It is true that qualitative nouns can be distinguished in Turkic languages — but the difference between these two classes is of a quantitative rather than a qualitative nature; separate analysis of isolated taken syntactic constructions and derivational pairs does not provide any strict criteria to distinguish them properly. To do that, it is necessary to apply distributive-statistical methods.

One can also feel the influence of English translations on the interpretation of Turkic deverbal adjectives — p. 100: "OTk. *kïzïl* 'red' from OTk. *kïz-* 'be red'" — hardly so; a more accurate translation would be 'to glow red'; "OTk *bädük* 'big, great; greatness' from OTk. *bädü-* 'be(come) big, great'" — again, a more accurate translation would be 'to grow', while the primary meaning of **bädük* is 'high'. In fact, these verbs are not truly *stative verbs*: they show processual semantics and cannot be judged as evidence in favor of the primarily verbal character of Turkic attributives.

With respect to the remarks made above, we can make the following comment on section 3.2.6 ("Scenario for the development of Transeurasian adjective typology"). Prototypical adjectives in the world's languages are qualitative; the typology of qualitative adjectives in the languages that concern us here is by no means mixed, but rather nominal; in Turkic, Mongolian

and Tungus-Manchu they are evident nouns. In Korean qualitative adjectives are verbs and in Japanese also, those Japanese attributives that look nominal are an unproductive class (as is shown also in corresponding sections of the book under review) and, therefore, most probably residual. By the way, judging by these residual phenomena, we should rather talk of split-adjective typology for Japanese. Thus, the statement that "Transeurasian languages, at least in their earlier stages, share mixed adjective typology" is probably true, but the behavior of these systems with respect to productivity indicates that their typology was gradually changing from nominal coding to verbal, and, therefore, continental typology reflects an earlier stage (the author proposes an inverse scenario).

A small remark on table 2 ("Etymologies relating Japanese verbal adjectives to adjectives in the Transeurasian languages"): Tk *baya*(*-kI*) 'recently' is a denominal attribute name, pTk **baya* is an adverbial-attributive noun (ESTYA 2, 30); Ud. *baji* 'early' ← pTg **baji* (not **badi*!) is a noun, see SSTMYa 1: 64.

The cross-linguistic map on p. 105 (borrowed from the *World Atlas of Language Structures*, http://wals.info/chapter/118), is not very characteristic of the issue in question: it is a map of verbal / non-verbal encoding of *predicative adjectives*. Another map that provides data for a different syntactical position (Feature 60A: Genitives, Adjectives and Relative Clauses — http://wals.info/feature/60A#2/10.6/150.5), would probably be more useful, as it shows that non-verbal encoding (not as relative clauses) is inherent to many languages of the area.

This general section is then followed by an analysis of attributive and verbal root etymologies which the author traces back to the Proto-Altaic stage. As in Robbeets' previous monograph, they mostly represent polished versions of EDAL etymologies, for most of which derivational and semantic features are analyzed much more thoroughly than it was done in the source. The sound correspondences of EDAL are reduced to a smaller table that had already been set up and justified in Robbeets 2005.

A few specific remarks must be made. First, on the etymology of the aforementioned **baya* 'early' — the author states: "The expected medial consonant reflex in the Old Turkic cognate is *-d-*, according to the sound correspondence PJ **-y-* :: pK **-l-* :: pTg **-d-* :: pMo **-d-* :: pTk **-d-*. Intervocalic *-d-* in Old Turkic developed over a fricative *d* in Kharakhanid to a glide *-y-* in Middle Turkic and in some contemporary varieties, e. g. OTk *adak* > Kharakh. *adaq* > MTk *ayaq* 'foot'. In some cases, the lenition is already completed in Kharakhanid, e. g. OTk. *adaš* > Kharakh. *adaš ~ ayaš* 'foot'. If the Turkic member pTk **baya-* 'earlier, recent' belongs here, we must assume that the lenition was already completed in Old Turkic, as was the case for the initial pTk **y-* < **d-*" (p. 111). This statement contains certain errors. First, there is no such pair as Kharakhanid *adaš ~ ayaš* 'foot'; *adaš* means 'friend', rather than 'foot'.[7] Second, such South Siberian reflexes as Khakas *paja,* Shor *paja,* Tuvinian *bije,* Tofa *bijɛ*, clearly support PTk **-j-* (**-d-* would have yielded Khakas, Shor *-z-*, Tuvinian, Tofa *-d-*). The comparison in EDAL consisted of potential cognates between Japanese, TM, and Turkic, with the reconstruction of PAlt **j*; Robbeets also adds the Korean reflex *pparu-* 'to be quick, fast; early', MK *polo-* 'to be straight, fast, act quickly' vs. MK *spolo-* 'to be fast; be sharp, pointed', but it cannot be cognate to the Turkic forms.

P. 112: "Ma. *sara-* 'to become white', Ma. *sari* 'light', Evk. *se:ru:-* 'to sparkle, glitter, flash', Evk. *se:ru:n*, dial. *še:ru:n* 'rainbow' (cf. pTg **-n* deverbal noun; Section 7.5.3), Evk. *sereme* 'yellow' (cf. pTg **-mA* nominalizer; Section 7.4.3), Orok *se:rro, siro* 'rainbow', pTg **sia:ra-* 'to be light, white'". Evk. *sereme* means 'grey', not 'yellow', and it cannot be connected to Evk. *se:ru:n*

[7] Mahmud al-Kashgari mentions the form *ayaq* as a dialectal (Oghuz) variant of *adaq* 'foot', see Clauson EDT: 44.

'rainbow' because of the difference between the vowels in the roots. 'Rainbow' is reconstructed as common Proto-Tungusic **siārū(n)*, accordingly derived from the verb **siārū-* 'to sparkle, glitter, flash'. Since the vocalism does not correspond to that of the words in Manchu, the cited Manchu color terms probably do not have any equivalents in Proto-Tungusic, and may be considered Mongolisms. In Proto-Turkic **siārï-g* a diphthong should be reconstructed, since the Chuvash reflex *šur-* 'to become white' indicates a narrow vowel of the second syllable rather than *-a-* — generally speaking, the law of *i*-breaking before *a* in Proto-Turkic has not been proven yet, but, on the contrary, there are certain reasons to reconstruct a system of diphthongs or diphthong-like combinations in Proto-Turkic, see SIGTYa 2006: 159, Dybo 2007: 46–48. In Old Turkic there is no such form as *šarïg* 'yellow', see below.

P. 114: 'be high': PJ **taka-* 'to be high'; pK **teki-* 'to increase, make high'; pTg **deg-* 'to go up'; pMo **dege-* 'to be high'; pTk **yeg* 'high part; better as'. This comparison is better than the one accepted in EDAL, at least as far as the "continental" language groups and Korean are concerned. The Japanese cognate is not very appropriate because of both vowels and consonants (we would expect **d-*). It would seem better to restore Japanese **də̀-* 'good' as a correlate, since its semantics agrees well with both Korean and Turkic.

P. 117: pJ **koru-* 'be hard, painful', pK **kwolwu-* 'be hard, painful'. Here I would have preserved the PTM cognate **xurge* 'heavy' from EDAL, with a standard nominal suffix. The Turkic counterpart in EDAL, indeed, does not look very reliable, since it consists of two different entities: a) OT **Kɨr*, a noun functioning as an intensifying epithet to the words with the meaning 'enemy' — as a pejorative intensifier, the word hardly permits any reasonable hypotheses about its original meaning; b) Oghuz **Kɨr-an* 'epidemic, destruction', deverbal noun = Kipch.-Oghuz **Kɨr-gɨn* from the verb **Kɨr-* 'to destroy, exterminate'.

Section 3.3 ("Verbs") begins with a discussion of phonetic correspondences between various Altaic languages. Not surprisingly, the author mainly repeats the system already exposed in Robbeets 2005; I only have a few additional points to make.

Concerning the argument on PAlt vowel harmony (pp. 125–126): the phonetic / articulatory basis for vowel harmony can shift easily. Vowel harmony can easily disappear and reemerge, and it can also undergo typological change under the influence of neighboring languages (cf. the situation in Chuvash, Uzbek, Modern Uighur — at least according to SIGTYa 2002). As to RTR-harmony, which is currently quite fashionable and is being ascribed to nearly any vocalic system that sounds unusual for the English or the Russian ear, see Aralova 2015, where it is shown quite convincingly that for Tungus-Manchu (and the same is also correct for quite a few other languages of the world), there has really been no reliable instrumental research on articulatory phonetics so far that could demonstrate that phonological or morphophonological vowel harmony in these languages truly relies on RTR; on the other hand, for some languages whose reliance on RTR has been convincingly demonstrated, researchers have observed the opposite phonetic consequences of what is usually assumed about TM. Therefore, it seems rather premature to state with confidence that PAlt had vowel harmony and, moreover, that it was based on RTR articulation.

As to the vocalic correspondences in EDAL, it is true that they have not been elaborated to perfection — at the very least, Proto-Turkic vocalic reconstruction has not been carried out rigorously in all comparisons; and for Proto-Tungusic, a slightly simplified reconstruction from S. Starostin 1991 was taken (at least for PTM, we currently prefer to reconstruct vowel harmony). However, EDAL took an approach that increased the explanatory power of the reconstruction, namely, assuming the possibility of the influence of the second vowel on the first vowel in bi- or polysyllabic stems. Robbeets completely omits this part — and, therefore, it is completely unclear (table 17), for instance, how MK *kùt-* 'to be hard' (*kwut* in Yale notation)

can be traced to pK **kata-*; even with the reference to table 16, it is still noted that MK *wu* < PK **u* (in EDAL this development is accounted for by setting up a labial vowel in the second syllable: PAlt **kʻétʻò*[8]). On the other hand, the internal distribution of Japanese vocalic reflexes proposed by Robbeets (pp. 127, 130, 131) is original and deserves special attention.

Pp. 89–173 are given over to the analysis of verb etymologies. These largely represent refined versions of EDAL comparanda, usually without comments, and often with parts of previously included comparanda removed from the etymology for various reasons. In general, we can agree with many of the edits; a particularly important step forward compared to EDAL is the author's attempt to provide derivational analysis for the compared forms (the same attempt was also made in the section on adjectives). However, in the process some minor inaccuracies in the analysis of continental data still managed to creep in, cf. some examples:

Table 27: The Ancient Turkic runic form is given as *šarïg* 'yellow', however, *śarïγ* (as in EDAL) would be more accurate. Earlier, it was implied (Mudrak 1988) that the postulation of the Proto-Turkic diphthong, primarily based on Chuvash palatalization, could also be supported by cases where in Orkhon runic inscriptions we see consonants, usually typical for words with front vocalism, in words with back vocalism (although the hypothesis remains questionable). In any case, it is the Chuvash form that should have been listed here — the testimony of Ancient Turkic is much weaker;

p. 136: (on **sïp-*, **sïp-kar-* 'to swallow') "The formant in Mtk *sipqar-* and Az. *sïfqar-* is probably the lexicalized causative pTk **-gAr.* The lack of voice is explained by the fact that the opposition /k/:/g/ is very weak after consonants in Old Turkic". Actually, the lack of voice is explained not by the weakness of the opposition, but by the synchronic rule for the selection of affixal allomorphs — one that still functions in modern languages, as well as in Old Turkic;

p. 149: *nebse-yi-* 'to be wide and long (of clothes)' (cf. also p. 109: *muru-yi-* 'to be bent'): *-yi-* is not a deverbal affix (and not anti-causative either: an anti-causative verb is an *intransitive verb* that shows an event affecting its subject, while giving no semantic or syntactic indication of the cause of the event), but a denominal affix with the meaning of a stative verb, i. e. intransitive qualitative verb; it is used, in particular, for deriving pro-verbs from pronouns (*te-yi-* 'to do so', *ka-yi-* 'to do what?'). See Chuluu Ujiyediin 1998: 67–68, where it is stated that this affix forms verbs of regular meaning (implying *neutral Aktionsart*) from adverbs, with *nebse-yi-* as one of the examples). The same source quite plausibly explains the formation of deverbatives with *-gar* as derivatives from these verbs, with regular omission of the suffix. Thus, these derivatives do not confirm the verbal nature of the original stem.

In the note 18 on page 151 the author seems to misunderstand the development rules for PTM **ö* and **u*, the way they were conceived by Benzing. She provides the following table ("according to the correspondences in Benzing 1955"):

PTg	Ma	Evk	Even	Sol	Neg	Oroch	Ud.	Olch.	Orok	Na
*ö (*ʌ)	u	u	o	u	u	o/u	o	o/u	o/u	u
*u	u	u/-ï[9]	u/-ï	u/-ï	u/-ï	u	u	u	u	u

[8] Although I would agree with Robbeets on the elimination of Manchu *etu-xun* 'strong, hard' from the comparison.

[9] *-ï* appears here because of the second syllable of the trisyllabic form **aduli* 'net', which Benzing himself did not reconstruct accurately enough. According to the materials, in his reconstruction it should have been **adüli*; I would now prefer to reconstruct this form with a diphthong (**adujlï*).

Benzing himself does not provide a table of vowel correspondences, but on pp. 23–24 he discusses examples which, according to his opinion, should have either one or the other of these vowels in the protoform. If we draw a table for these examples (see also more accurate transcriptions for the modern languages in SSTMYa and other more recent sources), it will look as follows ("MF" means morphophonological frontness, "MB" means morphophonological backness):

PTg	Ma.	Evk.	Even.	Sol.	Neg.	Oroch	Ud.	Olch.	Orok	Na.
*ö (*ʌ)	u, e/P_	u (+MF)	ɵ/u (+MF)	u (+MF)	ɵ/u (+MF)	u (*CuCu > CoCo)	o/ɵ	u (*CuCu > CoCo)	u (*CuCu > CoCo)	u
*ō̃	uwe, we/0_	ū (+MF)	ɵ/u (+MF)	u (+MF)	ɵ/u (+MF)	u	o/ɵ	u	u	ū
*u	u	u (+MB)	u (+MB)	u (+MB)	u (+MB)	u (*CuCu > CoCo)	u	u (*CuCu > CoCo)	u (*CuCu > CoCo)	o

Thus, the first syllable in Benzing's system for PTM **töru-* 'to hold' is reconstructed correctly, but it could be reconstructed much more reliably from such forms as Evk. *туринмукэн-* 'to restrain smb. with smth.', where the vocalism clearly reveals the property of morphophonological frontness (*-ken-*, and not *-kan-*). As for the second syllable, I would rather reconstruct its PTM vocalism as **i* than **u*: labialization in Even is due to labial attraction, and in some other languages it may be due to the accommodation of the causative-passive suffix **-bu-* → *-wu-* and similar (see SSTMYa 2: 330). (Additionally, I myself now conceive the PTM vocalism somewhat differently — see the system of correspondences in my article "Tungus-Manchu languages" in BRE 31, forthcoming in 2016).

The section on copulas (3.4, pp. 153–163) seems flawed inasmuch as a whole mix of different Turkic formants is traced back to the same existential verb **ā-*. From a morphosyntactic perspective, this seems reasonable for the denominal verbalizer *-A-*, as well as for the formant *-A-* in the durative participle *-A-gAn*, but hardly makes sense for the deverbal nominalizer (actually, a future participle) *-Ar* and the *-A*. In fact, even the denominal verbalizer *-A-* is not a perfect candidate, since it forms both transitives and intransitives.

Justification of low probability of verb borrowing in Transeurasian languages because standard borrowing strategies do not coincide (pp. 168–169) does not seem very convincing to me, since borrowing strategy as a typological feature can vary broadly and evolve in the course of language history — who really knows which particular strategies were preferred by the ancestors of Japanese and Korean peoples in the 2nd millennium BC?

Without going into too much detail on the analysis of verbal markers (pp. 174–484), constituting the central part of the monograph, it may be said that, on the whole, it looks fairly convincing; however, it would be desirable to strengthen the paradigmatic approach with a more detailed study of the development of each marker in all the categories of every language group.

Negation in Altaic, in particular, is analyzed quite thoroughly. We definitely agree that (p. 207) "the indications of inheritance are stronger than those of diffusion". In addition to simple correspondences between "nasals", we have a rigorous vocalic correspondence and a well-established grammatical class. One remark: on p. 205 it is supposed that the reflex of the PAlt negative verb **an-* in Old Turkic is *anïg ~ aṅïg ~ ayïg* 'evil, sin' as a deverbal noun in *-g-*; however, this scenario does not work, since the Turkic word should clearly be reconstructed with palatal **-ń-*, which would contradict at least the Tungusic reflex **-n-*.

"Verbalization and Actionality" (pp. 209–270): the title of this chapter is somewhat confusing, since, in addition to *Aktionsarten*, it also covers the subject of denominal verb derivation, and out of all the affixes that are discussed only one (PAlt **ga-* 'inhoative') carries the semantics of action development. The reference to Bybee 1985: 100 on p. 209: "the semelfactive in Russian *kašljanut'* 'to cough' and *blesnut'* 'to flash' is situated halfway between lexical and derivational expression because these stems do not occur without the element *-nu-*, which prevents us from identifying *-nu-* as a suffix" is quite suspicious — actually, in these verbs the suffix *-nu-* can be very easily separated from the root, cf. the respective forms without this suffix: *kašljat'*, *blestet'* (with the phonetic development *-tn-* > *-n-*). Even if this is a minor remark, it goes a long way in showing how unsafe it can be to rely blindly upon typological data without the necessary precautions.

The analysis of verbal derivation affixes is mostly excellent; however, the chronological reasoning (pp. 225–226) which is used as an argument in favor of the archaic character of the affixes, looks a bit naïve. In particular, Robbeets refers to Bakker and Hekkig 2012, who, based upon the material of Spanish loanwords in Quechua, Guarani, and Otomi, have established the time period necessary for the borrowed derivational affixes to become productive as equal to 500 years. But why should we think that this time period is necessarily universal? Let us assume that the deverbal noun suffix *-izirova-*, borrowed from German, becomes productive in Russian some time around the 1920s, while the stream of German loanwords that brought this suffix begins about mid–1700s; this puts an upper limit of 170 years (probably even less, about 150 years) that was needed in order to make the German suffix *-isier-*(*en*) (borrowed from French) productive in Russian. If we accept 150 years as a possible term for becoming productive, the chronology, presented on p. 226, would change in the following manner: ther table below depicts chronological conflict in Vovin's borrowing scenario of the deverbal noun suffix **-la*.

Stage in borrowing process	Example	Estimated date
Proto-Turkic original	OTk. *boguz* 'throat' → *boguzla-* 'to cut the throat (tr.)'	before 100 BC
Mongolic borrows Turkic verbs	No base → MMo. *bo'orla-* 'to cut the throat (tr.)'	after 100 BC
productivity	pMo **-lA-*: WMo. *cegeji*(*n*) 'memory' → *cegejile-* 'to memorize (tr.)'	after 50 AD (instead of Robbeets' 400 AD)
Tungusic borrows Mongolic verbs	No base → Ma. *šejile-* 'repeat by heart'	after 50 AD (instead of Robbeets' 400 AD)
productivity	pTg **-lA:-*: Ma. *gucu* 'friend' → *gucule-* 'to make friends'; Even *tew* 'berry' → *tewle:-* 'to gather berries'; Ud. *anda* 'friend' → *andala-* 'to make friends'[10]	after 200 AD (instead of Robbeets' 400 AD)

It is much closer to the chronology of split that was proposed by the author (p. 225–226: "These estimated dates for productivity, summarized in Table 1, conflict with the real dates of productivity proto-Khitan-Mongolic and proto-Tungusic. Since Khitan preserves reflexes of **-lA-*, the suffix can be traced back to the common ancestor of Khitan and Mongolic, i. e. before 180 AD. This argument is even stronger for Tungusic: as all contemporary Tungusic languages reflect **-lA-* it must have been productive in proto-Tungusic, i. e. at least before 220 AD").

[10] Again, though, the Ud. example is inappropriate, since *anda* 'friend' was borrowed from Mongolic.

It is also hard for me to agree with the assessment of the PTk reflexive *-Xn* as a reflex of the PAlt verbalizer **-na*. The argument on p. 237 ("Although the majority of *-Xn-* derived verbs do not refer to a direct object, some verbs, such as OTk. *basïn-* 'to come under stress (intr.), impose restraint on oneself (intr.), oppress, repress (metaphorically) (tr.),' can become transitive when used in a metaphorical, benefactive sense. This semantic modification without impact on the valency of the verb can serve as an indication that we are dealing with an original actional suffix instead of a diathetical marker") does not look convincing, since this is the only typologically expectable behavior of reflexives and medials — cf. the situation in Latin and Ancient Greek; also Nedyalkov, Geniushene 1991: 251–272. Actually, we could limit this comparison to only a small number of Chuvash denominative verbs in *-n*, and this, taking into account the essential possibility of ambiguous verbal-nominal roots for Turkic languages, does not look too promising — most of such derived verbs in Chuvash are intransitive (cf. Levitskaya 1976: 166–167).

The conclusion on the drift of verbalizers from nouns via adjectives to verbs undoubtedly looks interesting, but we have to remark that it contradicts the author's own thesis on the primarily verbal nature of adjectives in Altaic languages.

Voice affixes are very expertly analyzed (pp. 271–328), in particular because the author had the opportunity to rely on mostly accurate solutions in earlier research literature on Altaic languages. (Technical remark: for some reason, the left running title has "spread" from this chapter over to chapter 7, "Nominalization and the development of finite temporal distinctions", pp. 330–449, which made navigation across the book more complicated).

Chapter 7 begins with a section on the typology of "finitization". I should say that, as a specialist in continental Altaic languages, I find most of its argumentation somewhat superfluous for diachronic research, since it is fairly obvious that almost all finite forms in Tungus-Manchu, Mongolian, and Turkic languages are not distinguished (or, at least, have not been distinguished until very recent times) from the corresponding forms of secondary predications (deverbal nouns, participles, gerunds — all carrying the same TAM meaning), i. e. they can be viewed simply as nominal predicates. The only difficult moment is the origin of the Turkic preterite in *-dI*, which, among other things, demonstrates (as well as the conditional mood in *-sa-*) a different system of personal endings that could possibly represent some archaic relics of a pre-Altaic state. For this form, however, Robbeets does not propose any Altaic parallels.

Again, the study of the etymologies of common Altaic deverbal nouns is conducted very accurately. Some problems remain as far as Mong. and Turk. reflexes of PAlt **-k̓a* are concerned: as we mentioned earlier, Robbeets does not take into account the currently accepted distinction between PMo. **-g-* and **-γ-* (or **-ʔ-*)[11]. Likewise, it is difficult to distinguish between Turkic **-k-* and **-g-* at morpheme boundaries; this can probably be done only with the help of indirect information (such as the existence of duplicate affixes with deleted *-g-* — but then, how legitimate would it be to match them?). Voiced and voiceless gutturals outside the first syllable in simple stems in modern Turkic languages (upon which, as Robbeets notes on p. 413, transcriptions for Ancient Turkic forms are usually dependent) are mostly the results of secondary development of voiceless consonants — automatic intervocalic voicing in South Siberian languages; voicing when preceded by a sonorant after primary long vowel and in the position more than one syllable away from the beginning of the word in Oghuz lan-

[11] This opposition does not have a fully straightforward correlation with the dropping of the velar consonant in modern Mongolic languages. The conditions, nevertheless, are quite definite (e. g. **-γ- > -g-* by dissimilation if the wordform contains another **-ɣ-* or has a glide-containing diphthong, according to the so-called Vladimirtsov's rule), ruling out possible accusations of irregularity on behalf of our anti-Altaicist colleagues.

guages; lack of voicing after sonorant and primary short vowel of the first syllable in Oghuz languages (for even more details see the section on Oghuz morphonology in SIGTYa 2002: 96–103), etc. Voicing — but not dropping — of the initial guttural in affix after vocalic stem endings speaks in favor of reconstructing **-k-*, whereas dropping rather indicates **-g-*, so here we would expect different consonants for different affixes, as we have it in Mongolian languages.

As for the inclusion of the conditional marker **-sAr* into the group of reflexes of the PAlt nominalizer **sa*, this seems more dubious than the inclusion of the Chuvash future participle marker *-As*, which corresponds to one of the common Turkic present tense markers, appearing after the negative marker: (*-mA-*)*s* (along with (*-mA-*)*z*, (*mA-*)*r*), see Levitskaya 1976: 85–87, Kormushin 1984: 29.

On the whole, the analysis gives a very favorable impression: in particular, the author succeeded in making some plausible conjectures about the combinatory potential of every reconstructed affix (p. 443). The same applies to the reconstruction of two Proto-Altaic gerund affixes — **-i* and **-kʻu*, although, as far as accuracy of the reconstruction of the initial consonant in the second affix is concerned, see notes above on PAlt **-kʻa*.

In summary, the author succeeded in reconstructing, quite accurately and plausibly, the form and functionality of 19 verbal markers. Although all of these etymologies, wholly or in part, had already been published in previous literature on Altaic languages, Robbeets has managed to make a more precise selection of cognates, based on careful analysis of morpheme usage in separate languages and language groups, and in a number of cases successfully proposed new specific affinities between morphemes and realistic typological foundations for change in usage and semantics, postulated for certain etymologies. The author is correct in stating that the groups of reconstructed affixes form a kind of paradigmatic relationship. Of course, a more serious discussion on the reconstruction of paradigms will only be possible once we have reconstructed for a grammatical category the entire history of the development of grammatical affixes from Proto-Altaic to all of the individual languages, i. e. when we have shown how all of the systems reflecting it in descendent languages were formed. In some cases this seems possible, but should be relegated for future research. At the present time, complete success in this direction seems unlikely, considering that, overall, the morphology of Altaic languages is mostly non-paradigmatic.

We can fully agree with the author's explanation of qualitative and quantitative differences between Altaic and Indo-European reconstruction (section 9.4: "Why is the evidence not consistent with the Indo-European model?"). Indeed, it would be strange to expect from the Altaic reconstruction "clearcut inflectional paradigms in the core parts of nominal and verbal morphology" which are demanded, for instance, in Janhunen 2014: 3 (it should also be noted that Janhunen's conception of the degree of successful reconstruction for Proto-Indo-European paradigms is somewhat exaggerated). Nevertheless, we must acknowledge that a major step forward towards a better grounding of the morphological reconstruction of Proto-Altaic has been undertaken by Martine Robbeets in this book.

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А. В. Дыбо. Новое в европейской алтаистике.

Статья посвящена обсуждению текущих актуальных проблем алтайского исторического языкознания размышлениям, преимущественно завязанном на критической оценке двух больших монографий Мартины Роббеетс — об алтайском происхождении японского языка (Robbeets, Martine. 2005. *Is Japanese related to Korean, Tungusic, Mongolic and Turkic?* Wiesbaden: Harrassowitz) и о подтверждении алтайской гипотезы на материале сравнительной глагольной морфологии (Robbeets, Martine. 2015. *Diachrony of verb morphology: Japanese and the Transeurasian Languages*. Berlin: Mouton de Gruyter). Наряду с анализом основных методологических положений и отдельных этимологических решений М. Роббеетс на конкретном материале рассмотрен и подвергнут критике ряд тезисов, общих для антиалтаистического направления.

Ключевые слова: алтайские языки, историческая тюркология, глагольная морфология, дальнее родство языков, история японского языка, этимология.

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Arabic in the context of comparative studies*

The paper discusses certain characteristics of Arabic that define its position in Semitic comparative studies and are determined not only by Arabic language structures at different levels, but also by extra-linguistic factors, viz. socio-cultural and psychological. This combination of both linguistic and extra-linguistic factors makes Arabic a special phenomenon among languages of the world. Before discussing the place of Arabic in Semitic studies, I present a brief overview of the history of intra-Semitic comparisons prior to the emergence of contemporary comparative linguistics and to the role of Arabic medieval grammar tradition in this respect. The next section focuses on the role of Arabic as a model for proto-Semitic reconstructions and on the drastic changes that it underwent over the history of comparative Semitic studies. The last section discusses certain specific features of Arabic and Semitic phonetics and lexicon and their correlation with the standard Neogrammarian paradigm of comparative linguistics. These issues deserve special attention, since theories based on these phenomena contradict the standard paradigm of comparative linguistics, and the Arabic language may be regarded as an archetypal case of these phenomena.

Keywords: comparative linguistics, Semitic languages, Arabic, Neogrammarian paradigm, regular correspondences, reconstruction, binary opposition, perfective aspect, imperfective aspect, historical typology.

In this paper, I would like to discuss the place and role of Arabic in Semitic comparative studies and historical reconstructions. In the beginning we shall focus on the history of intra-Semitic comparisons and the role that Hebrew-Aramaic biblical studies and medieval Arabic grammar traditions play within this framework. Following that, we shall discuss the status of Arabic as a model in Semitic reconstructions at different stages of development of Semitic comparative historical linguistics. In the early period of modern Semitic studies standard Arabic was duly considered the most archaic of the living Semitic languages. Although the task of establishing regular sound correspondences between classical Semitic languages was accomplished, these correspondences were, in fact, graphic and not phonetic in the proper sense of the word; their phonetic value was established mostly on the basis of classical Arabic pronunciation. Only at the later stages were the data from Modern South Arabian and Ethiopian Semitic included into comparative Semitic studies on par with classical languages, and this brought serious changes to the comparative panorama of Semitic. Special attention will be paid to the reconstruction of the Semitic verbal system according to the model worked out by the present author, as well as the place of Arabic in this model. Finally, particular attention will be devoted to the evolution of the status of Arabic as a model for proto-Semitic reconstructions. The last part of the paper is dedicated to different attempts to consider certain features of Arabic and Semitic phonetics and lexicon as a source for inferring glottogonic processes. These trends contradict standard Neogrammarian paradigms and deserve special discussion.

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1. It is a matter of general knowledge that comparative studies and linguistic reconstructions emerged at the beginning of the 19th century on the basis of Indo-European languages. This was quite logical, since the new scientific theory was essentially based on data from European languages, well known to European scholars. Moreover, the notions of basic European ethno-linguistic units (such as Celtic, Germanic, Slavonic, etc.) had also been formed by that time. These notions were mostly based on general impressionistic criteria that included language, culture, oral traditions etc. (cf. the classification of languages by J. Scaliger and the development of this approach by G. W. Leibniz). A major breakthrough, i. e. the formation of a new linguistic discipline — comparative historical linguistics — was triggered by the inclusion of Sanskrit into European philological discourse.

As to Semitic languages, we already attest them in the earliest attempts at language comparison; suffice it to mention *Targumim* (Aramaic translations of Biblia Hebraica). Édouard Dhorme, one of the eminent scholars in the field of Semitic and Biblical studies, had noted in his introduction to the Pléiade French version of the Old Testament that these Aramaic versions were rather interpretations than mere translations of the Hebrew Holy Script [Dhorme 1956: XXV]. The text of the Biblia Hebraica abounds in '*dark passages*', *hapax legomena* etc. It means that generations of highly trained scholars minutely and thoroughly studied and compared every word and every sentence in these two closely related languages in order to understand and comment on every letter of the Biblia Hebraica. We should emphasize here that Biblia Hebraica includes a rich collection of different texts (prosaic and poetic, philosophical and historical), created over the span of many centuries; there is arguably no other example of such a deep, intensive and protracted tradition of text collation. As a sidenote, it may be added that the famous Biblical episode of "*shibboleth ~ sibboleth*" (Judges 12,6), which in all probability is the earliest attested case of the use of phonetic isoglosses for ethnic differentiation, is due to this tradition of text collation. In this case, it is not an instance of an Aramaic vs. Hebrew opposition, cf. the comment to this passage by Édouard Dhorme: "La population d'Éphraïm se distinguait des autres tribus par une prononciation défectueuse de la chuintante *shin* qui devenait *sin* dans leur bouche [Dhorme 1956: 770, footnote 6].

Traditions of Aramaic–Hebrew comparisons developed in the epoch of medieval Arabic and Hebrew grammatical schools. Medieval Semitic grammatical traditions first emerged within the framework of Arabic studies, but very quickly began to include Hebrew as well. The main principles, notions and paradigms of medieval Arabic grammars were successfully applied to Hebrew data. This symbiosis was so deep that there were instances of writing in Arabic using Hebrew letters and the other way round; consequently, Arabic also began to be included into Hebrew-Aramaic comparisons. Thus, "Risāla", the major work by Yehuda ibn Quraish (10th century AD), is divided into three parts:

— comparison of Hebrew and Aramaic;
— explication of 17 hapaxes;
— comparison of Hebrew and Arabic (Cassuto 2007: 17).

I think there is every reason to consider Arabic-Hebrew medieval grammar traditions as the Golden Age of comparative Semitic studies, although this by no means signifies that these medieval grammar traditions should be considered a part of contemporary comparative linguistics. The point is that they are more extensive and better developed than those that existed in the European philology of the same period. It may be added that many of the principles and ideas of medieval Arab grammarians are still present in modern linguistic discourse.

Nevertheless, the theory and methodology of modern comparative historical linguistics was eventually worked out by specialists in Indo-European languages, based on European

philological traditions. By the end of the 19th century, a new theoretical approach to comparative analysis was codified by a group of specialists in Indo-European linguistics known as Neogrammarians (Young Grammarians, Junggrammatiker); the key point of their theory was identified as the principle of regular sound correspondences, metaphorically labeled 'sound laws'. From that time on, Neogrammarian principles have functioned as the base paradigm of comparative linguistics (on paradigms in the sense of [Kuhn 1962], see additional notes below).

2. The task of establishing regular sound correspondences between classical Semitic languages was accomplished without major problems. Incidentally, regular sound correspondences served as a solid base for deciphering and reading of the ancient written monuments in different extinct Semitic languages, which in its turn supplied new data for comparative studies. However, as a matter of fact, these correspondences were graphic, rather than phonetic in the proper sense of the word. The phonetic value of the graphemes used in ancient Semitic writing systems and incorporated into the system of regular sound correspondences was established mostly on the basis of traditional Arabic pronunciation, which served as a model for the common Semitic phonetic system. This was one of the reasons why in the early period of Semitic studies classical Arabic was considered the most archaic among the living Semitic languages. Moreover, medieval Arab grammarians had left very good descriptions of classical Arabic pronunciation. All of this made the Arabic language extremely important for comparative Semitic linguistics.

Another characteristic also contributed a lot to the status of Arabic as a model for proto-Semitic reconstructions — namely, the remarkable stability of its consonantal root structures, which are practically not liable to conditioned phonetic changes (assimilations, dissimilations etc.). Even more striking is the presence of complex, but perfectly transparent and consistent Arabic morphological structures with minimal exceptions. During the earlier stages of historical studies language structures of this type were considered as the most archaic, even prototypical, not "spoiled" by later development (cf. the status of Sanskrit in early Indo-European studies). However, gradually it became evident that such morphological structures may rather result from intensive processes of analogical leveling, with an additional role played by the efforts of medieval philologists in the codification of classical Arabic (and similar reasoning may be applicable to classical Sanskrit).

Only at the later stages of the development of Semitic linguistics were the data from Modern South Arabian and Ethiopian Semitic included into comparative Semitic studies on par with classical languages, and this brought serious changes to the Semitic comparative perspective. Arabic could no longer be considered as the privileged model for phonetic reconstructions, even though generations of Semitologists continued to reject the idea to consider non-written languages of Southern Arabia, Soqotra, and Ethiopia of equal importance for proto-language reconstructions with the classical extinct languages of some of the greatest world civilizations and religions. This drastic change of approach to Semitic reconstruction led to two most important reconsiderations:

— reconstruction of glottalized emphatic consonants instead of pharyngealized ones (the latter reconstruction was based on traditional Arabic pronunciation);

— reconstruction of lateral sibilants on the basis of Modern South Arabian pronunciation. Incidentally, this reconstruction allowed to explain the historical phonetic value of Hebrew *sin* and Arabic *¥ad*.

The historical shift from glottalization to pharyngealization in Arabic can be accounted for by the affricate theory worked out by Igor Diakonoff (1988: 36–39). According to Diakonoff's

reconstruction, phonemes that were traditionally interpreted as sibilant fricatives actually reflect of Proto-Semitic affricates. Phonetically, glottalized consonants are double-peak (or bifocal), the second occlusion being the glottal stop; turning into fricatives, affricates lost the main occlusion. It is true that glottalized sibilants are attested in certain languages (cf. glottalized *s*' in some Hausa dialects); however, they still cannot be considered as "proper" fricatives, since they preserve the glottal stop. This phonetically awkward situation could quite naturally lead to the shift from glottalization to pharyngealization in emphatic sibilant fricatives, whereas occlusive emphatics remained glottalized. Such a situation is attested in Modern South Arabian (Naumkin, Porkhomovsky 1988: 12–13). In Arabic, emphatic plosives also lost glottalization due to analogical change.

Turning to morphology, I shall focus on the verbal system as the key aspect for comparative studies and historical reconstructions. Together with North-Central Semitic (Hebrew, Aramaic, Phoenician etc.), Arabic was considered as a prototypical morphological model during the first decades of comparative Semitic. Later, with the progress of Assyriology, data from Akkadian language stock were included into comparative Semitic studies. Despite some obvious parallels, Akkadian verbal morphology on the whole is not historically compatible with Central Semitic, yet it also could not easily be explained away as a secondary development because of its obvious antiquity. Thus, for a certain time two incompatible morphological models co-existed in Semitic comparative linguistics.

This problem was the main reason for a paradoxical statement by A. Meillet:

> … toutes les langues indo-européennes sont des formes différenciées d'une seule et même langue… Les langues sémitiques sont plus semblables entre elles que ne le sont les langues indo-européennes ; à les observer, on a souvent l'impression de formes diverses d'une même langue plutôt que de langues vraiment différenciées, comme le sont les langues indo-européennes ; et malgré cela, on n'arrive pas à poser un "sémitique commun", un *Ursemitisch*, comme on pose un "indo-européen commun", un *Urindogermanisch*. En particulier, l'akkadien (babylonien) offre des traits qui diffèrent tout à fait de ceux qu'on observe dans le groupe de l'hébréo-phénicien, de l'araméen, de l'arabe. (…) Néanmoins, la famille sémitique — y compris l'akkadien — est nettement définie, et l'on a ici un ensemble qui est aisément reconnaissable, plus même que ne l'est celui des langues indo-européennes. (Meillet 1927: 445)

Still later, the data from Modern South Arabian and Ethiopian Semitic formed a third nucleus in the common Semitic verbal panorama, making the whole situation even more paradoxical. There were numerous attempts to preserve the traditional approach by interpreting fully vocalized prefix-conjugated Imperfective[1] forms in Modern South Arabian and Ethiopian Semitic as a later secondary development and not as genetic isoglosses with similar Akkadian forms; were these isoglosses accepted, Arabic, Hebrew, and other Central Semitic languages would lose their status of archaic, even prototypical Semitic languages — instead, it would be necessary to consider them as the most innovative languages in the field of verbal morphology, even more innovative than unwritten Modern South Arabian or Ethiopian Semitic languages. Earlier, I have proposed (see Porkhomovsky 1997, 2001/2, 2008) a new model of reconstruction for the Proto-Semitic verbal system which was based not on the traditional approach, viz. comparative analysis of forms according to Neogrammarian standards, but rather on diachronic typology. Reconstruction based on diachronic typological analysis of the respective morphological paradigms rather than individual forms was suggested as the first diachronic

[1] It should be noted that in the discussion below the terms "Perfective" and "Imperfective" are used as conventional labels for members of the basic binary aspect opposition. In specific languages they are often assigned temporal semantic values, i.e. "Past" and "Present" respectively.

step, after which it was possible to explain the changes in verbal systems as a development of particular Semitic language groups or individual languages.

This diachronic typological model may be summarized as follows. Within the framework of the postulated common Semitic binary opposition "Perfective vs. Imperfective", both members were prefix-conjugated with full vocalism in Imperfective (formed by *-a-*Ablaut in the first syllable and gemination of the second root consonant in certain languages) and reduced vocalism in Perfective. In derived verbal stems (stirps) the situation may be more complex. The form of Perfective was weak (unmarked); it was also used in special syntactic constructions, in negative constructions, and as Jussive/Subjunctive, at the same time preserving its semantic value of Perfective. This situation demanded the formation of a new strong Perfective. Such an archaic situation is attested in Akkadian where the new Perfective is based on the derived verbal form with the infix *-t-*. In all other Semitic languages the new Perfective is a suffix-conjugated form, parallel to Akkadian Stative or Permansive (which is not a finite verbal form in Akkadian). In Modern South Arabian and Ethiopian Semitic the old prefix-conjugated Perfective with reduced vocalism is used only as Jussive/Subjunctive. The fully vocalized prefix-conjugated Imperfective is preserved.

Arabic and other Central Semitic languages represent a new step in the development of the verbal system. Since the opposition between Perfective and Imperfective has come to be expressed by the opposition of suffix- versus prefix-conjugated forms respectively, the existence of two prefix-conjugated forms became redundant. The fully vocalized form was lost and the form with reduced vocalism preserved its functions as Jussive/Subjunctive, but also acquired functions of Imperfective. However, in certain cases it preserved its original Perfective functions: as negative Perfective (after the particle *lam*) in Arabic, in constructions with *waw consecutivum* in Hebrew, in archaic poetic texts in Ugaritic and Hebrew, after new suffixed Perfective in certain homogeneous constructions in Arabic (see more details and a complete presentation of this model of reconstruction in Porkhomovsky 2008). Further development of the basic binary opposition "Perfective vs. Imperfective" is well attested in modern Arabic dialects and in Tigrinya (Ethiopian Semitic). The same typological evolution is repeated in these languages for the second time. In Tigrinya the new strong suffix-conjugated Perfective became a weak unmarked member of the opposition, and this led to the formation of a new marked suffix-conjugated Perfective on the basis of the historical Gerund (nominal form).

It could be surmised that within the framework of the Semitic opposition 'Perfective vs. Imperfective' the perfective form is always weak (unmarked). However, evolution of the Arabic verbal system does not allow for this interpretation. As in all Semitic languages at the first stage of morphological evolution, Perfective in Arabic became weak, and a new suffix-conjugated Perfective emerged. But in modern Cairene Arabic the Imperfective member of the basic opposition "Perfective vs. Imperfective", i.e. the prefix-conjugated form with reduced vocalism, became weak, and a new marked Imperfective emerged, formed by the prefix *b-*, added to the existing prefix-conjugated form with reduced vocalism. Since the form of Imperfective in classical Arabic is a reflex of the old weak Perfective, this evolution indicates that it is not the aspect semantics that determines which form becomes unmarked in the basic aspect opposition, but the decisive role is actually played by a formal criterion: the prefix-conjugated verbal form with reduced vocalism is the primary finite verbal form in Semitic, and all the other forms are derived from it. Thus, this form is a weak (unmarked) one *par excellence*.

In Tigrinya (as in all Modern South Arabian and Ethiopian Semitic languages) this form was pushed out of the aspect opposition and has retained only modal functions. In other words, the derived prefix-conjugated Imperfective form with full vocalism remained marked in Tigrinya, whereas the more simple suffix-conjugated Perfective form became weak. It may

be added that the process of formation of new finite verbal forms within the Imperfective semantic field on the basis of participles, as attested in modern Arabic dialects (i.e. in Tunisian and Egyptian), belongs to the same diachronic typological trend.

3. The Neogrammarian paradigm (in the sense of [Kuhn 1962]) is valid only for the phonetic level, i.e. the establishment of regular sound correspondences. The format of the present paper does not allow for a detailed discussion of Kuhn's model of scientific evolution. Applied to linguistics, Kuhn's model means that comparative historical studies which do not tally with Neogrammarian principles are not compatible with standard (paradigmatic) comparative linguistics (cf. more on this in [Porkhomovsky 2013]). The standard approach is based on two axioms:

1) arbitrariness of the linguistic sign (with the exception of onomatopoeic words and *Lallwörter*);
2) uniqueness and continuity of the diachronic transmission of languages to new generations of speakers. This principle presumes the possibility of reconstructing only one proto-language for genetically related (parent) languages. The existence of mixed languages is not allowed for by this axiom. (The situation with pidgins and creoles deserves special discussion in this respect, but it lies outside the scope of the present paper.)

As to the higher (viz. morphological, syntactic and semantic) language levels, comparative historical studies at these levels cannot be considered paradigmatic in Kuhn's sense, since their linguistic data generally allow for multiple interpretations. The same applies to the problem of genealogical classifications: absolute classifications, which determine the very fact of genetic relationship, belong to the paradigmatic sphere of comparative linguistics, since absolute genetic status is determined on the basis of regular sound correspondences. On the contrary, the internal classification of parent languages into branches, groups and subgroups according to the genealogical tree model does not belong to the paradigmatic sphere of comparative linguistics because it depends on the interpretation of established isoglosses. The main problem here is to differentiate between genetically-based isoglosses and areal ones within the groups of related languages.

It is obvious that after the formation of the Neogrammarian comparative paradigm it became possible and necessary to distinguish between paradigmatic and non-paradigmatic approaches to historical linguistics, since they are not compatible and the same terms may reflect different notions. The non-paradigmatic approaches do not conform either to one of the axioms of the Neogrammarian paradigm mentioned above, or to both of them.

These non-paradigmatic models are quite numerous and widespread in historical linguistics. The reason for this obviously lies in the fact that comparative studies on levels higher than phonetic are not paradigmatic, so they allow for alternative approaches to genetic relationship. One of the earliest and the most influential is the conception of mixed languages, usually associated with the name of Hugo Schuchardt. Various linguistic schools and numerous authors belong to this trend in historical linguistics, e.g. the Italian neolinguistic school; one of the latest examples of this approach is R. Dixon's theory of punctuated equilibrium (Dixon 1997). These non-paradigmatic trends in diachronic language studies are usually based on typological and areal arguments.

Another theoretical approach to linguistic reconstruction, based on the epistemology of positivism, consists in the interpretation of the results of comparative studies only as sets of correspondences between languages. Forms not attested in real languages, extinct or living, are not taken into consideration, hence reconstructions of proto-languages are excluded from scientific analysis. In principle, this approach does not contradict the Neogrammarian para-

digm. Incidentally, Antoine Meillet, a prominent figure in Indo-European comparative studies, was a proponent of positivism.

All these non-paradigmatic theories and hypotheses are often applied to linguistics as a whole, irrespective of what particular language families are involved. However, they are much more widespread in the comparative studies of language groups without long written traditions that were only recently included into professional linguistic analysis. This approach is less popular in relation to language families with long and rich written traditions that present abundant material for reconstruction of language archetypes.

Semitic languages make an obvious exception to this case. The idea to reconstruct Proto-Semitic archetypes was quite often met with reserve or even objected to throughout the history of Semitic comparative studies by numerous scholars, beginning with Carl Brockelmann and his predecessors and ending with contemporary authors. Apart from issues mentioned above and valid for comparative linguistics as a whole, there are special reasons for such an approach, specific for the Semitic area. One group of these reasons lies outside linguistics as such and is highly hypothetical. The Semitic language family includes languages of world religions and great ancient human civilizations — languages that preserve their special sociolinguistic status in modern times, irrespective of individual attitudes of particular researchers. This fact can create a certain psychological context, open or hidden (latent), which is not too favorable for the idea of reconstructing archetypes that underlie and antecede attested linguistic phenomena in these particular languages.

A more obvious and powerful reason pertains to the first Neogrammarian axiom mentioned above, i.e. arbitrariness of the linguistic sign. The phenomenon of stable correlations between phonetic forms and their semantic or pragmatic characteristics outside the group of evident onomatopoeic words and *Lallwörter* is attested in practically all languages of the world. In some languages this phenomenon is more widespread than in others, cf. the so-called "ideophones" in different African languages. The history of linguistics knows many attempts to use phonetic symbolism in particular languages and language families for glottogonic theories; however, it must be emphasized that Semitic languages as a whole, and the Arabic language first and foremost, have a certain privileged status in this linguistic trend.

A good example of it is A. Gazov-Ginzberg's work "Is language imitative by origin? (Evidence from common Semitic stock of roots)" (Gazov-Ginzberg 1965, in Russian with a brief English summary). The author claims to identify the following four groups of onomatopoeic lexemes on the basis of his typological analysis of imitative lexicon in many Semitic and non-Semitic languages with a special focus on Arabic and Hebrew (Gazov-Ginsberg 1965: 171–172):

A. "Internal imitation": 1. blowing, whiff, puff; 2. snuffing, breath; 3. sniffing (pshawing); 4. imbibing, sipping, sucking; 5. smacking (one's lips), champing; 6. licking, lapping, etc; 7. snapping (biting), chattering one's teeth; 8. spitting, sprinkling; 9. labial vibrant pshawing; 10. snoring, hoarseness; 11. choking; 12. laughter; 13. sighing, moaning; 14. crying, roaring; 15. whistle, hissing; 16. whispering, babbling, murmuring, etc; 17. keeping mum, hushing; 18. hopping; 19. trembling; 20. expiration for warming (one's hands, etc).
B. "External imitation": 1. animal voices (different animals and birds); 2. tramping, stamping, stepping; 3. grasping, grabbing, gripping; 4. slapping, clapping (one's hands); 5. knocking, tapping; 6. rumble (of thunder), rattle; 7. breaking, crack, crash, etc; 8. creak, scrunch, chirr, etc; 9. rustle, rash; 10. slipping, sliding, gliding; 11. slitting; 12. bursting; 13. bubbling; 14. splash (of water); 15. dripping; 16. fluttering (of a bird), hum, buzz (of an insect); 17. tinkling, ringing.

C. Gestures of oral organs: 1. opening one's mouth; 2. shutting one's mouth; 3. pouting (one's lips); 4. stretching (sinking) one's cheeks; 5. imitation of full mouth; 6. lolling out (one's tongue); 7. squeezing, clenching (one's teeth); 8. total constriction; 9. imitation of urination; 10. names of vocal (oral) organs.
D. Babbling (nursery) words.

It is evident that the author's answer to the question that constitutes the title of his book is positive, hence, not compatible with Neogrammarian paradigm. He claims that Semitic languages have preserved the most archaic state in the process of the formation of the human language. The special status of Semitic languages and the exclusive status of Arabic, sometimes together with Hebrew, is accounted for by the specific structure of the Semitic consonant root. The triconsonantal structure of the Semitic root, where one of the consonants is prone to various alternations, is a very convenient object for different glottogonic theories, since it presents various possibilities to correlate the phonetic value of these alternating consonants with semantic shifts in the respective consonantal root. Different theoretical models to analyze consonantal root alternations in Semitic languages were put forward in the works of certain Russian Semitologists of the first half of the 20th century. Thus, in order to explain these consonantal variations, N. Yushmanov postulated the existence in Proto-Semitic of "diffuse" phonemes (or "archiphonemes"). According to him, each of these diffuse phonemes may be a source of several phonemes in particular Semitic languages (Yushmanov 1998: 126–191). S. Mayzel' presented a detailed analysis of consonantal variants in Semitic triconsonantal roots and a semantic typology of these variations (Maisel' 1983; see more on this in Porkhomovsky 2007). G. Bohas proposed a different approach to these consonantal variations within the framework of his model "*matrices et étymons*" (Bohas 1997, 2000).

Alternations of root consonants are characteristic of Semitic languages in general, but Arabic is especially rich in this respect (with the second place obviously belonging to Hebrew). There is no doubt that data, collected by the authors of the above-mentioned works, and their typological analysis make an important contribution to Semitic linguistics. At the same time these alternations often violate regular sound correspondences; for this reason, it is difficult to make a choice between alternating consonants and to decide what particular variant should be considered a reflex of the prototype. For this reason the task of reconstructing Proto-Semitic archetypes is often viewed with reserve or is even considered utterly impossible. In my opinion, this is the main obstacle on the way towards the creation of a comprehensive Semitic etymological dictionary with reconstructions of common roots, cf. the following comment by I. Diakonoff:

> It is necessary to point out a very interesting phenomenon which is rather widely spread in Semitic languages (especially in Arabic) but not unknown in other language families. This phenomenon consists of semantic connection between phonetically (acoustically or articulatorily) close roots, which are not regular reflexes. Thus, cf. the following root series in Arabic: ksr, ksf, qsm…, qṭ', qṭṭ, qtl < *qṭl … All these roots have the meaning 'to cut off', 'to tear', 'to break off' etc. … Probably this is a case of onomatopoeia, not only direct (imitation of natural sounds) but also secondary (imitation of already existing roots)… It is also quite evident that phonic incompatibilities valid for one dialect, but not for another, also played their part, as well as interdialectal loans… Be it as it may, the phenomenon in question is yet one more means of word-formation, not studied before, and which is probably diachronically rather late. (Diakonoff 1988: 55–56, note 13)

Thus, the problem of root consonant alternations in Semitic may be summarized as follows. The analysis of this phenomenon is an important part of Semitic linguistics, but attempts to consider it as an argument for glottogonic hypotheses cannot be accepted, since it is not

possible to consider Arabic or even Proto-Semitic as direct reflexes of the original human language. Most likely, these late alternations result from the powerful mechanism of development by analogy, which was best pronounced or best preserved in Arabic. But at the same time these evident facts of Semitic languages and, above all, of Arabic language structures served as additional reasons for the negative attitude towards the reconstruction of Proto-Semitic archetypes in comparative Semitic studies.

Conclusion. The main goal of the present paper was to focus on certain aspects which make Semitic languages a special case within the framework of comparative linguistics. The particular choice of Arabic for most of the illustrations was natural, since it presents the archetypal, most evident case of these specific features, falling into three different groups, viz. (1) correlation between classical languages of great civilizations of the Ancient Near East and world religions and modern unwritten languages in the context of comparative studies and reconstructions, (2) a most unusual situation in the Semitic *tense-aspect-mood* verbal system that made straightforward reconstruction of the Proto-Semitic verbal system and its further evolution practically impossible, (3) consonantal alternations and variants within triconsonantal Semitic roots that became a serious obstacle to the reconstruction of Proto-Semitic lexicon.

At first glance, these three aspects have nothing in common, but one should take into account their combined cumulative effect on the formation and development of Semitic comparative studies. This resulted in the paradoxical state of the art that is characterized by a very high level of comparative studies, viz. establishing correspondences on all language levels, combined with an obvious reluctance towards proto-language reconstructions.

Concerning the first aspect, the most evident result is that only during the last decades non-written languages of Southern Arabia and Ethiopia were included into comparative studies on par with classical languages. This almost immediately brought about a real scientific revolution (in Kuhn's terminology) in comparative phonetics, morphology and internal classification of Semitic languages. The second aspect, viz. the Semitic verbal system in comparative context, may well provoke a teleological approach, since it makes another serious obstacle to the reconstructions of Proto-Semitic. The note by A. Meillet, cited above, emphasizes this very strange situation. The system of three verbal aspects is attested in Semitic languages and its semantic cells are filled with three morphological units which are obviously identical in their structures; hence, they must be derived from the same proto-forms. At the same time the correlations between semantic and formal sides of these categories are opposite in different Semitic language groups as far as the main binary aspect system *perfective vs. imperfective* is concerned, and this does not allow to arrive at a sound proto-level reconstruction. The solution of this problem, proposed by the present author, allows to overcome this obstacle and, as a result, to propose a consequent internal genealogical classification of Semitic. However, it should be noted that this reconstruction is based not on the Neogrammarian principle of the comparison of forms with the focus on phonetic and morphonological criteria, but on reconstructions within the framework of diachronic typology. This approach seems justified, since morphology is systematic and the evolution of the system may well be a more powerful factor than the phonetic evolution of forms. Arabic language presents the most straightforward case of a verbal system evolution from the postulated proto-Semitic stage towards classical Arabic and further on to modern spoken dialects.

The shift of consonants in the Semitic root, discussed in the last part of this article, presents a serious methodological problem, since it allows to challenge the principle of arbitrariness of the language sign, which is a basic axiom of the comparative paradigm. Again, it is in Arabic that we find the most abundant data in favor of such an approach. In any case, the exis-

tence of this phenomenon is not a hindrance towards establishing regular sound correspondences and reconstructing the phonological system of Proto-Semitic. The problem is in reconstructing particular proto-language consonantal roots, since one must either postulate a large amount of quasi-synonyms with minimal phonetic differences or to select one root in a particular semantic and phonetic lexical group as a prototypical one and to explain away other roots as consonantal variants. As a result, even though the overall level of Semitic lexicography is very high and the fundamental dictionaries of classical languages comprise abundant lexical cognates from other Semitic languages, the task of compiling a comprehensive Semitic etymological dictionary is far from actual realization.

In conclusion, it is possible to assert that Semitic historical linguistics is characterized by a very high level of comparative studies as such, yet the same cannot be said about proto-language reconstructions of grammar and vocabulary which should normally be an integral part of any comparative studies.

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В. Я. Порхомовский. Арабский язык в контексте сравнительных исследований.

В статье обсуждаются некоторые характерные особенности арабского языка, определившие его место в семитских сравнительных исследованиях. В основе этих особенностей лежат не только структуры арабского языка разных уровней, но и экстралингвистические факторы, прежде всего социокультурные и психологические. Подобное сочетание лингвистических и экстралингвистических факторов делают арабский язык уникальным феноменом среди языков мира. Прежде чем рассматривать место арабского языка в семитологии дается краткий обзор истории внутрисемитских сравнений до возникновения современного сравнительного языкознания и роли в этом плане средневековой арабской грамматической традиции. В фокусе следующего раздела статьи находится роль арабского языка как эталона для протосемитских реконструкций, а также обсуждаются коренные изменения, которые претерпела эта роль в истории сравнительных семитских исследований. В заключительном разделе рассматриваются некоторые характерные черты арабской и семитской фонетики и лексики и их соотношение со стандартной младограмматической парадигмой сравнительного языкознания. Эта проблематика заслуживает специального внимания, поскольку теории, опирающиеся на эти феномены, выходят за рамки стандартной парадигмы сравнительной лингвистики, а арабский язык может рассматриваться как архетипический пример подобных феноменов.

Ключевые слова: сравнительное языкознание, семитские языки, арабский язык, младограмматическая парадигма, регулярные соответствия, реконструкция, бинарная оппозиция, перфектив, имперфектив, историческая типология.

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Layers of the oldest Egyptian lexicon VIII: Numerals*

This paper belongs to a series of publications whose goal is to survey the most ancient part of Egyptian basic lexicon, classified by semantical domains, in order to stratify the different lexical layers (wherever they are present) in the light of Semitic vs. African Afro-Asiatic dichotomy, which was already suggested by P. Lacau several decades ago. The current paper focuses on the etymologies of Egyptian numerals.

Keywords: Ancient Egyptian, Afro-Asiatic languages, etymology, historical phonology, numerals.

In memoriam A. Zaborski (1942–2014)

Introduction

The first part of my series "Layers of the oldest Egyptian lexicon"[1] re-examined the controversies of P. Lacau's (1970) old observation on a binary opposition of certain items of Ancient Egyptian anatomical terminology in the context of many new results issuing from current progress in Afro-Asiatic (Semito-Hamitic) comparative linguistics. The etymological examination of Ancient Egyptian anatomical terminology presented therein has corroborated a surprising distribution: one member of the synonymous pairs is usually a Semitic word, whereas the other one(s) has/ve non-Semitic cognate(s) solely attested in some of the African branches of our language macrofamily. A relatively deeper presence of the extra-Semitic vocabulary in Egyptian has also become apparent. The subsequent papers in this series ("Layers of the oldest Egyptian lexicon II–VII") focused on the rest of the Ancient Egyptian anatomical terminology,[2] led by the wish to see to what degree this etymological dichotomy was characteristic there,

* It is here that I have to express my thanks to the Bolyai research fellowship (Hungarian Academy of Sciences, reg. no.: BO / 00360 / 12) facilitating my project on Egyptian linguogenesis, which resulted, *inter alia*, in a number of papers including this and the preceding parts of my series "Layers of the oldest Egyptian lexicon". I am pleased to express my gratitude to both Prof. W. G. E. Watson (Morpeth, UK) and Prof. G. Hudson (East Lansing, USA) for unselfishly devoting some of their precious time to correct the English of this text.

This paper is a farewell to my dear senior Semito-Hamiticist fellow, my unforgettable *Doktor-* and *Habilitationsvater* (ELTE, Hungary, June 1998 and October 2003, resp.), whose tragical premature passing away (autumn 2014) I cannot comprehend, to whom I owe so much. His famous studies on the Omotic and Cushitic numerals (1983 and 1987, resp.) are also considered here.

[1] Takács, G.: Layers of the Oldest Egyptian Lexicon I. *Rocznik Orientalistyczny* 68/1 (2015), 85–139.

[2] Part II deals with the Egyptian anatomical terminology for parts of the head and the neck, which is published in *Rocznik Orientalistyczny* 69/1 (2016), 59–124. Part III (with an etymological study on the upper torso) is planned to appear in *Rocznik Orientalistyczny* 69/2 (2016). Part IV (terms for the lower torso), V (parts of the foot), VI (back parts of the body and below), and VII (terms pertaining to the body in general, e.g., skin, flesh, blood etc.) are still being prepared, but not yet ready for publication, although the relevant raw lexical materials have already been accumulated and so certain preliminary impressions are already available.

with the outcome that the overwhelming majority of Egyptian body part names was merely South Afro-Asiatic.

Now, as in my previous communications, the Egyptian numerals, as part of the basic vocabulary, are examined from the same standpoint so that we can see these diverse (South vs. North Afro-Asiatic) layers of our numeralia. May this paper express my high esteem and affection for our great Master in comparative Afro-Asiatic (Semito-Hamitic) studies, whose department at the Jagellonian University of Kraków was the only one in the world devoted to Afro-Asiatic linguistics in the recent decades.

Eg. √wʕ "eins" (OK–, Wb I 273–276): in spite of the many unsuccesful attempts at its Afro-Asiatic etymology made over the past one and a half of a century,[3] only recently has W. Vycichl (DELC 518), followed by A. Ju. Militarev (in Starostin et al. 1995, 23), found its phonologically completely satisfactory cognates, which only appear in Semitic, where the latter scholar reconstructed the underlying root as *√wʕy "to sweep together", cf. OT Hbr. √yʕy qal (hapax, Is. 28:17) "wegraffen", hence yāʕīm (pl.) "Schaufeln" [GB 306–7] = √yʕy "to sweep away (hail)", hence *yāʕ or *yāʕe(h) "shovel to clean the altar" [KB 419] = √yʕy "to sweep toegther and carry away" [Klein 1987, 261a] | OSA √yʕy "to snatch away" [Müller quoted in KB], Ar. √wʕy I: waʕā "1. rassembler, ramasser, réunir sur un seul point, 5. s'amasser sur un seul point (se dit, p.ex., du pus dans la plaie), 6. être guéri (se dit d'un os fracturé dont les éclats se réunissent)" [BK II 1570] = "sammeln" [GB] = "to collect, hold" [KB] = "umfassen, enthalten" [Lsl.]. Besides, it is this root that, following F. Rundgren (1961, 121–127) and W. Leslau (1987, 23), also the Semitic term for "Eingeweide" (usually taken from *√mʕy), is derived from an assumed primary stem **miʕwʕay- "(etwa) Sammlungsort, Gefäß".

Eg. √sn (hence masc. dual sn.wj, fem. sn.tj) "zwei" (OK–, Wb IV 148) is identical with Sem. *t̲in- "2" [Djk.] = *√t̲ny [Vcl.] ||| Brb. *sin "2" [Mlt. 1991, 167],[4] i.e., this numeral root is only at-

[3] The most widespread etymology was its combination with Ar. √wḥd and its Semitic kindred, cf. Sethe 1916, 21, §1; Ember 1917, 87, #134; 1926, 305, #3.4; Albright 1918, 90; 1927, 200; Behnk 1927, 81, #7; ESS §5.c; Dolgopol'skij 1967, 300, #5; Schenkel 1997, 114. In addition to this Eg.-Sem. comparison, which was rightly declined already by V. Blažek (1999, 30, §4.1), several scholars, e.g., L. Reinisch (1874, xii, fn. 3), F. Behnk (1928, 139, #18), E. Zyhlarz (1931, 134–135; 1950, 407), Ju. Zavadovskij (1967, 43; 1974, 105; 1975, 45), and then E. Lipiński (1997, 284, §35.3.e) suggested further cognates in NBrb. yn̲ (m), yt̲ (f) and SBrb. iyen (m), iyet (f) "1" [Zhl.] derived by E. Zyhlarz from *√wgy (1931) and later even from an artificial *√wʕy (1950) or most surprisingly by E. Lipiński (l.c.) from a *waʕ(-n). V. Blažek (1987 MS, §1.2; 1990, 34; 1999, 30, §4.1), in turn, identified both Sem. *√wʕy and Eg. wʕ (in 1990, strangely, only Eg. wʕ) with the Berber numeral for "1", whose Proto-Berber etymon has been recently reconstructed as *yīw-ān/-at [Prs.] = *ya-N/T [Zvd.] = *iyyaw-an/at (m/f) [Mlt.]. L. Homburger (1928, 335 along with many other untenable non-AA parallels) and H. Abel (1933–34, 305) connected Eg. wʕ to Common Nubian *wēr "1". Similarly, W. Leslau (1962, 47, #27, cf. Conti 1978, 43, fn. 5) assumed a relationship with ES: Tigre woro "1". Both suggestions suffer from the fact that the correspondence of r to Eg. ʕ is irregular. M. L. Bender (1975, 179), in turn, affiliated the Eg. numeral with SCu.: WRift *wak- "1" [GT pace Zbr. 1987, 343], in which, however, there is no trace of the *ʕ. In addition, as Ch. Ehret (1980, 312) pointed out, the WRift term is "probably" juxtaposed from two demonstrative roots (*wa + *ka), which is certainly not the case of Eg. wʕ. V. Blažek (1990, 34; 1993 MS, 3, §1.9) too, beside the Berber parallels (above), could not resist comparing SCu.: Ma'a (Mbugu) wé "1" [Green, Wtl.] and WCh.: Karekare wàiké "each, all" [Krf.], where he singled out an "element" *wV "1".

[4] See Hommel 1883, 96, §11; Erman 1892, 118; Sethe 1916, 19, §2; Albright 1918, 91; 1926, 189; 1923, 68; 1927, 200; Ember 1926, 305, n. 7; Farina 1926, 15; Behnk 1928, 140, #44; ESS §11.a.50; Zyhlarz 1931, 135, §2; Vycichl 1955, 310; 1958, 378, 399; 1974, 62, §5; D'jakonov 1965, 46; 1974, 742; 1986, 61; Hodge 1968, 27, #113; 1981, 410; 1990, 646, §9.A; Zavadovskij 1967, 43; 1974, 106, §6.1; 1975, 45–46; Dolgopol'skij 1973, 111; Bender 1975, 194; Belova 1989, 14; Militarev & Stolbova 1990, 56; Militarev 1991, 75; Dombrowski & Dombrowski 1991, 343; Lipiński 1997, 284, §35.4; Blažek 1999, 30–31, §4.2.

tested in the northern branches of the Afro-Asiatic macrofamily of languages. Elsewhere, it is unattested with *-n. The Semitic root has, however, also a heteroclitic variety with *-r, which may be traced back even on the Proto-Afro-Asiatic level, cf. AA *čir- ~ *čar- "two" [GT] > Sem. **ṯir- > *ṯər- "two" [GT][5] ||| presumably SCu.: WRift *čar- (unless < *čad-) "two" [GT][6] ||| PCh. *√čr "two" [GT].[7] The Sem.-SCu.-Ch. etymology was first suggested by V. Blažek (1987 MS, 8–9, #2.2; 1990, 36). Which of these root varieties (AA *√čn vs. *√čr "2") is to be considered as the primary one is not to be answered here. It is, however, noteworthy that only Semitic has both of them.

Eg. √ḫmt (hence occuring as masc. pl. ḫmt.w, fem. ḫmt.t) "drei" (OK–, Wb III 283): the mystery of its origins has sometimes led to sometimes to absurd etymologies.[8] In his prestigious LÄ article on Egyptian numerals, A. Loprieno (1986, 1308), however, all too hastingly and carelessly remarked that "*eine überzeugende Etymologie liegt nicht vor*", which was by far not true even in his day. Surprisingly, he overlooked and did not even quote the most hopeful approach suggested at that time by a number of outstanding comparatist authors like A. Trombetti (1902, 196, §3), C. Meinhof (1912, 233), and M. L. Bender (1975, 192), who all combined the Egyptian numeral with NOm.: Kafa kämō "3" [Rn. 1888, 56] = kḛ́mō [Mnh.] = kēmō [Crl. 1951, 461] = keymo [Bnd. 1971, 259], a numeral apparently standing totally isolated within Omotic. Whether the similarly isolated WCh.: Karekare kumu (sic, -m-) "3" [IL apud JI 1994 II 326][9] is, in fact, also cognate, is hard to determine as elsewhere in the West Chadic daughter

[5] Attested in Biblical Aram. tərēn, fem. tartēn [GB 931], Mandean tartin ~ atrin [Drower], Neo-Aram. iṯr(i), fem. ṯare(i) [Bergsträsser], Neo-Syriac trī ~ tīrti ~ tirwē ~ tarwē [Kutscher] (NWSem.: KB 2009) || MSA: Soqotri tro (tiro) ~ (poetical) tróho (so, t-) [Lsl. 1938, 445] = trɔ, fem. trih [Jns.], Harsusi ṯerō, fem. ṯerét [Jns. 1977, 133], Jibbali ṯroh, fem. ṯrut [Jns. 1981, 285], Mehri ṯru (tru), fem. ṯrīt [Jahn] = ṯərō ~ ṯroh, fem. əṯráyt ~ ṯrɛlt [Jns. 1987, 418].

[6] Based on the equation of Iraqw tsar and Burunge čada (WRift: Ehret 1980, 229, #4).

[7] Attested in WCh.: NBauchi *čir ~ *čar [GT], cf. Jimbin šír [Skn.], Pa'a čírù [MSkn.] = čiřu [IL], Siri bi-čáre (ch-) [Gowers] = bə-čâr [Skn.] = bù-čáṛì [IL] (prefix bV- of numerals), Miya cîr (ts-) [Skn.], Mburku cər (ts-) [Skn.] (NBch.: Skinner 1977, 33) | Bade sērīn [IL], Ngizim šírín [Schuh] = šíṛin [IL] || CCh.: Musgoy sray [Mch.], Daba sraj [Pascal] = sìrāy [Lienhard], Kola sàrây [Schubert] || ECh.: Sumray sór [Jng.], Tumak hɛè [Caprile], probably < *sēr [GT] | WDangla sèèr, sèèrɔ́ [Fédry], Migama sê:rà [Jng.], Mokilko sìré [Jng.] | Mubi-Toram *sīr(i) [GT] > Mubi sìir [Lks. 1937, 185] = *sìir [Bnd.-Drn. 1983, 78, #90] = sìr [Jng. 1990 MS, 42], Birgit síirì [Jng. 2004, 358], Minjile *sir [Bnd.-Drn. 1983, 78, #90], Kajakse *siri [Bnd.-Drn. 1983, 78, #90], Masmaje sìrrì [Alio 2004, 284, #151], Toram see [Alio 2004, 262, #397], Jegu šee [Jng. 1961, 117], Kofa sèy [Jng. 1977 MS, 16, #402].

[8] W. M. Müller 1907, 303, fn. 1; Sethe 1916, 21, §3; Albright 1918, 91; 1927, 199; Farina 1926, 14; ESS §10.a.33: Eg. ḫmt < *ḫnt < *šnt < *šlt < *ṯlṯ ~ Sem. *ṯalāṯ- "3". K. Sethe (l.c.) remained neutral with hesitation: "*… aber m mit sem.* l, t *mit* ṯ *zu identifizieren, fehlt mir vorläufig doch der Mut*". M. M. Bravmann (1933, 148–149) assumed Eg. ḫmt < *ḫlt < *flt < *ṯlt allowing even that "*there is no problem with m < *l in Egyptian*" without further evidence. Even W. Westendorf (1962, 27, fn. 1) mentioned the alleged cognacy of Eg. ḫmt vs. Sem. *ṯalāṯ- among the instances of the interchange of Eg. m ~ n (sic). A. Ember (1917, 88, fn. 1), in turn, was "*inclined to believe*" in its cognacy with Sem. *√ḫmš "5". K. Sethe (1916, 23, fn. 2), following this idea, assumed that there "*war bei der Trennung der beiden Sprachzweige noch ein unbestimmter Vielheitsausdruck, den der erste Zweig dann für das eine, der andere für das andere absterbende Zahlwort einsetzte*, which A. Loprieno (1986, 1315–1316, n. 18) rightly doubted: "*vermag ich weder phonologisch noch semantisch zu verstehen*". L. Homburger's (1928, 336) African parallels (outside AA, such as, e.g., Bantu satu, Agni nsâ) are evidently out of the question equally for phonetic reasons. Ju.N. Zavadovskij (1967, 43; 1974, 107, §7; 1975, 47, §7.0) put forward his strange idea that Brb. √krḍ "3" "*соответствует до некоторой степени*" to Eg. ḫmt (1967 l.c.: "*параллелизм здесь выражен цепочкой 'гортанный + сонант + зубной'*"; 1974 l.c.: both roots are of parallel structure: post-palatal + sonant + dental), which V. Blažek (1999, 63, §3.1) has already correctly rejected as it "*does not respect any known phonetic law*".

[9] Note that J. Lukas (1966, 202) recorded Karekare kúúnù (sic, with -n-), which is, contrary to the record made by the IL with the unexpected anomalous -m-, in accordance with the rest of the comparative evidence usually gained from West Chadic.

language groups (Angas-Sura, Ron, Bole-Tangale), there seems to emerge a proto-form *ḳun- "3" [GT] = *kunu [Stl. 1987, 209, #595].[10] But where is the trace of a dental plosive C_3 in Kafa and Karekare? Nowhere, in fact. This lack of the third radical makes me doubt this Egypto-Chatic comparison and search further.

The West Chadic biradical root was handled, e.g., by H. Jungraithmayr and D. Ibriszimow (1994 I 168A) as a remnant of their triradical PCh. *√knḍ "3" via apocopy. Interestingly and astonishingly, this is precisely the very same sequence of those root consonants that Eg. √ḫmt also represents, i.e., velar + nasal + dental! All three radicals of this Proto-Chadic triradical root have been preserved until now, with the necessary *Lautverschiebungen*, of course, by the following daughter languages: WCh.: Jimbin kəndí [Skn.], Diri hyíinzù [IL] = hìnzù [Skn.] < *kind- [GT] || CCh.: PMasa *ḥindi, regular < *Kindi "three" [GT]: Banana yìntì(di) [Krf.] = yìnti [Zima], Musey hindi [Krf.], Gizey/Wina, Ham, Musey, Lew, Marba ḥìndì [Ajl. et al. 2001, 56], Lame hinzi [Lks. 1937, 139] = híncʲiʔi [Krf.] = hínʒìʔì [Scn. 1982, 516], Zime-Batna híḏì [Jng.] = híndzìʔì [Scn.], Peve hínʒ̄ī [Krf.], Zime-Dari hinyi < *hinǯi < *hindi [Str.] = hinyi [Lks. 1937, 139] = hinǯiʔ [Venberg 1975, 41], Zime-Misme hindi [Krf.] (Masa group: Zima 1990, 268; Ch. data: JI 1994 II 326–7). In the light of these data, the reconstruction of PCh. *√knḍ "3" [JI] might be modified on two points. First, the correspondence of k- in the majority of the Chadic daughter languages to h- in the Masa group speaks for a PCh. fricative *ḫ- (cf. Stolbova 1996, 68, §I.6, table 6) and not a plosive *k-. Secondly, the glottalized *-ḍ is not really supported by any of the reflexes listed above, where we mostly find either plain -d or its palatalized sequence (-ǯ > -y), which is not at all a typical phenomenon with a glottalized dental plosive and evidently indicates *-d. All in all, if the cognacy between PCh. *√ḫnd ||| Eg. √ḫmt "3" is true, it is to be explained by the circumstance that the cluster -C_2C_3- of PCh. *ḫind- resulted from a voicing process (influenced by *-n-)[11] and an assimilation ultimately from **ḫimt- [GT]. To the best of my knowledge, so far nobody (not even V. Blažek in his exhaustive 1999 book on the numerals in Afro-Asiatic and Indo-European) has suggested this Ch.-Eg. comparison.

In a number of Chadic reflexes of this (?) root, the medial nasal radical is not reflected, only a velar C_1 (*k-) and a dental C_2 (possibly *-d), i.e. *√kd or sim.[12] These Chadic forms may be akin to ECu.: Yaaku ḫāt "3" [Heine quoted by Zbr. 1987, 342], regularly derivable from a hypothetical ECu. **kād- [GT], which is completely isolated within the whole Cushitic family. Does the underlying etymon, in fact, represent the ultimate biconsonantal root? In addition, H. G. Mukarovsky (1987, 35) combined these reflexes with NOm. *√kd/z "3" [Mkr.] = *√hʒ [Zima] = *Kaʒu > *Kawʒ- > *Kayǯ- [Blz. 1990, 39] < *ḫayd- [GT],[13] which only confirms the sup-

[10] O. V. Stolbova (l.c.) was unaware of the Tal and Goemay data, which betray a glottalized *ḳ- instead of plain *k-.

[11] The same voicing effect of the nasal has been observed in the cluster -nC- throughout the whole Egyptian *Sprachgeschichte*, cf. the shift of Cpt. (S) nc > nz attested in Eg. ʕ.t-n.(t)-sbʔ "school" > (SF) ancybe, (SL) ancyb, (S) anZybe, (B) anZyb, etc. (KHW 8); cf. already the OEg. alphabetic writing nzw for nsw "king", which was certainly vocalized as *j/ʔinsiw with a cluster *-ns- as cuneiform evidence from the 13th century BC also indicates (Wb II 325–9; Sethe 1911, 16–30; 1912, 98; Farina 1926, 16; ÜKAPT IV 54, ad PT 814c; AÄG 51–52, §116).

[12] Cf. WCh.: (?) Bokkos ʔátát [Jng.] < *ḫad- (?) [GT] | Warji kə́ǯì [Jng. and Skn.] = kʰə́ʒì (-dz-) [IL], Tsagu kə́də́ [Skn.], Kariya and Miya kə́dì [Skn.], Pa'a kə́dù [Jng. & MSkn.] = kədu [IL], Siri bu-kudde [Gowers] = bù-kúdì [IL] = bu-kúdi [Skn.], Mburku kídí [Skn.] || CCh.: Mandara kə́ǯə̰ [Mch.] = kiʃǯe [Meek] = kíʃǯé [Eguchi] < *ki[r]de (?) [GT] | Masa hidi [Mch. 1950, 59, so also Krf.] = hìdiʔ [Jng.] = [ḥìdī][12] [Ctc. 1983, 88] = ḥìdí "trois" [Ajl.], Masa-Bongor hìdīʔ "trois" [Jng. 1973 MS] || CCh.: Mandara kəḍye [Wolff 1974, 16] || ECh.: (???) Mokilko ʔáḍó (perhaps < *ḫaḍ-, cf. káḍùwé "zum dritten Mal") [Lks.] (Ch. data: JI 1994 II 326–7).

[13] Cf. NWOmeto *hayʒ- [GT]: Welamo hezzā [Moreno] = hĕza [Bnd. 1971, 252] = esa, eza, heza [Chiomio 1938, 4; Da Trento 1941, 206], Gofa heʒa (-dz-) [Moreno], Zala hezzā [Moreno], Malo héza [Moreno], Kullo hezu [Allan

position of an ancient biliteral root. If this latter scenario is true, we would have to assume a PAA *√ḫd [GT], which, however, contradicts the development of PCh. *ḫind- < **ḫint- < **ḫimt- [GT] outlined above and possibly also the equation with Eg. √ḫmt.

It is very probable that CCh.: PDaba *makad "3" [GT][14] represents merely the same biconsonantal root (*√kd or sim.) extended by an m- prefix instead of being the reflex of a hypothetical AA *√mḫT, i.e., the metathetic cognate of Eg. √ḫmt "3", however tempting this may seem *prima vista*.

Eg. √ḫmt "3" was identified by C. Meinhof (1907, 123; 1912, 233),[15] E. Zyhlarz (1931, 135–136, §3), W. Vycichl (1959, 33), H. G. Mukarovsky (1987, 45), and V. Blažek (1987 MS, 14–15, §3.1; 1990, 38; 1993 MS, 5, §3.1; 1999, 32, §3)[16] with the Bed. numeral "3", which was apparently constructed on the root √mhy.[17] Although J. D. Wölfel (1954, 5; 1965, 617) voiced only his reservations against this Eg.-Bed. comparison and in A. Zaborski's (1987, 319) view too, "*this is phonologically rather improbable*", one is tempted to ponder whether Zyhlarz (l.c. supra) was correct, having ingeniously envisaged a PBed. *măhádi (or sim.) on the basis of the supposed shift of Bed. y from an earlier palatalized dental, which is in fact valid for Bed. y = ECu. *z, cf. Bed. hayúk "Stern" [Rn. 1895, 133] || LECu.: Somali haḍig [Rn.] = ḥiddig [Sasse] = hadig [Zhl.] < ECu. *ḥizk-/*ḥuzk- "star" [Sasse 1979, 35 etc.]. Following this scenario, one might be inclined to surmise in both PBed. *mahadi "3" [Zhl.] and CCh.: PDaba *makad "3" [GT] (above) the same m- prefix extension of the same root. On the other hand, it is equally inspiring to observe — together with H. G. Mukarovsky (1987, 45) — the closeness of Bed. √mhy to WCh.: SBauchi *√mKy (perhaps *m$^{(w)}$ā[ḫ]ay?) "3" [GT],[18] since the latter can by no means be explained from *ma-ḫad (or sim.) the same way as in Bedawye, and — even more interestingly — the common

1976, 330] = hēza [Bnd. 1971, 252], Dache heʒa (-dz-) [Bnd. 1971, 253], Dorze hēʒa (-dz-) [Bnd. 1971, 253] = heiza [Flm.], Male hāyco (-yts-) [Da Trento 1941, 206; Bnd. 1971, 255], Oyda hāyʒi (-dz-), oyddi [Bnd. 1971, 254] (NWOmeto: Moreno 1938, 37) | SEOmeto *hayʒ- [GT]: Zayse hayc (-ts) [Crl. 1938 III, 194], Zergulla hayc (-ts) [Bnd. 1971, 257], Gidicho hāyʒⁱ (-dz-) [Bnd. 1971, 256], Koyra hayʒe (-dz-) [Hayward, also Bnd. 1971, 252], Mezo hayʒi (-dz-) [Chiomio 1938, 235], Basketo hayzzā [Crl. 1938, 108] = hay/d/zi [Bnd. 1971, 254], Doko oyzē [CR 1927, 248] = hāyzā [d'Abbadie apud CR l.c.], Dollo ayz [CR 1927, 250] | Dizoid *kad(d)u [GT]: Dizi kadu [Toselli 1938, 13] = kàdú [Allan 1976, 381] = kaddu [Crl. 1951, 309], Sheko kaddu [CR 1925] = kādu [Bnd. 1971, 262] = kādem [Crl. 1951, 309], Nao kaddu [CR 1925] = kādu, kaddŏ [Bnd. 1971, 262] | Janjero kēz [Crl. 1938 III, 57] | Chara kezā [Crl. 1938 III, 151] | Gimirra kazu [Toselli 1939, 35], She kaz [CR 1925], Bencho kəz [Bnd. 1971, 260] | Kefoid (or Gonga) *keʒʒ- [GT]: Kafa kaǯā (-ğ-) [Rn. 1888], Mocha käʒʒo (-ğğ-) [Lsl. 1959] = keǯo (-ğ-) [Bnd. 1971, 260], Shinasha (Bworo) keza [Schuver in Grottanelli 1940, 103] = ke'ǯa (-'ğ-) [Grottanelli 1941, 266] = kēze [Brauner 1950, 70] = kēzza [Bnd. 1971, 259], Anfillo keʒʒo (-ğğ-) [Grottanelli 1940, 103] = kē'ǯo (-'dj-) [Bnd. 1971, 258] (NOm. Data: Zbr. 1983, 384–387). Note that V. Blažek (1990, 39) erroneously explained the NOm. stem from his AA *ʒaKu "3" via metathesis based on his comparison with Agaw *seq^{w}/γ^{w}a "3", PIraqw *dakati "8", WCh.: Hausa takwas "8", CCh. *tVkwazV "8".

[14] Attested as Musgoy makat [Mch. 1950, 59] = maakaa (sic) [Str.], Daba makat [Mch. 1966, 133] = maakaa (sic) [Str.] = màkāḍ [Lienhard], Hina maakáá (sic) [Str.], Kola màkâd [Schubert] (CCh.: Str. 1910, 456).

[15] In his paper from 1912 he meant this comparison beside the Kafa root √km for "3".

[16] Zyhlarz equated at the same time the Eg. numeral also with the Guanche term for "3".

[17] Recorded as (Bisharin) mehéy ~ máhi ~ maháy [Almkvist 1885, 46] = (Hadendoa, Halenga, Bisharin) emhá/ə́y ~ meháy ~ maháy ~ seldom mẹhá/ə́y [Rn. 1894, 10; 1895, 18, 167] = məhɛ́y [Roper 1928] = mhay [Bnd.] = (Arteiga) mhày ~ miháy [Hudson] = (Hala/enga) maháy [Rn.] = (Ammar'ar) mhäyy-t (f) vs. mhäyy-b (m) [Dlg.] (Bed. data: Dlg. 1973, 319; Zbr. 1987, 328; 1989, 589, #85).

[18] Attested in Boghom mói ~ mòi [Jng.] = mwày [Smz.], Zangwal maya [Smz.], Wangday mà·kí [IL] = mà:kai [Smz.], Zaranda maaki [Smz.], Dokshi mààγi [Smz.], Dikshi and Bandas mààgi [Smz.], Boodli (Zumbul) mààγa [Smz.], Zodi (Dwa/ot) mààgai, Zakshi mààgi [Smz.], Boot, Zaari, Sigidi mààki [Smz.], Zaar máì [IL] = mà:yi [Smz.], Zaar of Kal mààyi [Smz.], Zaar of Gambar Leere màài [Smz.], Zaar of Lusa maayì [Smz.], Tala mee [Smz.], Sho (Ju) miyaa [Smz.] (SBauchi data: Shimizu 1978, 39, #76).

biradical root *√ḫy that might in principle be singled out by assuming an m- prefix here, also finds a surprising match in the southernmost extremity of Cushitic, namely SCu.: Ma'a kaí ~ ḫaí "3" [Ehret], which is similarly attested with a prefix mi- (this, in turn, being from Bantu), cf. Ma'a mi-ḫai "3" [Mnh. 1906, 314]. As for the Southern Cushitic background of the Ma'a numeral, Ch. Ehret (1980, 249, #C2) suggests a comparison with Dahalo ḳáßa "3". The loss of final consonants is indeed an attested feature of Ma'a *Lautgeschichte*. The problem is, however, that in the Ma'a *Auslaut* we have a -y (and not zero as expected) that can hardly be regarded as a trace of a former *-b.[19] In any case, Blažek's (1990, 38) AA *ḫami (?) "3" based on the comparison between Eg. and Bed. "3" (including also the Guanche numeral "3", cf. below) is not well-founded even if Bed. √mhy and SBauchi *√mKy were related to Eg. ḫmt via metathesis. But this − as correctly stated by A. Zaborski (l.c. supra) − is at the moment quite improbable.

Another difficult question is how to evaluate CCh.: Mandara *√ḫkrd "3" [GT][20], where, in principle, we may account for the regular shift of -r- < PCh. *-n- and for a prefix *ḫ- (of numerals???), which eventually leads to assuming **ḫV-kind-. The cognacy of the Mandara numeral seems thus phonologically fully possible, although it might just as well be combined with Brb. *kraḍ "3" [Zvd. 1974, 107, §7; 1975, 47, §7.0] as suggested in JI 1994 I 168A, which, however, would lead to a completely distinct AA root. Furthermore, the dental radical is apparently additional, cf. CCh. *ma-/ga-ḫ-kər < *-kən [GT].[21]

Another surprising coincidence is represented by the isogloss of SOm. *makan "3" [Blz. 1990, 38] = *mākan > *makkan (hence *-m by assimilation) [GT][22] ||| WCh.: Dira miyaḫkᵊn "3" [Krf.] | SBauchi *makʷan "3" [Blz. 1990, 38] = *mʸaḫ(k)an [GT][23] || CCh. *ma-kanu "3" [Blz. 1990, 38] = *mʷāḫkan (?) [GT].[24] As far as I know, H. G. Mukarovsky (1987, 36) was probably the first to point to the connection of the Ch. m-(ḫ)-k-n/r/d forms, Bed. √mhy, and SOm. *√mkn. V. Blažek (1990, 38) unified all the extended varieties of PCh. *√kn "3" (prefix *m-, postfix *-d) with SOm. *makan under Common AA *(ma)-kanu-(di) "3". Similarly, M. Lamberti (1993, 70) equated the South Omotic stem with the Chadic m-k-n forms under a South Afro-Asiatic *mVkkVn- "3", which can only be true if we accept a prefix m- in both branches, which is certainly the case with PCh. *√kn "3", but we do not yet know anything about SOm. *mākan in this respect, whereas Bed. √mhy can hardly be related as the ultimate root cannot be isolated as **√mḫ.

[19] Cf., e.g., the zero reflex in Ma'a we "1" vs. WRift *wak "1", although the case of Ma'a hai "4" vs. ERift *hak- "4" speaks against (Zaborski 1987, 343, §1 and §2).

[20] Attested in Glavda ḫkàrda [Rapp] = ḫkərda [Wolff], Guduf ḫəkrrḍà [Smz.] = ḫ'ák^{h}əṛət [IL] = ḫkarde [Wolff] (Mandara group: Wolff 1974, 16).

[21] Cf. Lamang ḫkáná [Wolff] | Dghwede ḫkré [Frick] = ḫkare [Wolff] = xákṛè [IL], Ngweshe kḫwáṛò [IL], met. < *ḫkwar- [GT], Ghvoko ḫkwaro [Wolff] | Kotoko gaḫkər [Mch.] = gáḫkór [Lukas] = ʔàkàrà [Bouny] (CCh.: Wolff 1974, 16; Ch.: JI 1994 II 326–7).

[22] Ari maakkan, makkán, mʌkkʌn [Bnd.] = mākɛn [Bliese 1982], Banna məkəm [Bnd. 1971, 264] = mᵊʔkəm [Bnd.], Hamer makan [Crl. 1942, 262] = məkkan [Flm.] = m'aḳan [Lydall], Dime mɛkɛm [Bnd. 1971, 263] = mıkkım [Flm.], Karo makàmm [CR 1927, 252], Bako makken [Da Trento 1941, 206] (SOm.: Bnd. 1971, 263–264; 1994, 160, #86; Zbr. 1983, 388).

[23] Attested in Geji mekan [Gowers] = mékáŋ [IL] = meekaṇ/ŋ [Smz.] = mekᵊn [Krf.], Guruntum mian [Gowers] = myaŋ [Smz.], Kir ŋwe:n [Smz.], Buli min [Gowers] = mìyɛ̀n [IL] = mye:n [Smz.], Tule màŋkí [Smz.], Jimi mwaikan [Gowers], Pelu ḍè-mèèkaŋ [Smz.], Zul myaḫkan [Smz.], Barang myakan [Smz.] (SBauchi data: Smz. 1978, 39, #76).

[24] Cf. Ga'anda mahkᵊn [Krf.], Hwona maḫᵊn [Krf.] | Bura and Margi makər [Wolff], Margi-Gwara makəno [Wolff], Chibak makr̲ [IL] = makər [Wolff] | Bata mooaakĕn [Str.] = mwakən [Mch. 1950, 59], Bachama mùwa:kún [Skn.], Nzangi mwọọkən [Mch. 1950, 59] = menfén (sic) [Str.], Gudu makʌn [IL] | Sukur má:k^{h}àn [IL] | Paduko məkra [Mch. 1950, 59; Wolff] | Matakam màkâr [Schubert], Mofu máákàr ~ mahkàr [Brt.], Gisiga-Dogba maakar [Lks.], Muturwa makir [Str. 1910, 456] (CCh. data: Wolff 1974, 16).

Three scholars, E. Zyhlarz (1931, 135–136, §3), followed by O. Rössler (1966, 228; 1971, 284, 299) and V. Blažek (1987 MS, 14–15, §3.1; 1990, 38; 1993 MS, 5, §3.1; 1999, 32, §3) supposed a cognacy of Eg. √ḫmt with the Guanche word for "3" recorded as (Gran Canaria?) amelotti (cf. amierat-marava "13") [Niccoloso da Recco], (Tenerife?) amiat [Pseudo-Sosa, Marín y Cubas, Berthelot] = amiet [Cedeño de Chil] (Guanche: Wölfel 1954, 4 and 14–18; 1965, 616 and 626–630), in which they (except for Rössler) included also Bed. √mhy. What the ultimate root of the Guanche forms (known to us only through imperfect late medieval records and fully isolated in the whole Berber language family using a totally different root for "three") is, has been answered in different ways. E. Zyhlarz (l.c.) assumed √ʔmrt ~ √ʔmlt (with -t as part of the root), which he regarded as a correspondence of Eg. *√ḫmʒt (???), but he failed to present any proofs for the hypothetical –3- in the latter root, let alone the enigma as to how the Guanche *Anlaut* -∅ = Eg. -ḫ and where the reflex of the Guanche -r/l- is in the Bedawye root. Later, however, Zyhlarz (1950, 407) offered a completely different analysis of the Guanche word: *amelⁱ-hǫḍⁿ "der andere Zeiger" = "Mittelfinger". J. D. Wölfel (1954, 4; 1965, 616), in turn, singled out the stems *amel(o)-, *amier- in the Gran Canaria records, but how these could be compatible with Tenerife (?) amia/et, he failed to answer definitively: "*Deux explications possibles: ou bien le* -t *appartient au radical, ou bien le* -t *de* amiat *est là à la place de* -r- *de* amierat." Wölfel (1954, 6; 1965, 618) was convinced "*que le mot canarien pour « trois » n'a rien à faire ni avec l'égyptien, ni avec le mot bedja. … ce mot reste inexpliqué et complètement isolé.*" O. Rössler (l.c.) defined the root of the Guanche numeral as √ʔmt and derived it from an earlier AA *√ʕmt, which theoretically might indeed be a possible source for Eg. √ḫmt may have originated from (due the incompatibility rule of AA *ʕt > Eg. ḫt, cf. EDE I 326–7). But he failed to answer why the Gran Canaria records have -r- and -l-. V. Blažek (1999 l.c.) has equally failed to explain both the anomaly of the Anlaut in Eg. vs. Guanche[25] and the traceless -l-/-r- in Egyptian. So his (Blažek 1990, 38) hypothetic AA *ḫami (?) "3", which is supposed to underlie the Egyptian, Guanche, and Bedawye parallels, remains ill-founded.

Eg. √fd (masc. pl. fd.w, fem. fd.t) "vier" (OK, Wb I 582): no Semitic cognates at all, although there were attempts at forcing it together with the numeral "4" in Semitic[26] and Berber.[27] Instead, its cognates are to be found in Cushito-Omotic and they are especially wide-

[25] He solely relied upon an outline of Guanche vs. Berber consonantal correspondences (where Berber *γ/ḫ- > Guanche ḫ-, h-, g-, but also ∅- is admitted) by A. Ju. Militarev (1991, 167–168, more precisely §7 on p. 168), who, however, did not present any etymological evidence either for the case of Guanche ∅-.

[26] Several linguists (A. Trombetti 1902, 197, #4; K. Sethe 1916, 21–22; W. F. Albright 1918, 91 [with reservation]; A. Ember 1926, 302, fn. 10; ESS §4.a.13; recently A. B. Dolgopol'skij 1973, 231–232; 1983, 125; O. Rössler, followed by W. Schenkel 1990, 56; F. Kammerzell 1994, 170, 180 etc.) tried to demonstrate a relationship of Eg. fd (and/or LECu. *afar-) to Sem. *ʔarbaʕ- "4". The phonological anomalies were explained various unlikely ways through unjustified steps in the suggested hypothetic chain of phonological changes, e.g. Eg. jfd < *rfd < *rbḏ < *rbʕ or Eg. jfd < *jfr < *jrfʕ < *ʔrbʕ! The Eg.-Sem. equation was rejected already by numerous authors: W. F. Albright (1927, 201), E. Zyhlarz (1931, 136, #4), W. Vycichl (1934, 70, fn. 1; 1959, 33), W. A. Ward (1985, 239), V. Blažek (1999, 235–241; 1999, 32–38), H. C. Fleming (2000 MS, 6–7). As pointed out already by Zyhlarz (1931 l.c.), the expected correspondence of Sem. *ʔarbaʕ- would be Eg. *ʒfḫ (or *rfḫ) on the analogy of Eg. sfḫ = Sem. *šabʕ- "7". Besides, Stolbova (1987, 68) linked Sem. *ʔarbaʕ- to WCh. *rabu "2", while Blažek (1997, 8; 1999, 235–241; 1999, 31–38) compared it to LECu.: Orm. (Wellega) barʔū "palm of hand" [Gragg 1982] and possibly NOm. *biraḍ- (sic) "finger" [Blz.].

[27] No evident cognates in Berber. The common Brb. root for "four" can by no means be related to Bed.-Eg.-Ch. *√fṭ "four" as proposed by Ju. N. Zavadovskij (1967, 43; 1974, 110; 1975, 50), H. Jungraithmayr (1982, 8; JI 1994 I, 73), cf. e.g. NBrb.: Shilh: Sus qqǫẓ [Dst. 1938, 237] | Nefusa okkoz [Lst. 1931, 285] || EBrb.: Ghadames aqqiz [Lst.] || SBrb.: Ahaggar okkoẓ [Lst.], Ghat okkoz [Nhl. 1909, 195]. Cp. WCh. *kuĉA "nine" [Stl. 1987, 208, #590]. Comparing

spread in Chadic, cf. Bed. *faḍig “four” [GT],[28] supposed to derive from an older **fardig(a) [Blz. 1999, 33][29] ||| NOm. *Peč- [from an older **feṭ-?] “four” [GT][30] ||| Ch. *f^{w}aḍV [GT].[31] The common AA root here can only be *√fṭ.

In Lowland East Cushitic and in two Chadic groups, the presumably same common root appears to be *√fr, cf. LECu. *afr- [Black] = *afar-/*afur- [GT][32] ||| WCh.: Angas-Sura *f$\bar{e}_1$r [Stl. 1977, 154] = *fīr [Stl. 1987, 160] = *f’$\acute{e}_2$r [GT][33] || ECh.: PLay (PNancere) *p[o]ri [GT].[34]

Berber “4” to Eg. fd was rightly rejected already by M. G. Mercier (1933, 309) and recently by V. Blažek (ll.c.). V. Brugnatelli (1982, 76), followed by V. Blažek (1997, 9; 1999, 235–241, #4; 1999, 32–38, #4) compared SBrb.: Ahaggar ê-feḍ, pl. ê-fḍ-en “quantité innombrable (nombre qui dépasse tout ce qu’on peut compter)” [Fcd. 1951–2, 305, cf. Prs. 1974, 407], ETawllemmet ə-fəḍ “se multiplier”, e-fəḍ, pl. e-fəḍ-ăn “1. million, 2. nombre immense” [PAM 1998, 59]. For the semantic shift Blažek quoted Khoe thíyà “four” vs. thíyà “many”. Blažek (ll.c.) suggested alternatively NBrb.: Iznasen, Ait Ammart, Iboqqoyen, Ait Tuzin ṯa-fḍėn-t “orteil” [Rns. 1932, 298] | Qabyle ti-fden-t “orteil” [Dlt. 1982, 191] = (dial.) ṯi-fədn-in “orteils, doigts de pied” [Zvd.] || EBrb.: Ghadames ta-fadən-t “toe” [Lanfry], which is semantically dubious.

[28] Attested as Bed. faḍḍeg [Kremer] = fardik [Krockow] = ferdik [Lucas] = faḍíg [Rn. 1894, 10; 1895, 76] = fáḍig [Rn. 1890, 7; Roper 1928, 179] = faḍìg [Hds.], Bed. of Beni Amer farig [Rn.] (Bed. records: Dlg. 1966, 60; Blz. 1993 MS, 6–7, #4.1; 1999, 235ff.; 1999, 32ff.).

[29] There are controversial theories on the etymological analysis of Bed. “4”. A. Trombetti (1902, 197) explained it from PCu. *afar-dig. E. Zyhlarz (1932–1933, 167): Bed. *faḍí-g extended by “ein Numeral zusammenfassendes Suffix *-ga”, cf. Bed. -ga “a dual and plural ending” [Roper 1928, 183]. I. M. D’jakonov (1965, 47), did not exclude even an archetype *šaḍig (sic). Acc. to W. Vycichl (1960, 255, 262; 1978, 75), Eg. fd and Bed. “4” are not at all cognates (Vycichl explained Bed. -ḍ- from an ancient *ǯ or *g!). V. Blažek (1993 MS, 6–7, #4.1; 1997, 5; 1999, 235–241, #; 1999, 32–38, #4) supposed PBed. *fa[rd]ig, derived from a compound *fari-da-g(a), where Bed. -g would be identical with Bed. -ga “a dual and plural ending” [Roper 1928, 183] and the prefix *g- of numerals (presumed already by V. Ja. Porhomovskij in PKotoko *gVḍV “four” < *g-fVḍV?). Ch. Ehret (1995, #93), in turn, derived Bed. -ḍ- from PAA *-dl- [i.e. *-ǯ-]!

[30] Attested in Janjero hēč̣-a [h- < *p^{h}-] “quarter (fraction)” [Flm.] | Mocha p̲èč̣-o [č̣ < *ṭ possible] “quarter” [Lsl. 1959, 44] = βèč̣-o “quarter, fourth” [Flm.] | Mao (sic) beç-e ~ meç-e [-ts’-] “four” [Flm.], Hozo bɛc-ī́ [-ts-] “four”, Sezo bɛṣ-ɛ́ ~ bèṣ-ɛ́ “four” (Mao: Sbr.-Wdk. 1994, 13; NOm.: Flm. 2000 MS, 6–7).

[31] The underlying root for “4” has been exceptionally well preserved nearly in all Chadic languages. This apparent uniformity cannot be found in the case of other Chadic numerals. For a very detailed presentation and analysis of the reflexes in the Chadic daughter languages see EDE II 600–602. D. Ibriszomow (1988, 68–69) supposed an old quadrinary counting system in Chadic. The PCh. etymon has been set up in various forms: *p^{h}wVḍV [IS 1966, 21] = *f-ḍ- [NM 1966, 235, #38] = *f^{w}aḍə [Nwm. 1977, 26] = *fwVḍV/*VfwVḍV [Dlg. 1983, 125] = *-p-ḍ [JS 1981, 113; JI 1994 I, 73] = *(m)-p-ḍ-(w/y) [JS 1981, 113A] = *fid-oḍ- (sic) [Stl. 1996, 29]. O. V. Stolbova (1987, 160, §136) has WCh. *firadu based on Bole pórdo [Koelle] = p’ordo (sic) [Stl.], attested elsewhere as poḍḍo [Nwm., Lks.] = podo [Grb.] = poḍḍau ~ poḍḍo [Schuh 1982] = foḍo [IS, NM, Haruna] = fòḍḍó [Schuh 1984] = fɔḍḍɔ [IL]. The PCh. etymon suggested by P. Newman (1977 l.c.) and A. Dolgopolsky (1983, l.c.) seems most convincing.

[32] For the LECu. data see Rn. 1886, 845; PB 1963, 469; Black 1974, 104; Heine 1976, 215; Dlg. 1973, 231; Zbr. 1987, 328–340. The etymological connection of LECu. *ʔafar- “4” to the Chado-Egyptian isogloss is debatable. E. Cerulli (1938 III, 153) traced back LECu. *afr to “common Cushitic” (i.e., Cu.-Om.) *aft. A. B. Dolgopolsky (1973, 231; 1983, 125; 1988, 629, #6), in turn, with special regard to LECu. met. var. *ʔarf- (above), connected LECu. *ʔafar- to Sem. *ʔarbaʕ- “4”, which he explained as a met. of an earlier *√brʕ. Dolgopolsky’s theory was queried by F. A. Dombrowski & B. W. W. Dombrowski (1991, 341). At the same time, Dolgopolsky (1983, 125) compared Sem.-LECu. “4” also to Bed.-Eg.-Ch. “4”, although the LECu.-Sem. comparison excludes an equation of LECu. “4” with the Eg.-Ch. root. For the time being, most probable seems a common origin with LECu. *afar- from PAA *√fr.

[33] For the Angas-Sura data see Grb. 1958, 300, #1; Jng. 1965, 166, 168, 180–181; Stl. 1972, 182; Hfm. 1975 MS, 18, #35; GT 2004, 105. Contrary to O. V. Stolbova (1996, 29), who maintained AS *-r < Ch. *CVdVC (while PCh. *CVḍ- → AS *CVt), I see no justification for explaining AS *-r = PLay *-r from common Ch. *-ḍ.

[34] Cf. Nancere peri [Hfm.], Lele poring [Hfm.] = pōrīng [WP 1982, 77], Dormo porin [Hfm.], Gabri porin [AF] = pari [Dcr.], Chire porbu [Hfm.], Kabalay pori [Hfm.] (Lay gr.: Hfm. 1972, 204).

These data, according to our present knowledge, can by no means be explained from AA *√fṭ.[35]

Eg. √dj (masc. dj.w, fem. dj.t) "fünf" (OK–, WB V 420) is in fact not a word root at all as it has for a long time been unequivocally regarded as a nisbe of the extinct Eg. word *d or *jd "hand" (Osing: *dǐ́y.aw *"die zu einer Hand Gehörigen"), akin to Sem. *yad- "hand".[36] A similar semantic shift is attested in SCu.: Dahalo dáwàtte "5", act. *daßa-watte, lit. *"one hand", cf. WRift-Dahalo *daba "hand" (SCu.: Ehret 1980, 162, §ii.a.3). But for phonological and etymological reasons, H. G. Mukarovsky (1987, 45) and V. Blažek (1990, 30; 1991, 210) are presumably wrong in assuming a direct cognacy between the Dahalo and Ancient Egyptian numerals for "5".

Eg. *√srs[37] > √sjs (occuring as masc. pl. sjs.w, fem. sjs.t) "sechs" (OK–, Wb IV 40) is, according to *communis opinio*,[38] in the light of a few other instances of rhotacism of *d > Eg. r[39] (attested

[35] The underlying PAA form has been heavily debated. Ju. N. Zavadovskij (1974, 110; 1975, 50): PAA *√fd (incorrect, since AA plain *-d > Bed. -d ~ -t = Ch. *-d). I. M. D'jakonov (1986, 61; 1988, 67): PAA *fVdC/*-fVrC (where C denotes an unclear weak consonant in final position). V. Blažek (1987 MS, #4.2, 1990, 29; 1993 MS, 6–7, #4.1; 1999, 235–241, #; 1999, 32–38, #4) suggested PAA *fira-du/*fari-du/*faru-di. He explained Eg. & Om. *-d- vs. Bed. & Ch. *-ḍ- from a cluster *-rd-, i.e. PEg. *fida[r]wa.t < *faridwa.t (?) ||| PBed. *faridaga > *fa[rd]ig, still preserved in some old records as fardik [Krockow] = ferdik [Lucas], quoted after Almkvist (1883–1887) ||| POm. *aḇurd- or sim. ||| PCh. *faridu/*farudi (cf. Stolbova 1987, 160, #136: WCh. *firadu). This reasoning might be valid at least in Bed., cf. Bed. fű́ḍa ~ furda "Molo, Ankerplatz" < Ar. furḍ-at- "anchorage, sea-port" [Rn. 1895, 82]. In Eg. too (Eg. fd < *fʔd = *frd would be plausible). The case of Chadic is more problematic, where we would need to collect sufficient and convincing evidence for common Chadic *-ḍ- = Angas-Sura and PLay *-r < AA *-rd-. F. Kammerzell (1994, 22–26; 1994, 180), in turn, proposed a development of Eg. fd = *fiṯṯá- < *firṭá- < *firdá- to set up PAA *√PrD, var. *√PrG "four" (though *-G is not justified by the reflexes), based on Eg., Bed., LECu., NOm., Ch. "four" and Sem. *ʔarbaʕ- (!).

[36] Müller 1909, 191, fn. 2; Sethe 1916, 22, §5; 1927, 60–61; NBÄ 313; Brunner-Traut in LÄ II 582; Loprieno in LÄ V 1213, n. 26 and in VI 1308. Ultimately, the same idea was accepted by L. Homburger (1928, 336–337), albeit in a chaotic form (misquoting the Eg. word as d.t pace Lexa 1922, 176, a rudimentary mistake!) and along with a number of dubious African parallels.

[37] The older Eg. root *√srs was still preserved by srs ~ sjs "Art Leinen: Sechsgewebe" (MK, Wb IV 40, 8 and 200, 17).

[38] For the Eg.-AA etymology see Erman 1892, 117 and 127, fn. 1; Ember 1911, 89; 1912, 90, fn. 4; 1914, 303; Sethe 1916, 19–20; Albright 1918, 90, fn. 2 and 91; 1926, 188–189; Farina 1926, 21; Behnk 1927, 82, #16; ESS §4.i; Zyhlarz 1931, 134, 137; Vycichl 1934, 42, 77; 1953, 42; 1957, 21; 1958, 378; Greenberg 1955, 60; 1963, 62; D'jakonov 1965, 47 (with doubts about Eg. srs); Rössler 1966, 227; Zavadovskij 1974, 108, #9; 1975, 48; Hodge 1975, 15 and 24, #161; Loprieno 1986, 1308 and 1316, n. 25–26; Blažek 1987 MS, 31; 1999, 39–42, §6; Bomhard 1988, 446–447; OS 1988, 79, #64 (excluding Eg. srs); Dombrowski-Dombrowski 1991, 342; Lipiński 1997, 287, §35.11; Schenkel 1997, 114, Abb. 4, n. 4. Apparently ignoring the fact of an occasional development of Eg. r < *d (below), V. Blažek (1990, 39–40) surprisingly denied the cognacy of Egyptian and Semitic "6" and, instead, he preferred the phonologically naturally more comfortable equation of Eg. *srs with Sem. *ṯalāṯ- "3", which he even extended to ECu. *šazḥ-, *šizḥ-, *saziḥ - "3" explaining its *-z- with a shift of *-z- < *-ls- < *-lč-, which is attested nowhere.

[39] Cf. **(1)** Eg. rj.t "Farbe zum Schreiben und Zeichnen, Tinte" (MK, Wb II 399, 9–12) equated by Th. O. Lambdin (1953, 149) and O. Rössler (1966, 227) with NWSem. *√dy: OT Hbr. (hapax) dəyō, Aram. dəyūtā, Syr. dəyōtā, dəyūtā "ink", which is suggested to be an early loan from MEg. But even so, the change r ~ d is highly remarkable. Contrary to Rössler, Lambdin explained OT Hbr. dəyō as a graphemic error for *rəyō, which contradicts the rest of the Canaanite evidence. **(2)** Eg. ḫrd "child" (PT, Wb III 396–398) equated by O. Rössler (1971, 296, 306) with Sem.: Geez ḫədāṭ "a small amount, little, a little while, few in number …" [Lsl.], cf. Geez √ḫṭṭ "to be small" etc. (Sem.: Lsl. 1987, 269). **(3)** Eg. srq "öffnen" (PT, Wb IV 201–203) compared by O. Rössler (1966, 227) with Ar. √šdq "weit öffnen" [Rsl.] = "avoir les coins de la bouche très-larges (se dit d'un homme dont la bouche est très-large quand il l'ouvre)" [BK I 1205]. Ignoring these facts, V. Blažek (1990, 39–40) denied the cognacy of Eg. and Sem. "6" and in-

also in the Kefoid reflexes and a number of Chadic daughter languages quoted below), evidently identical with Sem. *šidš- “6” [Sethe] = *šidṯ- [Djk., Lipiński][40] ||| Brb. *√sds > *sadis (south) vs. *sddís (north) “6” [Zhl.] = *sids [Djk.] = *saḍīs ~ *sūḍus with *-ḍ- < *-dd- [Blz. pace Prasse] = *sḍis [Lipiński]. Among the derivatives of Common Afro-Asiatic “6”, the above listed forms, including Egyptian, undoubtedly represent reflexes of a NAA *√sds, whereas the related Southern Afro-Asiatic daughter languages display the original biconsonantal *√sd, which apparently had a rhotacistic variety *√sr, cf. NOm. (hardly borrowed from Ethio-Sem.): Sheko šir-itt-o “6” [Lmb.] | PKefoid (PGonga) *šir-itt- “6” [GT][41] ||| WCh. *sidu “6” [Stl. 1987, 176, #288]: Hausa *sidda [Grb., Djk.] > šídà, Sokoto dial. šíddà [Abr. 1962, 809],[42] Gwandara šídà [Mts. 1972, 108] | Ngizim sedu [Koelle] = zədù [Schuh 1981, 179] = zɨdù [Krf.], Bade əzdù [Krf.] || CCh.: Gidar sĕrrĕ [Str. 1910, 457] = ṯirre (θ-) ~ šire [Mch.] | PMusgu *ŝār- ~ *ŝir- [GT][43] || ECh.: Kwang-Modgel sidee [Lks. 1937, 96].[44] Especially noteworthy from the standpoint of SAA *√sr, is the suggestion by V. Blažek (1987 MS, 31) about a possible ancient areal parallel like PDravidian *caṟu “6” [DED §2051].

Eg. √sfḫ (masc. sfḫ.w, fem. sfḫ.t) “sieben” (OK, Wb IV 115) is identical with Sem. *šibʕ- [Conti l.c.] = *šábʕ- “7” [Dlg. 1986, 79, #16], as has long been commonly accepted.[45] The

stead, he preferred to equate Eg. *srs with Sem. *ṯalāṯ- “3”, which he even extended to ECu. *šazḥ-, *šizḥ-, *saziḥ- “3” [Sasse 1976, 138] explaining its *-z- with a shift of *-z- < *-ls- < *-lč- (attested nowhere).

[40] Most reflexes in the Semitic daughter languages reflect the third radical as *-š, only Old South Arabian has -ṯ (cf. SD 175: Sabaic s_1dṯ), which, following Garbini (1972), Loprieno (1986, 1316, n. 25–26) considered as a result of a dissimilation. The Ugaritic evidence, in turn, speaks for √ṯdṯ (DUL 900), which G. del Olmo Lete and J. Sanmartín (l.c.) explained from *√šdṯ via assimilation. For the Semitic derivatives with the assimilation of the 2nd and 3rd radicals see Brockelmann 1907, 170–171, §60.a; Moscati et al. 1964, 119, §14.8; Grande 1972, 107. Attractive is V. Blažek’s (1990, 30; 1999, 41) approach towards the partially reduplicative root structure of the Sem.-Eg.-Brb. isogloss: he supposed in PSem. an older *šid-šid- “3+3” or *šid-ṯin- “3×2” and so assumed a hypothetical PSem. **šid- (with an earlier *-ḍ-) “3”, which he identified with the isogloss of Akk. šīzum, later šizû “Drittel-Elle” [AHW 1254] ||| ECu. *šazḥ-, *šizḥ-, *saziḥ- “3” [Sasse 1976, 138]. The problem is, however, that the Afro-Asiatic evidence does not in any way support the reconstruction of Sem. **šiḍš- à la Blažek, whose 2nd redical must certainly have been *-d-.

[41] Attested in Kafa šír-itt-ō [Crl. 1951, 307] = širr-it-o [Bnd. 1971, 259] = širr-it-o [Lmb.], Mocha šīr-ítt-o [Lsl. 1959, 52] = šir-ítt-o [Bnd. 1971, 260], Shinasha sīr-t-a [Schuver in Grottanelli 1940, 103] = šir-t-a [Grottanelli 1940, 103; 1941, 266] = (Bworo) šir-ítt-ĕ [Brauner 1950, 70; Bnd. 1971, 259] = širrátà [Lmb.], Anfillo šir-t-o [Grottanelli 1940, 103; Bnd. 1971, 258; so also Lmb.] (Kefoid data: PB 1963, 468; Zbr. 1987, 384; Lmb. 1993, 379). Following E. Cerulli (1951, 309, §xxiv.1), M. Lamberti (1993, 379) and V. Blažek (1987 MS, 31; 1999, 40) too explained the Kefoid forms as loans from Ethio-Semitic *√sds, but among its reflexes he referred to (Leslau 1963, 137) there is not one single with -r- < *-d-, let alone that the Northern Omotic reflexes do not at all reflect the semi-reduplicative root *√sds. The way W. Leslau (1959, 52) argued for a borrowing (“*the Semitic Ethiopic* səddəst *was taken over in a modified form*”) did not answer any of the phonological questions. It remains thus but to accept the genetically inherited nature of Kefoid “6”.

[42] Earlier, when the rest of the Chadic data was unknown to the compartaive linguists, the Hausa word was explained as an Arabic loan (e.g., Greenberg 1945, 94 with the understandable note “*derivation doubtful*”), but the wide range of Chadic cognates (impossible to be regarded as coming from Arabic) has made it evident that the Chadic numeral is genetically inherited from the Common Afro-Asiatic lexical stock.

[43] Attested as Musgu saara (sic, s-, probably for sl-) [Roeder] = ŝáára (s̱-) [Krause] = taara (sic, t-, probably for tl-) [Overweg] = tará (sic, t-, probably for tl-) [Rohlfs], Mbara ŝírá (ɬ-) [TSL 1986, 270], Kad’a ŝírè (sl-) [Brt.-Jng. 1993, 133], Munjuk ŝaara [Trn. 1991, 117] = ŝààrà [Brt.-Jng. 1993, 133] (Musgu group data: Lukas 1941, 76).

[44] Strangely, H. G. Mukarovsky (1987, 38), equated the Chadic numeral (instead of the Sem.-Eg.-Brb. isogloss < AA *√sds) with the ECu. numeral for “3”, which he reconstructed as *√sdḥ, although H.-J. Sasse (1976, 138–139, 135) assumed ECu. *šaziḥ-/*šVzḥ- “3”.

[45] See Reinisch 1874, XII; Erman 1892, 118; Ember 1911, 91; 1912, 90, fn. 4; 1926, 308, #2; Sethe 1916, 20, §7; ESS §9.b.2; Albright 1918, 91; 1923, 68, fn. 1; 1926, 189; 1927, 199–201; Lang 1923–1924, 552; Farina 1924, 316; 1926, 14;

Amarna cuneiform evidence (šapḫa) and Coptic, cf., e.g., (S) cas=f, corroborate the vocalization *safḫ.aw (m) vs. *safḫ.at (f). The *Lautverschiebung* of Eg. *-ʕ > -ḫ was explained by K. Sethe (1916, 20, §7), F. Behnk (1927, 82), and A. Loprieno (1986, 1316, n. 27) − correctly − on the basis of the analogy of Eg. wsḫ vs. Sem. *√wšʕ "wide", although they did not realize the reason for this.[46] In fact, here we have — instead of an the influence of the *Anlaut* on the following numeral (Eg. ḫmn, cf. Blažek 1999, 43) — rather a *Lautverschiebung* generated by the incompatibility of s + ʕ in the same Eg. root (EDE I 326). As for the anomalous Eg. -f-, W. Vycichl (1958, 398) postulated a combinatory change due to the cluster -fḫ- < *-fγ- < *-pγ- < *-bγ- (?).[47] Whether the Berber numeral for "7" is also related as it was suggested by a number of scholars,[48] is, presumably, hardly a question itself, but the disturbingly anomalous loss of *-ḇ- even in the East Berber and Tuareg reflexes has to be explained,[49] cf. NBrb.: Tazerwalt ssa (m), ssa-t (f) [Prasse] || EBrb.: Ghadames sā (m), sā-t (f) [Lanfry 1973, 327, #1410] || SBrb.: Ahaggar e-ssa (m), e-ssāh-et (f) [Fcd. 1951–2, 1798] = ə-ssa (m), ə-ssāh-ət (f) etc. [Prasse 1969, 89, #620], Ghat sah-et (f) [Nhl. 1909, 66, 205]. The underlying PBrb. root is thus debatable.[50]

The attestation of this root for "7" in Southern Afro-Asiatic is sporadic and not without uncertainty, cf. LECu.: Elmolo s'ápa "7" [Heine 1980, 209] = sapa [Lmb.][51] ||| NOm. (borrowed from Ethio-Sem.?): Sheko šabātto "7" [Lmb.] | Kefoid *šab-att- "7" [GT][52] || SOm.: Hamer soʔb-a [Flm.], Karo sopb-o [Flm.] (SOm.: Bnd. 1994, 157)[53] ||| CCh.: PMafa-Mada *čib- "7" [GT]:[54] Mofu čibe (tsch-) [Str. 1922–3, 122], Gwendele cíba [Colombel], Hurzo cíba [Colombel] = číḅà [Rsg. 1978, 322, #622].

Behnk 1927, 82; Zyhlarz 1931, 137, §7; Lexa 1938, 223; Rössler 1952, 142, #66; 1966, 228; Vycichl 1958, 378; Illič-Svityč 1964, 7, #22; D'jakonov 1965, 47; Zavadovskij 1974, 109, #10; 1975, 49; Hodge 1976, 15, #162; Conti 1978, 28, fn. 2; Loprieno 1986, 1308; 1994, 120; 1995, 32; Blažek 1990, 31; Lipiński 1997, 287, §35.12.

[46] W. F. Albright (1918, 91) assumed the chain of phonetic shifts: Eg. sfḫ < *sfḥ < *sfʕ < *sbʕ. A. Ember (1926, 308, fn. 4–6) was, in turn, inclined to explain the change by "*partial assimilation*" of ʕ to f and that of b to s, for which he, however, failed to provide any parallel evidence. A. Loprieno (1994, 120) arbitrarily extracted the Egypto-Semitic parallel from a common *√sṗγ, but he failed to demonstrate the evidence for its *-ṗ- and *-γ-, which is attested nowhere.

[47] Where V. Blažek (l.c.) attributed the presence of -s- also some importance with a hint on Eg. ḫsb (PT 448c[W]), an occasional variety of standard ḫsf "abwehren" (OK–, AÄG 51, §114).

[48] Zyhlarz 1931, 137, §7; Rössler 1952, 142, #66; 1966, 228; D'jakonov 1965, 47; Vycichl 1966, 269; 1974, 63; 1992, 385; Zavadovskij 1967, 43; 1974, 109, #10; 1975, 49; Blazek 1990, 31; Lipiński 1997, 287, §35.12

[49] There is a small number of Ghadames and Augila words, where PBrb. *ḇ is not reflected as expected (namely, as ḇ), cf. Kossmann 1999, 79–80, §3.11; also Blažek 1999, 43 (discussing the case of the word for "heart").

[50] PBrb. *√swḥ > Tuareg *saḥ [Zhl.] = *assaʔu < **asbaʔu [Rsl. 1952 l.c.] = *saʕ (sic, -ʕ) < *sahʕ < **sabʕ (?) [Djk.] = *√sʔʔ [Rsl. 1966 l.c.] = *√h_1sh_2 [Prasse l.c.] = *sa [Zvd., Lpn.] = *sāh [Blz. 1990 l.c.]. In the view of Ju. N. Zavadovskij (1967, 43), the "*берберская форма представляется апокопированной*". M. Kossmann (1999, 76, §3.7, #106), in addition, who did not even list Brb. "7" among the instances of *ḇ, conceived the -h- appearing in Tuareg fem. forms (Ahaggar e-ssāh-et, Ghat sah-et) as intrusive in certain fem. numerals whose stem ends in long vowel.

[51] B. Heine (1973, 282), however, recorded Elmolo tf́paʔ "sieben", which continues ECu. *tVzb-.

[52] Attested as Kafa šabáttō (cf. šábo "70") [Crl. 1951, 307] = šabatto [Lmb.], Mocha šabátto (cf. šáb/ḅo "70") [Lsl. 1959, 49], Shinasha sawáte [Schuver] = šawata [Grottanelli 1940, 103; 1941, 266] = šâwatta [PB] = šawāta [Lmb.], Bworo šawátě [Brauner 1950, 70; Bnd. 1971, 259], Anfillo šabattó [Grottanelli 1940, 103; Bnd. 1971, 258] (Kefoid data: PB 1963, 468, 478; Zbr. 1983, 384; Lmb. 1993, 385). Generally in Ethio-Semitic and Omotic studies (e.g., Cerulli 1951, 309, §xxiv.1; Leslau 1959, 49; Lamberti 1993, 385), the Kefoid numeral is supposed to have been borrowed from Ethio-Semitic, cf. Amh. säbatt. But what explains the anomalous *Anlaut* in a loan?

[53] L. Bender (l.c.) suspected (with a question-mark) in these Southern Omotic forms borrowing from Arabic.

[54] Some of the Mafa-Mada group forms were first compared with Sem. *šabʕ- by V. Blažek (1990, 31, 38), who, however, included in this equation also his ECh. *caḅu "3" (although the evidence suggests rather *sūb-, cf. JI 1994 II 327), for which cf. rather LECu.: Elmolo sḗpe "3" [Heine 1980, 209].

It remains for later research to clarify whether the isogloss of ECu. *tVzb- “7” [Sasse 1976, 139][55] ||| POm. *tabz- “7” [GT][56] is eventually also related with a prefix t- (?) and a secondary voicing of **-s- in the cluster with *-b-, i.e., **tasb(ʕ)- > *tazb- (hence POm. *tabz- via metathesis < **tazb-?). The lack of any trace *-ʕ is, in any case, a not too supportive a circumstance.

Eg. √ḫmn (masc. ḫmn.w, fem. ḫmn.t) “acht” (OK–, WB III 282) is to be vocalized on the basis of its Amarna cuneiform reflex ḫaman (Albright 1926, 188–189) and the Coptic evidence, e.g., (S) smoun as *ḫămā́n.[ă]w, which almost perfectly coincides with Sem. *ṯamāniy- “8”.[57] This comparison has been commonly accepted[58] in spite of the disturbingly anomalous *Anlaut*. After several vain attempts at resolving this mystery,[59] the most natural reason is easy to be found, namely the influence of the *Auslaut* of the preceding numeral (√sfḫ), a quite natural factor leading to phonologically irregular numerals,[60] i.e., analogy, which V. Blažek (1999, 45, §8) in this case avoided even to mention as an alternative. Whether Brb. *tam “8” [Djk.] = *tām/*hittām “8” [Prasse] belongs to the firmly established triconsonantal Sem.-Eg. *ṯmn, is heavily debated as both the lack of the C_3 and the *Anlaut* are anomalous.[61] Turning against the conventionally accepted equation of the Egyptian, Semitic, and Berber roots mentioned above, step by step, V. Blažek (1991, 210; 1993 MS, 6, §3.5; 1999, 45, §8) excluded every single of the

[55] The East Cushitic word was borrowed into PBaz *tizzaba → PSNilotic *tɪsʌp → NMa’a sapa (Heine & Rottland & Voßen 1979, 85).

[56] Attested in NOm.: Basketo tabz-ā [Crl. 1938 III, 108], Doko tabs-ā [CR 1927, 248], Dollo tābez-ā [CR 1927, 250] | Dizoid *tubs- [GT]: Dizi tŭs-ú [Allan 1976, 381] = tus-u [Toselli 1938, 13] < *tuss- < *tubs- [GT], Sheko tubs-u [CR 1925; Bnd. 1971, 262] || SOm.: Hamer tobb-a [Crl. 1942, 262], Karo tsōb-à (sic, ts-) [CR 1927, 252], Ari tabz-a [Bnd. 1971, 263] = tabž-á [Bnd.], Galila (Ari) tabž-á [Flm.], Bako tabz-e [Da Trento 1941, 206], Dime toss-um [Bnd.] = tʊss-o [Flm.] < *tuss- < *tubs- [GT] (SOm. data: Bnd. 1994, 157).

[57] In a surprising manner, A. Loprieno (1986, 1308, n. 28), also here, misinterpreted Ar. ṯ- as a reflex of Sem. *š- (as in the case of Ar. √sdṯ < Sem. *šdš) and misleadingly presented it as a *communis opinio*, which is naturally not at all the case (cf., e.g., Moscati et al. 1964, 43, §8.59).

[58] Hommel 1883, 96, #11; Erman 1892, 116; Ember 1911, 91; ESS §10.a.32, §11.a.46; Albright 1918, 92; 1926, 188–189; 1927, 200–201; Farina 1924, 324; 1926, 20; Behnk 1928, 82, #28; Zyhlarz 1931, 137–138; Bravmann 1933, 147; Lexa 1938, 224; Rössler 1952, 146, #73; 1966, 228; Vycichl 1959, 33; 1966, 269; 1974, 63; 1992, 385; D’jakonov 1965, 47; Zavadovskij 1967, 43; 1974, 109, #11; 1975, 47; Hodge 1976, 15, #163; Loprieno 1986, 1308, cf. fn. 28; Belova 1989, 14; Blazek 1990, 31; Schenkel 1991, 116; Dombrowski-Dombrowski 1991, 347.

[59] So, for instance, K. Sethe (1916, 20, §8) correctly stated that Eg. ḫ vs. Sem. *ṯ are “*sonst nicht belegt*”, but because of m + n, such a shift may undoubtedly have taken place, and, in addition “*vergegnwärtigt man sich*” assuming that Eg. ḫ > Cpt. S worked “*ebenso wie*” Sem. *ṯ > Hbr. š, which, however, is an error and does not prove a anything about Eg. ḫ- vs. Sem. *ṯ-. Sethe concluded that “*So wird man auf die Vermutung geführt, daß in diesem* š *nahestehender Laut das Ursprüngliche gewesen sei, und daß das äg.* ḫ *nur eine unvollkommene Wiedergabe desselben darstelle*”. W. F. Albright (1918, 92 and fn. 2), in turn, assumed a chain of shifts (Eg. ḫmn < *šmn < *ṯmn), where, in his view, “š *for* θ *arises by dissimilation from the dental* n”, although, *pro primo*, OK ḫ- has not been known as a phoneme issuing from older *š, and, *pro secundo*, the expected Egyptian reflex of Sem. *√ṯmn is not at all *šmn but *smn! Of course, a shift of Eg. ḫ- < *s- is otherwise unkown. Later Albright (1927, 200–201) worked with the *Lautverschiebung* of Eg. ḫmn < *fmn < *ṯmn, which he equally failed to justify.

[60] Cf., e.g., Old Church Slavonic devętь “9” < IE *newn̥ under the influence of *desętь “10”.

[61] The Sem.-Eg.-Brb. comparison was supported by O. Rössler (1952, 146, #73; 1966, 228); W. Vycichl (1959, 33; 1966, 269; 1974, 63; 1992, 385); I. M. D’jakonov (1965, 47); Ju. N. Zavadovskij (1967, 43; 1974, 109, #11; 1975, 47). Rössler (1952, 146, #73) assumed PLibyan *tamnu(m), *tanatu (f), hence *tamn̥ (m), *tamn̥t and regarded *t- as regular (!) for Sem. *ṯ-. Later, in turn, Rössler (1966, 228) considered the *Anlaut* of the Berber numeral “*mit* t *für lautgesetzliches* s” as being due to assimiliation to “9” (Brb. *√tẓh). The change of m < *mn was explained by D’jakonov (1965, 47) via assimilation < *tamn. Similarly, for Zavadovskij (1967, 43) too, the Berber “*форма кажется апокопированной*” from the triconsonantal PAA root.

three *comparanda*. For him, Brb. *t- vs. Sem. *ṯ- was an otherwise unattested match, which is, however, not entirely true.[62] Therefore, he proposed a completely new etymology for Berber "8", namely SCu.: PRift *tam- "3" [Ehret],[63] where he assumed a pattern of (5 +) 3 = 8 to have worked just as in the case of ECu. *ša/izḥ- "3" vs. *ša/izzet- "8". This sugestion seems indeed attractive. But Blažek also found (pace Holmer 1966, 35) it evident that Eg. ḫmn is "*deriving quite naturally from*" Eg. ḫmt "3" (!) in the same way, although he did not explain this derivation, e.g., how did the -t of "3" disappear in "8", or, what was the function of -n of the latter numeral. Thirdly, in Sem. *ṯamāniy- "8", instead of a genetically inherited root *√ṯmn, he saw an inner Semitic innovation from the contraction of a hypothetic compound **tāniy-mā/**ṯanīy-mā "the second one no", or alternatively from **tāniy-/ṯanīy-min-(ʕaŝar-) "the second from (ten)". All this fails, however, due to the fact that the same PAA biconsonantal root *√čm for "8" appears also in NOm.: PKefoid (Gonga) *šim-itt- "8" [GT].[64] A borrowing from Ethio-Semitic[65] is hardly the case with the Kefoid numeral (isolated within Omotic) for several reasons.[66] It is here to be remarked that the Egyptian, Semitic, and Berber numerals "8" were compared by W. Vycichl (1959, 33) also with Bed. asemhá̶y ~ asumha̶y "acht" [Rn. 1895, 31] = asimhɛ́i [Roper 1928, 155] in spite of its analysis as a compound commonly accepted since L. Reinisch (1894, 7).[67]

Eg. √psḏ "nine (9)" (OK, Wb I 558) is a word with a very difficult etymology,[68] traditionally identified with Semitic *tiš(a)ʕ- "9" [GT] (Semitic data: Moscati et al. 1964,

[62] Cf. SBrb.: EWlmd. a-tăkămma, pl. i-tăkămma-t-ăn "bras supérieur" [PAM 2003, 785] ||| Sem. *ṯVkm- "neck and shoulders" [SED]: Ug. ṯkm "1. Nacken mit Schulter, 2. oberer Teil eines Gebäudes" [WUS] = "shoulder" [DUL 903], Hbr. šəkem "der Nacken mit den Schulterblättern, bes. als Körperteil, auf dem man eine Last trägt, der Teil des Körpers (Rücken), auf den man jem. schlägt, 2. Landstrich, eigtl. Rücken des Landes" [GB] = "1. the (nape of the) back or neck of a person, 2. shoulder (as a part of the body on which to carry a heavy load), the shoulder joint (as a part of the carcass of a sacrificial animal)" [KB] (Sem.: GB 826–7; WUS 334, #2866; Faber 1984, 210, #50; Lsl. 1987, 496; Voigt 1994, 107; KB 1492–3; SED I 251, §281) ||| PCu. *sVnk^{w}- "1. затылок, спина, плечо, 2. то место, на котором носят грузы" [Dlg.] = *sVkm- → *sVmk- "shoulder" [GT]. From AA *√čkm "shoulder" [GT]. Cf. also Dlg. 1983, 136, #9.2 (Sem.-Bed.-LECu.). Hardly a borrowing from Arabic, where its reflex (if related at all …) has undergone serious semantical shift, cf. Ar. ṯakam- "1. (tracé du) chemin, (milieu de la) route" [BK I 231b] = ṯakam-, ṯukm-at- "1. milieu (du chemin), 2. chemin, voie" [Blachère 1210a] = ṯakm- (sic) "shoulder (of road)" (sic) [Faber]. Besides, A. Ju. Militarev (1991, 242) admitted AA *č > Brb. *s, (?) *š, and also *t (no question-mark), although he did not provide the lexical evidence.

[63] Which was combined by Ch. Ehret (1980, 290) with Dahalo ʔíṯṯāṯóni "3rd day after tomorrow" to reconstruct SCu. *ʔitām- "tris, set of three".

[64] Attested in Kafa šim-itt-ō [Crl. 1951, 307; Bnd. 1971, 259] = simm-ít-o [PB] = šimm-itt-o [Lmb.: so also in Sheko!], Mocha šim-ítt-o [Lsl. 1959, 51; Bnd. 1971, 260], Shinasha sim-īt-a [Schuver in Grottanelli 1940, 103] = šim-at-a [Grottanelli 1941, 266] = šim-ítt-a [PB] = šəmm-ətt-à [Lmb.], Bworo šim-ítt-ĕ [Brauner 1950, 70; Bnd. 1971, 259], Anfillo šim-itt-ó [Grottanelli 1940, 103; Bnd. 1971, 258] (Kefoid data: PB 1963, 468; Zbr. 1983, 384; Lmb. 1993, 376).

[65] As suggested by E. Cerulli (1951, 309, §xxiv.1) and M. Lamberti (1993, 376).

[66] Hardly to be explained from *šimin-t- to have the 3rd radical of ES *√smn (as suggested by W. Leslau 1959, 51 with a hint on some Gurage dialects, where -n- was not preserved, cf. Chaha sumut, Muher, Selti səmmut, the vocalization of which do not fit, however), since, suspiciously, Kefoid 6, 7, 8 all have this suffix -Vtt-. In addition, how could ES *s- have become Kefoid *š- if it was a borrowing?

[67] The Bedawye numeral is evidently not an Arabic loan. According to the usually accepted segmentation, the Beja numerals from "6" to "9" are formed on the basis of the pattern of Bed. asa "growing" + "1", "2", "3", "4" (cf. Bed. √mhy "3").

[68] Any inner Egyptian derivation is vain here. Declining its commonly accepted Semitic etymology, V. Blažek (1999, 251) tried to explain Eg. psḏ "9" on the basis of Eg. psḏ "sich entfernen von, sich abwenden von (r)" (PT, ÄWb I 479; Wb I 556), i.e., "9" < psḏ{-mḏ.w} "[one] removed away from {ten}". However, Blažek ignored that the latter is a denominative verb of Eg. psḏ "back", and so it may literally have denoted *"den Rücken wenden" (Wb).

116),[69] which may seem impossible at the first glance as, in fact, only the second radicals correspond. The initial p- in Eg. instead of an expected *t- is unusual, which, after a few vain attempts,[70] W. F. Albright,[71] followed by others,[72] correctly explained by the incompatibility of OEg. *ts.[73] But they never discussed the question as to why this sequence turned into Eg. ps-. It is due to another incompatibility law, namely that of OEg. *sʕ, which had to turn either to *sḫ (cf. EDE I 326) or *sḏ (the irregular correspondence of Eg. -ḏ vs. Sem. *-ʕ occurs in a number of convincing examples, among which there are also roots devoid of s).[74] In either cases, we get a third radical which is compatible with p- only, the other possible voiceless stop to replace t- being k-, which is incompatible with both -ḫ and -ḏ. The choice between -sḫ vs. -sḏ was probably decided under the influence of Eg. mḏ "10".

Whether and how Berber "9" (usually bearing the consonants √tẓ or √tz), reconstructed in various forms,[75] and frequently included in the Egypto-Semitic etymology above,[76] can be related, is disputed. It is evident, that the medial radicals (Brb. *-ẓ- vs. Sem. *-š-) are not at all in agreement. In addition, V. Blažek (1999, 47) excluded the relationship of the Egypto-Semitic isogloss to Berber "9", which he explained as a contraction of *t(V)-[k]ūẓah "[5] + 4", cf. Brb. *hakkūẓ "4" [Prasse].

The Southern Afro-Asiatic evidence of the root for "9" reflected in Semitic and Egyptian is scarce. It occurs in fact only in ECh. *√tgs ~ *√gst "9" [GT][77] as suggested by A. Trombetti

[69] This Semito-Egyptian equation was accepted by A. Erman (1892, 111); W. M. Müller (1907, 303); A. Ember (1911, 91; 1912, 90, fn. 4; ESS §8.c, 112, §18.a.9, §24.d.4); F. Hommel (1915, 16, #2); K. Sethe (1916, 20); W. F. Albright (1918, 92; 1923, 68; 1926, 189; 1927, 201); E. Zyhlarz (1931, 138, §7); Sh. Yeivin (1932, 137); H. Mercier (1933, 313–314); O. Rössler (1966, 228; 1971, 302, 307); Ju. N. Zavadovskij (1967, 43; 1974, 109, 112; 1975, 49); KHW 153; W. Schenkel (1990, 52, 57; 1991, 116; 1997, 114); J. Zeidler (1992, 205); G. Takács (1999, 141; 2000, 343–344, #8.3; EDE II 516–7). The same comparison was declined by C. T. Hodge (1976, 15, #164), V. Blažek (1997, 16; 1999, 250–251, #9; 1999, 46–47, #9), and E. Lipinski (1997, 288, §35.14).

[70] E.g., K. Sethe (1916, 20) compared this phenomenon to the regular change of PIE *k^{w}a/o- → Gk. πα/o- vs. PIE *k^{w}e- → Gk. τε-, which has, however, not been established in the Egyptian *Lautgeschichte* as a regular shift.

[71] Cf. Albright 1918, 92; 1923, 68; 1926, 189; 1927, 201.

[72] O. Rössler (1966, 228; 1971, 302, 307), W. Schenkel (1990, 52, 57).

[73] This reasoning seems acceptable, since the sequence of word initial *ts- is not attested in Old and Middle Egyptian (cf. Wb I 328). Similarly, J. H. Greenberg (1950, 176) observed no instance of a dental followed by a sibilant in the Semitic root stock either except for Sem. *√tšʕ "9". For the frequent incompatibility problems in the Semitic numerals 1–10, cf. Greenberg 1950, 178, §5.

[74] **(1)** Eg. sḏm < *smḏ "to hear" (OK, Wb IV 144) ||| Sem. *√šmʕ "to hear" [GT] (Eg.-Sem.: Hommel 1882, 9; 1894, 351, fn. 1; 1915, 16, fn. 3; Müller 1907, 303; Ember 1911, 91; 1912, 90, fn. 4; 1918, 30; 1926, 6; 1926, 309, fn. 8; Yeivin 1932, 137; Vycichl 1934, 63; Vergote 1945, 142, §16.b.23; Cohen 1947, #82; Schenkel 1993, 143 etc.). **(2)** Eg. nḏs "klein, gering" (PT-, Wb II 384–385) ||| Sem. *√nʕs "to be small, weak" [GT] (cf. Hommel 1883, 441, fn. 30; 1894, 351, fn. 1; 1915, 16, fn. 3; Erman 1892, 113; Ember 1912, 90, fn. 4; 1926, 6; 1926, 309, fn. 8; 1930, §11.a.43, §24.d.2; Vycichl 1934, 63; Vergote 1945, 147, §24.b.2; Cohen 1947, #80; Rössler 1966, 228). **(3)** Eg. nḏm "süß, angenehm" (OK, Wb II 378–380) ||| Sem. *√nʕm "to be pleasant" [GT] (cf. Hommel 1883, 98; 1894, 351, fn. 1; 1915, 16, fn. 3; Erman 1892, 113; Müller 1907, 303; Ember 1911, 91; 1926, 6; ESS §10.a.25, §11.a.41; §24.d.1; Vycichl 1934, 63; Vergote 1945, 147, §24.b.1; Cohen 1947, #81; Schenkel 1993, 143; Loprieno 1994, 120). **(4)** Eg. ḏns "to be heavy" (MK, Wb V 468–469) ||| LECu. *ʕils-/*ʕuls- "heavy" [Sasse 1975, 245; 1976, 127] proposed by O. Rössler (1966, 228).

[75] PBrb. *təẓah (?) "9" [GT] = *√tsʔ [Rössler 1966, 228] = *taṣṣaʔu [Rössler 1952, 143] = *tẓa [Zavadovskij 1974, 109; 1975, 49] = *tiẓāh ~ *tūẓah [Prasse 1974, 403, 404].

[76] See Zyhlarz 1931, 138, §7; Mercier 1933, 313–314; Vycichl 1938, 135; 1966, 269; 1974, 63; 1992, 385; Rössler 1952, 143, #74; 1966, 228; 1971, 302, 307; Zavadovskij 1967, 43; 1974, 109, 112; 1975, 49; Zeidler 1992, 205; Takács 1999, 141; 2000, 343–344, #8.3.

[77] Cf. Lay group *√tgs [GT]: Dormo tigesu [Hfm.], Gabri tigesu [AF] = tegès [Dcr.], Chire tíngěšū [Hfm.], Kabalay tegesu [Hfm.], Lay tegese [Hfm.] | PSomray *√ts or *√ds [GT]: Somray dōso [Barth], Ndam disa [Bruel] = tiše

(1977, 53) and G. Takács (1999, 141; 2000, 343–344, #8.3). The phonological correspondence of ECh. *-g- < AA *-ʕ is not yet proven, however. As for the metathesis in East Chadic, it is noteworthy that V. Blažek (1990, 32; 1991, 210) supposes Sem. *tišʕ- "9" to reverse the order of Sem. *ʕašt- "1".

Leaving aside the equation with Semitic "9", G. Takács (EDE II 517–518) discussed all other alternatives (q.v.), and among others he ventured an alternatively a comparison of Eg. psḏ < *√psʕ with NOm. *√bz (stem vowel *-i-) "1" and "9" [GT],[78] which apparently stands isolated in Afro-Asiatic.

Eg. √mḏ (masc. mḏ.w, fem. mḏ.t) "zehn" (OK, Wb II 184): in spite of the abundance of various etymologies suggested until very recently a completely satisfactory solution has not been found. In any case, the Amarna cuneiform (14th cent. BC) evidence (mu-ṭu)[79] and Cpt. (SALMB) myt "ten" (CD 187b) suggest *mṹḏaw (m) vs. (f) *mṹḏˇt (Edel 1955, 166–176). Leaving aside the evidently untenable etymologies,[80] we may only describe all the considerable solutions:

(1) F. Behnk (1928, 139, #33) saw in Eg. mḏ [possibly < *mg] a metathesis of WCh.: Hausa góómà "10" [Brg. 1934, 397; Abr. 1962, 332] = góómàà [JI]. I.e., Eg. *mṹḏ.ˇw < **ḏṹm.ˇw < pre-OEg. **gṹm.ˇw? It is highly noteworthy that the sequence ḏm- was not typical in Egyptian. Regarded as "possible" also by V. Blažek (1989, 215–216; 1997, 17; 1999, 251–3, §10; 1999, 47–49, §10) and Ju. N. Zavadovskij (1974, 104; 1975, 50–51). The Hausa numeral for "10" is a reflex

[Décorse], Tumak disa [Décorse] = bisa [Bruel], Miltu disa [Hfm.], Sarwa doso [Hfm.] | Mokilko gɛ́ssát [Lukas 1977, 210] = géssá(t) [Jng. 1990, 101] (ECh. data: Hoffmann 1971, 9).

[78] Attested in SEOmeto *bizz-o "1" [GT]: Haruro (Kachama) bĭzz-o [Crl. 1936, 631, 642] = biz-ɛ [Sbr.], Zayse bizz-ō [Crl. 1938 III, 201] = bizz-o [Sbr.], Zergulla biz-o [Sbr.], Koyra (Badditu) bizz-ō [Crl. 1929, 60] = bīʒ-o [Bnd.] = bížž-o [Hyw. 1982, 215] = bɪʒʒ-ɔ [Sbr.], Gidicho bīz-e [Bnd.] (SEOmeto: Bnd. 1971, 256–257; Zbr. 1983, 387; Sbr. 1994, 18) | Chara biz-ā "9" [Crl. 1938 III, 165] = biž-a ~ biʒ-a "9" [Bnd. 1974, 19; Flm. 2000 MS, 7] | Sezo bɛṣ-ɛ́ "9" [Sbr.-Wdk. 1994, 15].

[79] Occurs in a list of Egyptian words (EA 368), cf. Smith & Gadd 1925, 230–8, esp. 236, §15; Lambdin 1958, 186; Edel 1975, 11f.; 1980, 17 & fn. g.

[80] **(1)** A. Trombetti (1902, 198), C. Brockelmann (1908, 487), W. Worrell (1926, 272), and G. A. Barton (1934, 30) erroneously equated LEg. md, Dem. mt, and Cpt. (S etc.) myt with Sem. *miʔ-át- "hundred" [Dlg.], which has rightly been declined by W. F. Albright (1918, 92, fn. 6), later also by F. A. Dombrowski and B. W. W. Dombrowski (1991, 342) and by V. Blažek (1999, 251–3, §10; 1999, 47–49, §10). **(2)** There is a long tradition of comparing Eg. mḏ with the reflexes of PBrb. *mḏraw "10" [Zhl. 1934–35, 185] = *marāw [Prs. 1974, 403, 405] = *mra (m), *mra-ut (f) [Zvd. 1975, 50–51, §14.0] = *märäw (sic) [Vernus] = *maraw [Mlt., GT], cf., e.g., Gabelentz (1894, 99); Meinhof (1912, 240); Zyhlarz (1931, 137–138, #8; 1932–1933, 104; 1934, 104, 106, 111, fn. 1); Mercier (1933, 314); Wölfel (1954, 58); Lefebvre (1955, 276) and Korostovcev (1963, 14): both misquoting the Brb. root as mzu (sic!); Rössler (1966, 227; 1971, 317); Zavadovskij (1967, 43; 1974, 111–112; 1975, 50–51, §14.0); Loprieno (1986, 1309); Blažek (1989, 215–216; 1990, 41; 1997, 17–18); Dombrowski and Dombrowski (1991, 344); Vernus (2000, 180, 192): Eg. mdw (sic) "*a un cognat possible avec le berbère*"! Rejected by W. Vycichl (DELC 124) and G. Takács (1995 MS, 4, #7; 1996, 139, #35; 1996, 442, #2.3) as there is no evidence for Eg. -ḏ ~ Brb. *-r-, while Brb. *-w is part of the root (contrary to Eg. masc. mḏ.w vs. fem. mḏ.t). **(3)** K. Sethe (1916, 17) and A. Loprieno (1986, 1309): Eg. mḏ "10" < mḏ "deep", but they failed to demonstrate the odd semantic shift with typological parallels. V. Blažek (1997, 17; 1999, 251–3, §10; 1999, 47–49, §10) excluded a direct connection. **(4)** Ju. N. Zavadovskij (1974, 112; 1975, 44) and A. Loprieno (1986, 1316, n. 32): metathesis of PCu. *√tmn "10". Absolutely unlikely. Eg. -ḏ ≠ Cu. *t-. Cu. *-n not reflected in Eg. **(5)** I. M. D'jakonov (1986, 61; 1988, 67): ~ Sem. *maʔd- "many", but Eg. ḏ ≠ Sem. *d. Declined already by V. Blažek (1989, 215–216; 1997, 17) and G. Takács (1994, 217; 1996, 139–140, #35; 1996, 442, #4; 1999, 136; 1999, 203). **(6)** A. Loprieno (1986, 1309, 1316, n. 33) suspected the ultimate common origin of Eg. mḏ "10" and mḏ "deep" with Sem. *√mṣṣ "aufsaugen"(!), *√mdd (!) "lang ziehen, ausdehnen", *√mṭṭ (!) "lang ziehen, ausdehnen". Impossible. E.g., how should one figure a relationship between "aufsaugen" vs. "10"? Rejected already by V. Blažek (1999, 251–3, §10; 1999, 47–49, §10).

of PCh. *g^{w}am- “10” [Nwm. 1977, 32] = *√g^{w}m [JS 1981, 263; JI 1994 I, 165].[81] C. Hoffmann (1970, 12–14) and H. Jungraithmayr & D. Ibriszimow (1994 I, 165) considered PCh. *√g^{w}m “10” to be an old Niger-Congo loan (cf. Benue-Congo *-kumi “10”), which would exclude its equation with Eg. mḏ. However, a genuine AA etymology of PCh. *√g^{w}m is also possible, cf. AA *√gm “complete (or sim.)” [GT]. V. Blažek (1987 MS, 41), in turn, combined the PCh.-Eg. parallel with SBrb.: Ahaggar a-g^{y}im (-ġ- apud Fcd.) “millier” [Fcd. 1951–2, 444], Ghat a-ǯim (a-djim apud Nehlil) [-ǯ- < *-g^{y}-] “mille” [Nhl. 1909, 179].

(2) V. Blažek (1987 MS, 41; 1990, 41) equated Eg. mḏ with CCh.: Higi gr. *muŋ- “10” [GT],[82] which might only be valid if Eg. *mŭḏ.˘w < **mŭnḏ.˘w (nowhere attested) and if the Higi numeral < **mung-. Mentioned also by G. Takács (1994, 217) in the context of further AA parallels. The etymology of Higi gr. *muŋ- “10” is uncertain.[83]

(3) C. T. Hodge (kind p.c. on 4 September 1994) has not excluded a connection with PBrb. *tē-mihḍay, pl. *tī-muhāḍ “100” [Prasse 1974, 406].[84] Since PBrb. *ḍ < PAA *ĉ (cf. Mlt. 1991, 242; Takács 2006, 57–59, 62), the phonological correspondence of Eg. ḏ ~ PBrb. *ḍ is regular, although PBrb. *-h- has no match in Eg. mḏ. The etymology of the Berber numeral is obscure.[85]

(4) V. Orel & O. Stolbova (1992, 202) identified it with their ECh. *m^{w}aǯ- “10” (no reflexes mentioned), which is certainly a false reconstruction. This asterisk-form is solely based on the

[81] Attested in WCh.: Gerka (Yiwom) [IL] | Dera (Kanakuru) gum [Pls.] = gûm [Krf., Jng.], Tangale gbọmọ [Jng.] < *g^{w}om- [GT] | (?) Tsagu wúúma [Skn. 1977, 34: < PCh. *g-m-] | Ngizim (< Hs.?) guma [IL] = gumə̀ [Krf.] = gúumà [Schuh], Bade (< Hs.?) gúmā [IL] = guumà [Krf.] (WCh.: also Pls. 1958, 85) || CCh.: Tera gwàŋ [Nwm. 1964, 36, #10], Tera-Jara gwom [Nwm.], Hwona gumdìḍi ~ kûm [Krf.], Boka kum [Krf.], Gabin kùm [Krf.], Ga’anda kum [Krf.] | Bura-Margi *kum- [GT] > Margi kūmú [Hfm.] = kumu [Krf.], WMargi kuma ~ kumɛ [Krf.], Chibak kymɛ [IL] = kuma [Krf.], Bura kuma [Krf.], Wamdiu kumò [Krf.], Hildi kúmɔ̀ [Krf.], Kilba kúmà [Krf.], Ngwahyi kuma [Krf.] | Fali-Kiria gwùm(ù) [Krf.], Fali-Jilbu gumù [Krf.], Fali-Mucella gùm [Krf.], Fali-Bwagira po-gumu [Krf.] | PMandara *g^{w}amgV (?) [GT]: Dghwede gwàŋgá [Frick] = ƥwáŋgá [IL], Ngweshe ùwáŋgò [IL], Paduko ǯuma [Mch.] | Sukur úwâŋ [IL] < *g^{w}am (?) [GT] | Musgoy gup [Mch.], Daba gúƥ [Lienhard] | Musgu gum [Roeder] | PMasa *g^{w}uƥ- < **g^{w}um- (?) [GT]: Lame gwúƥú [Krf.], Lame-Peve gwúƥ [Krf.], Zime-Batna gùp [Jng.] = gùƥù [Scn.], Misme-Zime goub [Krf.] || ECh.: Mokilko kòòmá(t) [Jng.] (Ch.: Mkr. 1987, 43, 222; Ibr. 1990, 211–212; JI 1994 II 320–321).

[82] Attested in Higi mĕngĕ́ [Str.] = m^{w}ɷŋ́ɛ́ [Mrl. 1972, 102] = mùŋəy [Brt.-Jng.], Higi-Nkafa mùŋəy [Krf.], Higi-Baza mūnge [Lks. 1937, 113] = mùŋə [Krf.], Higi-Kamale mùŋɛ [Krf.] vs. Kapsiki (= Kamale?) măng [Str.] = məŋ [WL] = məŋ(ə́) [Brt.-Jng.], Higi-Ghye mùŋəy [Krf.], Higi-Bana mə̀ŋə̂ [WL] = mə́ŋ [Brt.-Jng.], Higi-Futu mùŋi [Krf.], Fali-Gili mùŋ [Krf. 1972 MS] (Higi group data: Strümpell 1922–1923, 123; Wente-Lukas 1973, 7; Kraft 1981 II, 131, 141, 151, 161, 171, 191, #10; Brt.-Jng. 1993, 131).

[83] Contrary to V. Blažek (l.c.), D. Ibriszimow (1990, 211–2) excluded a metathesis of PCh. *gum-/*g^{w}am- “10” (above). Later, Blažek (1999, 251–3, §10; 1999, 47–49, §10) derived Higi gr. *muŋ- “10” from *mu-mg-, which might be etymologically identical with Agaw *mang- “many” [GT] || LECu. *mang- “many” [GT] ||| NOm.: Shinasha manga “heavy” [Lmb.] (discussed below). If this is correct, a remote kinship between Higi gr. *muŋ- with Eg. mḏ is not impossible.

[84] Attested, a.o., in NBrb.: Nefusa te-mîṭi [Mtl.] = tə-miṭi [Lst.] = te-miti [Mrc.] || EBrb.: Sokna sənnət t-mîtin “deux cents” [Lst.] || WBrb.: Zenaga ta-māde (sic, -d-) “100” [Ncl. 1953, 206] || SBrb.: Ahaggar té-méḍé, pl. ti-maḍ “centaine” [Fcd. 1951–2, 1165] = ti-miḍi [Mtl.] = tə-miḍi [Lst.] = ti-miḍi [Mrc.], ETawllemmet ti-miḍi [Bst.] = ETawllemmet & Ayr te-meḍe ~ Ayr ti-miḍa “1. centaine, 2. cent” [PAM 1998, 210; 2003, 524], Kel Ui ti-maḍi [Wlf.], Ghat či-miḍi “cent”, senat či-maḍ “deux cents” [Nhl. 1909, 138; Mrc.] (Brb.: Lst. 1931, 209; Mrc. 1933, 316; Wlf. 1954, 74).

[85] **(1)** A. Klingenheben (apud Wölfel 1954, 75) and M. G. Mercier (1933, 316) erroneously explained it as a late borrowing from Ar. miʔ-at- (!), which has rightly been excluded by Wölfel (l.c.). Surprisingly, this erroneous equation of the Berber numeral with Sem. *miʔ-at- “1.000” has been recently adopted by E. Lipinski (1997, 291, §35.20). **(2)** F. Nicolas (1953, 206) combined it with WBrb.: Zenaga √md “finir, être fini”. **(3)** GT: cf. ECh.: Mokilko mèedá (f) “cent, centaine(s)” [Jng. 1990, 138], although Mokilko -d- vs. Brb. *-ḍ- seem irregular.

isolated ECh.: Somray moʒ “zehn” [Nct. apud Lks. 1937, 80; Hfm. 1971, 9] = mwàʒ “10” [Jng. 1993 MS, 46; JI 1994 II, 321]. In theory, there could be a small chance that the Somrai form derives from an earlier *√m(w)g,[86] but this is surely not the case here due to the firm evidence for that Somray moʒ [Nct.] reflects *√mwd.[87] The Afro-Asiatic background of the ECh. numeral is disputed. V. Blažek (1997, 18; 1999, 251–3, §10; 1999, 47–49, §10): < *mVḍV ~ Eg. mḏ and even PBrb. *tē-mihḍay “100” [Prasse 1974, 406]. In principle, Somray -ʒ < ECh. *-ḍ < AA *ṣ/*č̣/*ĉ is plausible,[88] but we have insufficient evidence for *-ḍ- in the East Chadic numeral against *-d-. Consequently, the available records provide hardly anything for equating Eg. and ECh. “10”. G. Takács (1999, 136; 1999, 202–203, #3.2) connected ECh. *√m(w)d with Sem. *maʔd- “many” [Djk.] ||| PBrb. impf. *ya-mduh, pf. *yu-mdah [Prasse 1975, 227] = *ə-mdu < *√md[h] “to complete” [GT] ||| SOm.: Ari mūda “all” [Bnd. 1994, 1158, #1]. If this comparison proves to be valid, the East Chadic numeral can have nothing in common with Eg. mḏ.

(5) G. Takács (1994, 217–218; 1995 MS, 5–6, #7; 1996, 140, #35; 1996, 443, #7; 1999, 40, 50–51, 143) affiliated Eg. mḏ “10” with ECu. *mig-/*mug- “fullness”, *-mg- (prefix verb) “to fill” [Sasse 1979, 25] = *-meg- “to be full” [HL 1988, 127; Lmb. 1993, 353] = *-mig- “to be full” [Ehret 1997 MS, 196, #1771] = *mVg- “many, full” [GT].[89] This Egypto-East Cushitic equation was as-

[86] Cf. perhaps ECh.: Somray ʔáʒə̀ [Jng.] vs. Ndam yəgə̀ “to cut, chop” [Jng.] (ECh.: JI 1994 II, 99).

[87] Attested by its earlier record and its closest cognates listed by J. Lukas (1937, 74, 87) and C. Hoffmann (1971, 9): Somrai moid “10” [Adolf Friedrich] = moet [Gaudefroy-Demombynes], Dormo moid [Adolf Friedrich] | Gabri moid [Adolf Friedrich] = mwòʒə̀ [Caprile 1972 MS], Chire moodo “10” [Barth apud Lukas].

[88] Cf. ECh. *gaḍ-"cheek” [GT]: Kabalai kwaʒí [Cpr.] | Somray gàʒé “cheek” [Jng.] | WDangla gàḍùmò [Fédry] | Birgit gàḍáyó [Jng.] (ECh.: JI 1994 II, 69) ||| SBrb.: Ahaggar ă-g^{y}/gaẓ (-ġ- apud Fcd.) “joue” [Fcd. 1951–2, 491] ||| PCu. *gAc(c)- “лицо, лоб” [Dlg.] > Bed. gẽdi “das Gesicht, Antlitz, Auge” vs. g^{w}ad ~ g^{w}áda ~ g^{w}aʒ ~ gaʒ “Auge, Gesicht” [Rn. 1895, 89–90] = (also) g^{w}aḍ, pl. g^{w}aḍa “face, eye” [Dlg.] || NAgaw *gäc “face” [Apl.] = *gäç (?) [GT]: Bilin gäš, Hamir gaṣ, Qwara-Dembea gaš, Qemant gäš (NAgaw: Apl. 2006, 63) || ECu.* gaḍ- “jaw” [Apl., KM] || SCu.: WRift *gicē “forehead” [KM 2004, 117] < AA *√gç/č̣ “cheek” [GT] (cf. Cohen 1947, #197; Dolgopol’skij 1973, 297; HSED #866 vs. #914).

[89] Attested in Saho mag “anfüllen, voll machen” [Rn. 1890, 258–9] = mag “remplir” [Chn.] = -meg- (prefixed) “to fill” vs. mig-e “fullness” [Sasse] = -emmeg- “to be full” [HL] = emege (imp. amage) “to fill”, mig-e “fullness” [Vergari 2003, 78, 135], Saho-Assaorta mag-, pass. m-mag “essere molto, in molti, essere pieno” [CR 1913, 70] = meg- “to be numerous, full (быть многочисленным, полным)” [IS], Afar mag “anfüllen, voll machen” [Rn. 1886, 880] = -eng- [< *-emg-] “to fill” [Sasse] = -emmeg- “to be full” [HL] = enge “to fill” [PH 1985, 163], Afar-Tadjurah mog-o “many (много)” [IS] | Oromo mog-a “fullness”, miʒ-ū [-ʒ- < *-g-] “full” [Sasse], Oromo-Waata magā-ta “many” [Strm. 1987, 362], Oromo-Bararetta imieke “full” [Flm.], Konso imako-ta “full” [Flm.] = immak- “to be full” [HL], Gidole innako-ta “full” [Flm.] = innak- “to be full” [HL], Gato imako-da “full” [Flm.] | OSomali *ammūg- “füllen” [Lmb. 1986, 437] > Somali mug “Fülle, Vollheit” [Rn. 1902, 288] = múg- “fullness” [Abr. 1964, 182], PBaiso & Jiddu (sic) *ʔu/img- “full” [Ehret & Nuuh Ali 1984, 229], Baiso mig-i “full” [Flm.] = mig-i “to be full” [HL] = ʔamoga “many” [Sbr. 1994, 17] | Yaaku -mok [< *-mog], pl. -mṓʒeʔ “many, much” [Heine 1975, 130] (ECu.: Dlg. 1973, 256–257; Sasse 1979, 25; HL 1988, 127). In H.-J. Sasse’s (1979, 25) view, the Konso & Gidole parallels (with -n-/-k-) “*are obviously cognate, but display problematic correspondences*”, for which cf. NAgaw: Kemant imkuy “être abondant (le blé)” [CR 1912, 164] ||| WCh.: Tangale mụkmụk “somewhat full” [Jng. 1991, 121] || ECh.: EDangla mak “(idéophone d’accomplissement)” [Dbr.-Mnt. 1973, 192]. Do these parallels display traces of an AA root var. *√mk “full” [GT]? The relatedness of further possible parallels is still to be cleared, cf. LECu.: Rendille mig, pl. amíge, mimígé “strong, hard” [Heine 1976, 216, 220] = mīg (f) “Kraft, Macht” [Schlee 1978, 140, #774] = míg-e “strength” [Oomen 1981, 72] = mǵ “strength, stiffness, tightness, heaviness, hardness, difficulty” [PG 1999, 224] ||| NOm. *magg- “1. full (?), 2. (hence) heavy” [GT]: Haruro māgg-āys “essere contento” (lit. “to be full”?) [CR 1937, 653] | Kefoid *magg- “to be heavy” [GT]: Kaffa mag- [Crl. 1951, 470] = magg- [Dlg.], Mocha màggi-yé “to be heavy”, magg-o “heavy” [Lsl. 1959, 40], Sheko maggo “heavy” [Lmb.] (NOm.: LS 1997, 459 with semantically false comparanda) is semantically problematic. For the ECu.-NOm. comparison see Dlg. 1967, 9, #7; 1973, 256–257; IS 1976, 41–42; Lmb. 1993, 111 (Cu.-Om. *-mVg- “to be full, heavy”).

sessed by V. Blažek (1999, 251–3, §10; 1999, 47–49, §10) as the "*most convincing*" one of all the etymologies offered so far for Eg. mḏ. The reflexes of ECu. *mig-/*mug- [Sasse] and NOm. *magg- "full" [GT] have often[90] been compared with the Cushito-Omotic root containing an additional *-n-, cf. *√mng "much" [GT],[91] on whose etymology there is no agreement in Cushitic studies.[92] The ultimate source of Eg. mḏ and ECu.-NOm. *mVg- "1. many, 2. full, 3. heavy, 4. strong (?)" [GT] may be AA *√mg "1. big, 2. long, high" [GT].[93] The semantic shift of Eg. mḏ "10" as a "full, big" number is supported by a number of typological parallels.[94] The

[90] Cf. Reinisch 1886, 880; 1890, 259; Conti Rossini 1913, 71; Leslau 1945, 163; 1979 III, 408–9; Illič-Svityč 1976, 41–42; Appleyard 1977, 26/68; Haberland-Lamberti 1988, 127; Lamberti 1993, 353; Lamberti-Sottile 1997, 459 (with semantically false comparanda).

[91] Cf. NAgaw: Qemant māngā "foule, quantité, multitude" [CR 1912, 230] = manga "multitude, crowd" [Lsl.] (Appleyard, p.c. on 20 April 2007: "*without any doubt a loan from*"Amharic mänga "herd, flock, crowd", which, in turn, is "*obviously a loan from ECush.*") || SAgaw *menči [-či < *-ki] "many" [GT]: Awngi ménč "many" [Htz./Bnd. 1971, 238, §50] = mʸɛnŋči (so!) [Flm./Bnd.] = ménči [Bnd. 1973 MS, 7, #51] = ménč "many" [Apl. 1991, 8], Kunfal menči "many" [Birru & Adal 1971, 102, #50] = mɨnči "many" [Bnd. 1970, 3, #50] || LECu. *mang- "numerous" [GT] > Saho mang "viel, zahlreich werden, sich mehren" [Rn. 1890, 259, 269–270], Afar mang "angefüllt, voll werden/sein" [Rn. 1886, 880, 882] ||| NOm.: Shinasha-Bworo mang-á "heavy (schwer, gewichtig)" [Lmb. 1993, 111; 1993, 353].

[92] The Saho-Afar stem *mang- has been explained by L. Reinisch (1886, 880 1890, 259) from a pass. *m-ang "angefüllt werden", cf. Saho-Afar caus. s-ang < √mag. C. Conti Rossini (1913, 71) extended this also to NAgaw (Kemant) assuming a common PCu. *mag > *m-mag > *mamg > Kemant & Saho-Afar mang-. G. Banti (p.c., 19 April 2007), in turn, sees in the LECu. forms a prefix ma- ("*the form is like* mabla '*seeing*'" in Saho-Afar). D. Appleyard (p.c., 20 April 2007) shares the same view: "mamga *is certainly the more 'archaic' in so far as it is more transparently the nominal prefix* ma- + *the verbal root* -mg-, *i.e. PEC* *mig-/mug- *etc. 'be full' ... it seems to me quite reasonable to build a new 'root' on the basis of a nominal derivation* *ma-m[V]g-; *partial reduplication of the* $C_1VC_1VC_2$- *type seems less likely to me*". The Cu. stem was probably borrowed into Eth.-Sem.: Gafat mängä, Amh. mänga, Gurage-Soddo mänga "herd, flock" (ES: Leslau 1945, 163; 1979 III, 408–9; Appleyard 1977, 26/68 with less likely alternative Semitic etymologies). For reasons outlined here, the comparison of Cu.-Om. *mang- with CCh.: PHigi *muŋ- "10" [GT] (above) seems at the moment rather unlikely.

[93] Attested in Sem.: Akk. magāgu (also maqāqu) "(weg)spreizen" [AHW 574] ||| NOm.: Ometo *mēg- "col" [GT]: Wolayta & Dawro (Kullo) meg-uwa, Gofa & Gamu & Dorze mēg-o | Shinasha mēg-o (NOm.: Alm. 1993 MS, 8, #202b) ||| CCh. *√mg… "long (of stick)" [JS 1981, 169B_1]: Musgu masc. mógwa, fem. muguíí, pl. mogwáákai "lang, hoch" [Krause apud Müller 1886, 401] = mógoa [Rohlfs] = mogó "lang" [Overweg] = ana-mogó "it is big" [Rohlfs] = mogó "groß" [Roeder] = mugwi "hoch" [Décorse] = mógo "groß" [Lks.], Musgu-Pus mogo (m), mogwi (f), pl. mogokai "hoch" [MB 1972 MS, 4] = mogo (masc.), muguwiy (fem.) "long" [Trn. 1991, 106], Musgu-Girvidik mógó (m), mógwí (f), pl. mógwáy "hoch" [MB 1972 MS, 4] = mogo(m) "lang" [MB 1972–73, 70] (Musgu: Lukas 1937, 141; 1941, 68) || ECh.: Tumak māgón "nombreux", cf. māg "être capable, pouvoir, beaucoup" [Cpr. 1975, 81]. For the AA etymology see IS 1976, 41–42; HSED #1704. Cf. also SSem. *√mgn (root ext. *-n?) "very (much)" [GT]: Jibbali mɛ́kən "much, many, a lot of" [Jns. 1981, 170], Mehri maken [-k- < *-g-] "beaucoup, très" [Lsl.] = mɛ̄ken [Jahn] = mēkən "much, many, a lot of" [Jns. 1987, 264] || Amh. magan "très large" [Lsl.] = mägän "1. very large, unusually or strangely large (size), portentous, 3. type of long shield used by a fully-grown man" [Kane 1990, 343] (Sem.: Lsl. 1931–34, 35).

[94] Cf. **(1)** PCh. *g^wam- "10" [Nwm. 1977, 32] ~ WCh.: Angas-Sura *gam "to fill" [GT] (Angas-Sura data: Hfm. 1975 MS, 24, #215; Stl. 1972, 181; 1977, 154, #65; 1987, 217, #676; GT 2004, 121) | Bole-Tangale *(ŋ)gamu "to fill, be full" [Schuh 1984, 216] = *(n)-g^wam [GT] | NBauchi *g-m- "to gather, join, meet" [Skn. 1977, 23] (WCh. data: Stl. 1987, 217–8; JI 1994 II, 156) ||| Sem. *√gmm "völlig sein/machen" [GB] > Hbr. gam "zusamt, steigernd" [GB 143] | Ar. ğamma I "1. être riche, 2. être abondant, se remplir de nouveau d'eau, 3. être comble" etc., ğamm- "1. abondant, exubérant, 2. complet, 4. (mesure) comble" [BK I 321–2] (for further Sem. cognates see Hodge 1971, 42; Zbr. 1971, #58; MacDonald 1963–65, 75; WUS #664; Vycichl 1987, 114) ||| Eg. ngmgm (prefix n-) "sich versammeln" (XVIII., Derchain-Urtel 1973, 39–40 contra Wb II 349, 15) ||| HECu. *gumʔa "all" [Hds. 1989, 411] ||| NOm.: Oyda gāma "much, many" [Dlg. 1973, 78]. For the Ar.-WCh. comparison: Stl. 1987, 218; OS 1990, 80, #55; HSED #888. Or

same is to be observed about Afro-Asiatic "hundred",[95] "thousand",[96] "ten thousand",[97] and "hundred thousand".[98]

Summary

The results of the etymological analyses presented above lead us to the following table. Note that (+) in brackets signifies an existing, albeit indirect, correspondence of an Egyptian numeral, displaying some deviation in form. E.g., North Afro-Asiatic "two" (*√čn) is ultimately related to South Cushitic and Chadic "two" (*√čr), but only as ancient heteroclitic root varieties in Proto-Afro-Asiatic.

Eg.	Sem.	Brb.	Cu.	Om.	Ch.
√wʕ "1"	+	+?	-	-	-
√sn "2"	+	+	(+)	-	(+)
√ḫmt "3"	-	-	+	+	+
√fd "4"	-	-	-	+	+
√dj "5"	(+)	-	-	-	-
√srs "6"	+	+	-	(+)	(+)
√sfḫ "7"	+	+	+???	+	+
√ḫmn "8"	+	(+?)	(+)	(+)	-
√psḏ "9"	+	+?	-	-	+?
√mḏ "10"	-	-	(+)	(+)	+

cf. **(2)** Sem. *ʕaŝar- "10" [Dlg. 1986, 79, #14] ||| WCh.: Angas-Sura *ṣār "ten" [GT] (Angas-Sura data: Jng. 1965, 182; Hfm. 1975 MS, 20, #93; Stl. 1972, 182; 1977, 157, #188; JI 1994 II, 320; Takács 2004, 334–5) ||| Eg. ʕšʒ [< *ʕšr] "viel (sein)" (OK, Wb I 228, 8–26). For the Eg.-Sem.-Angas-Sura etymology: Trb. 1902, 199; Ember 1917, 88, #135; ESS §3.b.4; Alb. 1918, 92; 1931, 150; Vrg. 1945, 128, §1.c.8; Cohen 1947, #47; Hodge 1976, 15, #165; OS 1988, 82; Blv. 1989, 15; Mlt.-Stl. 1990, 65.

[95] Cf. NOm.: Kullo (Dawaro) tet-a "100" [CR 1913, 410] ||| Eg. twt "versammeln, versammelt sein" (PT, Wb V 259–260) ||| (?) WCh. *tVt- "to gather" [OS] (for the Eg.-PWCh. etymology see OS 1992, 195). Or cf. Sem. *√rbb "big" > Ebl. rib(b)a or ribab "10.000" [Brugnatelli 1984, 86–87; Gordon 1988, 261] || Ug. rbt, Hbr. rəbabā, Aram. ribbabtā "10.000" (Canaanite: Ember 1917, 87; WUS #2481).

[96] Cf. ECu. *kum- "1.000" [Sasse 1979, 12, 25; 1982, 120] || SCu. *kuma "1.000" [Ehret 1987, 30] ||| NOm. *kum- "1.000" [GT] ~ Eg. km "vollständig machen, vollenden" (MK, Wb V 128–130) ||| EBrb.: Siwa kôm, koma "tout, beaucoup" [Lst. 1931, 304] = "all, whole" [Mlt. 1991, 250] ||| LECu.: Baiso kamogani "much, many" [Ehret] ||| NOm.: POmeto *kum- "to be full" [GT] (NOm. data: LS 1997, 412).

[97] Cf. Sem. *√rbb "big" > Ebl. rib(b)a or ribab "10.000" [Brugnatelli 1984, 86–87; Gordon 1988, 261] || Ug. rbt | Hbr. rəbabā, Aram. ribbabtā "10.000" (Canaanite: Ember 1917, 87; WUS #2481). Or perhaps Eg. ḏbʕ "10.000" (I-, Wb V 365–366) ~ NOm.: She geba "many" [Flm.] || SOm.: Hamer & Karo gɛʔbi [Flm.: error for *gɛḅi?] "big" [Flm.] (Om.: Flm. 1976, 317) ||| ECh.: WDangla góóḅé "remplir un récipient (en l'immergent dans l'eau)" [Fédry 1971, 329]. As noted by W. Vycichl (1934, 80), the comparison of Eg. ḏbʕ with WCh.: Hausa dubu "1.000" (suggested by N. Skinner 1981, 187–8, #105 pace Barth) is excluded. For an alternative etymology of Eg. ḏbʕ see Takács 1997, 217, #9.

[98] Cf. Eg. ḥfn [< *ḥfl] "100.000" (I-, Wb III 74, 1) ~ Sem.: Ar. ḥafala I "reichlich vorhanden sein", V "sich in grosser Zahl versammeln", ḥafl- "Menge", ḥafīl- "zahlreich" [Vrg., Vcl.]. For Eg.-Ar. see Sethe 1916, 13–14; Ember 1917, 87, #135; ESS §9.a.7; Albright 1918, 93; Vergote 1945, 136, §9.b.26; Cohen 1947, #111; Vycichl 1958, 377; Loprieno 1986, 1310. For a different (less convincing) etymology of Eg. ḥfn see Holma 1919, 41; Hodge 1976, 12, #49; 1990, 370.

Conclusion

The first two, i.e., the most elementary and primary numerals, are evidently North Afro-Asiatic with no match in the southern block of the phylum, which clearly suggests an aboriginal northern affiliation of Egyptian just like the common North Afro-Asiatic apophony penetrating Semitic, Egyptian, and Berber morphology.

But the obvious South Afro-Asiatic nature of Egyptian "three" and "four" seems to testify to later renewed ties of Proto-Egyptian with the southern block, i.e., a secondary areal cohabitation, which agrees quite neatly with the lack of the prefix conjugation, an isogloss in the whole phylum shared by both Egyptian and Chadic grammar, which is paralleled by the undeniable domination of South Afro-Asiatic items in the overwhelming majority of Egyptian anatomical terminology, let alone the multitude of exclusively Egypto-Chadic lexical isoglosses.

Egyptian "five" must be an Egyptian innovation based on an extinct Eg. *jd "hand" = Sem. *yad- "hand" as a nisbe form, which was to render "5" only on the Egyptian side. This innovation was either very late having ousted Semito-Berber *√ḫms "5", or was simply much earlier than the latter. The former scenario seems more likely in the light of the separation of Egyptian from the Northern Afro-Asiatic block earlier than that of Semitic and Berber (cf. Takács 2015).

Once again the set of Egyptian numerals from "six" to "nine" comprises Semitic (and Berber) words (only "seven" seems to be sporadically attested in South Afro-Asiatic too), but, for some suspicious reason, all of them suffer from some fundamental phonological irregularity in Egyptian atypical of genetically inherited Egypto-Semitic cognates, cf. Eg. -r- vs. Sem. *-d- in "6", Eg. -fḫ vs. Sem. *-bʕ in "7", Eg. ḫ- vs. Sem. *ṯ- in "8", Eg. p-/-ḏ vs. Sem. *t-/*-ʕ in "9". Does this puzzle speak for a borrowed and not inherited nature of these higher numerals during a later secondary areal contact with Semitic, perhaps in the neolithic Nile valley (5th mill. BC?)?

Finally, Egyptian "ten" is a South Afro-Asiatic word exclusively attested in Chadic (although the underlying verbal root is Common Afro-Asiatic), which may indicate a common decimal system created (together with SAA "3" and "4") during the above mentioned secondary areal cohabitation of Proto-Egyptian and Chadic (or South Afro-Asiatic).

Abbreviations of languages

(A): Akhmimic, AA: Afro-Asiatic, Akk.: Akkadian, Ar.: Arabic, Aram.: Aramaic, (B): Bohairic, BD: Book of the Dead, Bed.: Bed'awye, Brb.: Berber, Ch.: Chadic, CCh.: Central Chadic, CT: coffin texts, Cu.: Cushitic, ECh.: East Chadic, ECu.: East Cushitic, E: East(ern), Eg.: Egyptian, EWlmt.: East Tawllemmet, (F): Fayyumic, GR: Greek (Ptolemaic) and Roman Period, GW: syllabic or group-writing, Hbr.: Hebrew, HECu.: Highland East Cushitic, IMP: Intermediate Period, JAram.: Jewish Aramaic, (L): Lycopolitan (or Subakhmimic), LECu.: Lowland East Cushitic, Lit.: literary texts, LP: Late Period, M: Middle, Mag.: magical texts, MK: Middle Kingdom, N: North, NBch.: North Bauchi, NBrb.: North Berber, NK: New Kingdom, NOm.: North Omotic, OEg.: Old Egyptian, OK: Old Kingdom, Om.: Omotic, OT: Old Testament, PB: post-Biblical, PCh.: Proto-Chadic, PCu.: Proto-Cushitic, PT: pyramid texts, S: South(ern), (S): Sahidic, SBrb.: South Berber, Sem.: Semitic, W: West(ern), WBrb.: West Berber, WCh.: West Chadic, WSem.: West Semitic.

Abbreviations of authors

Abr.: Abraham, Ajl.: Ajello, Alb.: Albright, Alm.: Alemayehu, Apl.: Appleyard, BA: Birru & Adal, BK: Biberstein & Kazimirsky, Blv.: Belova, Blz.: Blažek, Bmh.: Bomhard, Bnd.: Bender, Brg.: Bargery, Brk.: Brockelmann, Brt.: Barreteau, Cpr.: Caprile, CR: Conti Rossini, Crl.: Cerulli, Ctc.: Caïtucoli, Dbr.-Mnt.: Djibrine & Montgolfier, Djk.: D'jakonov, Dlg.: Dolgopol'skij, Dlt.: Dallet, Drnb.: Doornbos, Dst.: Destaing, Ehr.. Ehret, Fcd.: Foucauld, Fdr.:

Fédry, Flk.: Foulkes, Flm.: Fleming, Frj.: Frajzyngier, Frz.: Fronzaroli, Ftp.: Fitzpatrick, GB: Gesenius & Buhl, Gcl.: Gochal, Grb.: Greenberg, GT: Takács, Hds.: Hudson, Hfm.: Hoffmann, HL: Haberland & Lamberti, Hlw.: Hellwig, Hmb.: Homburger, HRV: Heine & Rottland & Voßen, Hyw.: Hayward, IS: Illič-Svityč, JA: Jungraithmayr & Adams, JI: Jungraithmayr & Ibriszimow, Jng.: Jungraithmayr, Jns.: Johnstone, Jst.: Justinard, KB: Koehler & Baumgartner, KM: Kießling & Mous, Kmr.: Kammerzell, Krf.: Kraft, Ksm.: Kossmann, Lks.: Lukas, Lmb.: Lamberti, Lnf.: Lanfry, LS: Lamberti & Sottile, Lsl.: Leslau, Lst.: Laoust, MB: Meyer-Bahlburg, Mch.: Mouchet, Mkr.: Mukarovsky, Mlt.: Militarev, Mnh.: Meinhof, MQK: Mous & Qorro & Kießling, Mrc.: Mercier, Mrn.: Moreno, MSkn.: M. Skinner, Mts.: Matsushita, Ncl.: Nicolas, Nct.: Nachtigal, Nhl.: Nehlil, NM: Newman & Ma, Ntg.: Netting, Nwm.: Newman, Old.: Ol'derogge, OS: Orel & Stolbova, PAM: Prasse, Alojaly, Mohamed, PB: Plazikowsky-Brauner, PG: Pillinger & Galboran, PH: Parker & Hayward, Pls.: Pilszczikowa, Prd.: Paradisi, Prs.: Prasse, RK: Reutt & Kogan, Rn.: Reinisch, Rns.: Renisio, Rpr.: Roper, Rsg.: Rossing, Rsl.: Rössler, Sbr.: Siebert, Scn.: Sachnine, Skn.: N. Skinner, Smz.: Shimizu, Snk.: Schenkel, Spg.: Spiegelberg, Srl.: Sirlinger, SSL: Simeone-Senelle & Lonnet, Stl.: Stolbova, Str.: Strümpell, Strm.: Stroomer, Sts.: Starostin, TC: Taïne-Cheikh, Tf.: Taïfi, Trb.: Trombetti, Trn.: Tourneux, TSL: Tourneux & Seignobos & Lafarge, Vcl.: Vycichl, Vrg.: Vergote, Wdk.: Wedekind, Wlf.: Wölfel, WP: Walde & Pokorny, Wst.: Westendorf, Wtl.: Whiteley, Zbr.: Zaborski, Zhl.: Zyhlarz, Zvd.: Zavadovskij.

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Габор Такач. Древнейшие лексические слои египетского языка VIII: числительные.

Статья продолжает серию публикаций автора, объединенных общей целью проанализировать древнейшие слои базисной лексики древнеегипетского языка, расклассифицированные по семантическим полям, и разделить их на «семитский» и «африканский» пласты, существование которых было предположено П. Лако несколько десятилетий тому назад. В данной статье анализу подвергаются числительные древнеегипетского языка.

Ключевые слова: древнеегипетский язык, афразийские (афроазиатские) языки, этимология, сравнительно-историческая фонетика, числительные.

Book Reviews / Рецензии

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А. И. Коган [Anton Kogan].
Проблемы сравнительно-исторического изучения языка кашмири [*The problems of comparative-historic study of Kashmiri language*].
Moscow: Fond razvitiya fundamental'nykh lingvisticheskikh issledovaniy, 2016. 208 pp. In Russian.

The monograph of the Russian linguist Anton I. Kogan "The problems of comparative-historic study of Kashmiri language" has become a long awaited treat for Indo-Iranian studies. Indeed, the question of attributing Kashmiri to either the Indic or the Dardic sub-branch has occupied the minds of researchers for many decades. Linguists of the past, such as G. Grierson and G. Morgenstierne, gave ambivalent answers to this question. Nor was the Kashmiri language well-studied from a diachronic perspective, and so the work of Anton Kogan fills in this important gap.

The first chapter of the book is dedicated to the problem of using the philological method for studying the history of Kashmiri. South Asia is a region with a variety of written and literary traditions, and the philological method is widely used in studying the history of Indo-Aryan languages. However, Kashmiri written tradition is only three hundred years old, and there are no reliable written sources for earlier periods. Some researchers considered the poetic inclusions in the Sanskrit philosophical work Mahānayaprakāśa and the phrase "Rangassa Helu diṇṇa" from a Sanskrit chronicle of 12th century Rājatarangiṇī as the earliest medieval examples of the Kashmiri language. However, the phrase from Rājatarangiṇī does not reveal any specific features which would distinguish its language from a literary Prakrit. Poetic inclusions in Mahānayaprakāśa offer more extensive and interesting material; nonetheless, detailed analysis reveals that it is not possible to establish a system of regular phonetic correspondences between this language and Old Indo-Aryan. The language of the inclusions looks like an artificial literary lect, created with the aid of traditional grammars and dictionaries of Prakrits and Apabhramsha. Such practice was widespread in Indian literary tradition — and it is quite probable that the scribes were speakers of Kashmiri; it is also possible to trace certain elements of Kashmiri influence in the language of Mahānayaprakāśa that can be explained as scribes' mistakes. However, Indo-Aryan historical phonetic development is much more characteristic of these texts in general. Therefore, we do not really know any texts in medieval Kashmiri, and the use of philological method for studying the language is rather limited.

In the second chapter the author studies a number of phonetic changes in Kashmiri and their dating. According to the data of internal reconstruction, regressive assimilation of vowels took place before the fall of final short *i*- and *u*-matra, but already after the period of massive Persian lexical influence. *Umlaut* in Kashmiri has developed as a result of regressive assimilation of vowels and deletion of final short vowels. In addition, this deletion of final short vowels has led to a new phonological opposition of palatalized and non-palatalized consonants. This opposition, distinguishing Kashmiri from most Dardic languages, is, therefore, a relatively late innovation.

Hesitation in attributing Kashmiri to Dardic languages is due to copious borrowings from Indo-Aryan languages, as well as a number of characteristic typological features that distinguish Kashmiri from most languages in the Dardic group. Thus, in Kashmiri, the opposition of affricates by place of articulation is two-fold (dentals and palatals), rather than three-fold (dentals, palatals, and retroflexives). This brings the system of Kashmiri consonantism closer to certain dialects of Western Pahari.

Based on external comparison, the author convincingly shows that this situation is the result of transition of retroflex consonants into palatals, and then, at a relatively later stage, of palatals into dentals. Morphophonological alternations and comparison with Shina and Phalura languages indicate that dental affricates existed in Kashmiri during the period preceding the transition. Therefore, until relatively recently Kashmiri must have had three rows of affricates.

Finally, the last section of the second chapter is dedicated to the shift of sibilants. In most cases,

Common Aryan *ś corresponds to Kashmiri *h*. The palatal sibilant *š* of Kashmiri corresponds to *ṣ* in most Dardic languages. The contemporary state of the systems of sibilants and affricates in Kashmiri can possibly be explained by the influence of Modern Indian languages, where retroflex consonants are neither affricates nor sibilants. It is typologically unlikely that the shift of sibilants preceded the shift of affricates, although we do not have a firm basis for relative dating.

Therefore, the main differences of Kashmiri phonology, compared to other Dardic languages, turn out to be a result of late changes.

In the third chapter, Kogan analyzes Indo-Aryan loanwords in Kashmiri. Their percentage in Kashmiri vocabulary is quite high, but definitively identifying these words in the absence of formal criteria is a difficult task. Easily identifiable strata are loanwords from Urdu, which became an official language in Kashmir in 1889, and Sanskritisms in Indian Kashmiri. Other borrowings from Indo-Aryan languages require more complicated analysis. The author proposes the following criteria to distinguish them:

1) a front vowel corresponding to Old Indo-Aryan *e* and common Iranian **ai* (the regular Kashmiri reflex is *a*);
2) laryngeal *h* corresponding to Old Indo-Aryan *h* and common Iranian **j* (the regular Kashmiri reflex is *z*);
3) *h* corresponding to Old Indo-Aryan **ṣ* (for borrowings that took place before the shift of sibilants);
4) *š* corresponding to Old Indo-Aryan **ś* (for borrowings that happened after the shift of sibilants);
5) *kh* corresponding to Old Indo-Aryan **kṣ*;
6) etymological parallels existing in Indo-Aryan, but absent in Dardic languages;
7) *d* and *t* corresponding to **rd* and **rt* (regular reflexes are *ḍ* and *ṭ*);
8) sequences of *ə̃* + voiceless consonants corresponding to sequences *a* + nasal + voiceless (the regular etymological reflex should be voiced).

Combination of features (4) and (8) in the same words allows us to suppose that the source language may have belonged to the Pahari group, where some languages have preserved the distinction of sibilants *s* and *ś*, and have also undergone fronting of **a* before consonant clusters of the "nasal + voiceless" type. Additionally, a number of semantic and morphological isoglosses that unite Kashmiri with Indo-Aryan languages could be the result of Indo-Aryan influence. The author supposes that Kashmiri was likely influenced by an Indo-Aryan substrate language that was common in the Kashmir valley before becoming assimilated by the Dardic-speaking population.

The fourth chapter establishes the genealogy of Kashmiri dialects. The Siraji and Rambani dialects, which Grierson considered as mixed, can be attributed to Indo-Aryan based on a number of features. Thus, Proto-Aryan short *ai* is reflected as *i* and *ĕ* in these dialects. Besides, they have voiced aspirates which usually correspond to voiced aspirates in Indo-Aryan languages. The distinction of dental and palatal affricates in Siraji and Rambani is not an exclusively Dardic feature; it is also characteristic of certain Pahari languages. As to the morphological and lexical features that Siraji and Rambani have in common with Kashmiri (pronominal suffixes, the stem of the copula etc.), this also does not seem a sufficiently solid basis for classification. The author makes his final decision upon conducting lexicostatistical analysis based on Swadesh's 100-item wordlists. The mean percentage of matches between Siraji and Indo-Aryan languages is 68,6%, between Siraji and Dardic languages — 50,6%, which allows to classify Siraji (and the closely related Rambani) as an Indo-Aryan language.

On the other hand, the Poguli and Kashtavari dialects should, most probably, be attributed to the Dardic group. Thus, voiced aspirated consonants are found mostly in Indo-Aryan borrowings. Some cases of development **ś* > *h* are explained individually by the author. It is surprising that the author does not apply lexicostatistic analysis to this pair of dialects as well, but dives instead into the explanation of examples that contradict his hypothesis by means of analogies, metatheses, contaminations, etc. This leaves an impression of asymmetric composition and somewhat inarticulate evidence. Another strange peculiarity is the urge to prove the originality of basic vocabulary even in those cases where the phonetic form of the word clearly indicates a borrowing (pp. 67, 127). Apparently, though, these details do not affect the author's final conclusions.

In the fifth chapter the question of Eastern Dardic linguistic unity, as identified by G. Grierson, is researched. The author criticizes the historical-phonetic innovations, proposed by G. Buddruss, which supposedly unite the Eastern Dardic languages (the shifts **w* > *b*, **st* > *t(h)* and **ṣṭ* > *ṭ(h)*), noting that the first two cannot be considered common for Kashmiri, Phalura, and Shina languages. Lexical isoglosses identifying Eastern Dardic among other Dardic languages unite them with Indo-Aryan languages. This certainly raises suspicions that the areal cohesion of different Dardic languages could be a consequence of common Indo-Aryan influence.

The author's own criteria are as follows: (a) front shift *śr* > *ṣ*; (b) voicing of voiceless consonants; (c) deletion of voiced consonants after a nasal; (d) the fate of the Proto-Indo-European cluster **ḱṣ*, which is reflected in Eastern Dardic languages as the palatal affricate *čh*, and its later developments in the original intervocalic position before a short vowel in the last syllable.

In the concluding section of the fifth chapter a lexicostatistical research is conducted, utilizing such methods as "nearest neighbors" and "least mean deviation». In both cases lexicostatistics confirms the fact of close genetic affinity between Eastern Dardic languages (Kashmiri, Shina, and the languages of Kohistan). Languages of Kohistan share a high percentage of common vocabulary with Kashmiri and Shina. At the same time, the fraction of correspondences between the Kashmiri and Shina lists is rather small. These facts allow us to suppose migration of the speakers of medieval Kashmiri from the Swat river valley to their modern habitat.

Overall, the monograph is a fascinating piece of historical research on the Kashmiri language, distinguished by the variety of methods employed by the author. Several shortcomings in the book's design slightly hinder the ease of comprehension: for example, only in the fourth chapter do etymological examples begin to be regularly separated from each other by paragraph marks. However, this technical glitch should not detract the reader from the substantial merits of the book.