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ABSTRACT

There is a new theory that Burushaski is related to Kartvelian, put forward by Holst (2017). Čašule (2017), published in this journal, made various statements about this new theory. Čašule says that he is not convinced by it and seeks to explain why he is not. Unfortunately, his assessment contains a number of misunderstandings, statements which do not entirely match the facts of Holst (2017), and other features which can be regarded as problematic. In addition, there are many issues on which divided opinions are possible. Given this, the present paper is intended to react to Čašule (2017). While doing this, new issues come into the debate.

KEYWORDS

Burushaski, Kartvelian, language relationship

On some recent claims on Burushaski

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1 Background and goal of the present paper

Burushaski is spoken by about 100,000 people in the very north of Pakistan in the Karakorum mountains, an offshoot of the Himalayas. It is a typologically and historically rather interesting and idiosyncratic language. The dialects are Yasin, Hunza and Nager; the latter two are rather close to one another. Conservative classifications count Burushaski as a language isolate, i.e. they deny that any relationship to other languages has been demonstrated. However, there have already been many attempts to relate Burushaski to other languages. Three theories are particularly prominent today. The following survey proceeds according to when these theories were first voiced (A = oldest theory of the three mentioned).

A. The *Dene-Caucasian* theory claims that Burushaski is related to Basque, Abkhazo-Adyghean and Nakh-Daghestanian (two families in the northern Caucasus), Yenisseian, Sino-Tibetan, Na-Dene (in North America) and possibly other languages. The most prominent linguists sharing this view are John D. Bengtson, Václav Blažek, Merritt Ruhlen and others. This view has been expressed in various publications such as e.g. Bengtson (2008), Bengtson (2017), in fact too many to be listed here.

B. The *Indo-European* theory claims that Burushaski is an Indo-European language or at least related to Indo-European. Its originator is Ilija Čašule (see e.g. Čašule 2012), and he has already defended this theory in a large number of publications as well, again too many to be listed here.

C. Another theory seeks to relate Burushaski to *Kartvelian*. This is a small family in the southern Caucasus consisting of four languages: Georgian, Mingrelian, Laz and Svan. This is the youngest of the three views mentioned here. Its originator is Jan Henrik Holst, and the most relevant publication is the 420-page book Holst (2017), written in German, but see also the brief English summary in Holst (2020: 104–107). Already before that, there appeared Holst (2014a), which is an in-depth synchronic and diachronic investigation of Burushaski in its own right. It is also necessary to mention in this context the work on the Kartvelian languages by Holst: most notably the monograph Holst (2014b), which has much to say on reconstruction within Kartvelian (updating it slightly), but also the papers Holst (2011) and Holst (2016), for instance.

The scholars subscribing to each theory work to substantiate that particular theory. This is of course a rather natural state of affairs in science. What can be observed is that the three theories rest on completely different evidence and argumentations. Thus, each "camp" claims etymologies (of lexical material and of morphemes) with those languages that are regarded as related within the framework in question. The three theories are also mutually exclusive. Not all of them can be correct, but at most one can. This is not to say that the researchers can never profit from each other; occasionally they do, in particular in details.

The scholars subscribing to each theory also sometimes lay out why they are not convinced by the other theories and the intellectual edifices behind them, attempting to point out weaknesses and issues they feel should be seen in a different light. This, too, is certainly welcome if scholarship is regarded as a collective enterprise searching for the truth. Such assessments can of course in turn be the target of criticism.

Recently, Čašule (2017: 3f.) has commented on Holst (2017). However, in my opinion his comments may be misleading for readers with no or little background on the languages in question and on the debate. The goal of the present paper is therefore to react to Čašule (2017).

The main purpose of Čašule (2017) is to claim certain etymologies concerning Burushaski and Slavic. This part of Čašule's work will not be commented on in the present paper. The focus will exclusively be on what Čašule (2017) says about Holst (2017), and on new insights which arise while discussing this or which I can add with this opportunity. For general rejections of Čašule's Indo-European theory see Tiffou (2004: 97f.), Bengtson & Blažek (2011), Bengtson & Blažek (2012), Holst (2014a: 16f.) and Holst (2017: 33–36).

In the following, I go through Čašule (2017: 3f.) paragraph by paragraph, react to his remarks and add any observation I can that may help advance the controversial discussion on the relationship of Burushaski.

2 Čašule's introductory statements

Čašule (2017: 3) says that he will "provide some preliminary comments only on his [= Holst's] etymologies (pp. 105-179)." This is a pity since the evidence in Holst (2017) consists of several parts, and the cognate sets (etymologies) pp. 105–178 (the chapter ends on p. 178, not 179) are only one such part. There is also a very regular historical phonology (pp. 179–242), an extensive historical morphology (pp. 243–346), a brief historical syntax (pp. 347–359), and miscellaneous other research results (pp. 360–382). Moreover, these parts are interconnected. However, of course Čašule is free to choose to comment on only part of Holst (2017).

The chapter "Etymologien" (Holst 2017: 105–178) contains 110 cognate sets. Each treatment is structured in the following way. First, the attested data from Kartvelian are presented, then almost always a Proto-Kartvelian reconstruction, then the Burushaski data and almost always a Proto-Burushaski reconstruction. Each time, a discussion follows. Finally, a so-called "memo" is presented, e.g. this one for the first etymology, 'ox' (Holst 2017: 107):

| Ka. * | *q* | *a* | *r-* | 'ox' |
| Bur. * | *h* | *a* | *r* | 'ox' |

A "memo" is thus a kind of a short presentation of the cognate set in tabular form, usually with the two reconstructions, and always with the corresponding sounds in the same slot (e.g. Proto-Kartvelian *q matches Proto-Burushaski *h).

After his statements at the beginning, Čašule (2017: 3) claims about Holst (2017):

"His comparison is generally on weak grounds because it puts forward only 110 etymologies (only 18 of which are verbs and only 8 adjectives) with few coherent semantic fields and too few items belonging to basic vocabulary."

There are several assertions involved here, which must be assessed one by one.

First, 110 cognate sets is not a small number, but a fairly large one – if these are genuine cognate sets and not only lexemes vaguely similar to each other by chance (an issue which a consistent historical phonology and other background information will reveal). It is true that Čašule meanwhile has over five hundred "etymologies" for his Indo-European theory (Čašule 2017: 1). However, his evidence is highly problematic with its segmentations, its semantics, its references to the very many Indo-European languages which provide much material for each meaning (often ten or more words to pick from), and in many other respects, as critics have pointed out repeatedly. Also the Dene-Caucasian theory has several hundred "etymologies" by now, although very many lack Burushaski. Trask (1995: 194), also author of a textbook on historical linguistics, explains the matter as follows:

"If those comparisons are deeply flawed and thoroughly unconvincing, their number is immaterial. A dozen highly persuasive comparisons would mean a lot; 300 bad ones means nothing."

Fleming (1999: 30 fn. 3) refers to a similar statement by another respected scholar (the book mentioned in the quotation is Greenberg 1987):

"Ives Goddard, a well-known critic of Greenberg's Amerind book, said at the famous Stanford conference in 1987 that he would believe any two or more languages were related if he or someone else could find thirty-five good cognate sets."

Although Trask and Goddard are known to be two scholars rather conservative in matters of language classification, here they refer to surprisingly low figures, 12 and 35 sets respectively. However, note that they speak of "highly persuasive" and "good" sets, which is decisive. Probably all linguists will agree that time makes the number of cognate sets smaller and smaller over time. At a certain point of time only 150 related items, for instance, will be left, of which a competent linguist may be able to discover most items, but not all. With a certain time depth, the wish to find many hundreds of etymologies rests on mistaken expectations; they are no longer there (due to language change) and cannot be found.

How then can evidence be probative with 110 or even fewer items? This can work if these are very well represented among the most basic of all items such as 'I', 'eye', 'ear', 'water', etc. There is research by Swadesh, Dolgopolsky and many others on which these most "stable" items are. To a great extent such items are represented in Holst (2017). The statement by Čašule (2017: 3) that "too few items belonging to basic vocabulary" are named is simply false. There's a lot of what one wishes for: body parts, animals, pronouns, basic activities, etc. Moreover, the discussion about numbers of etymologies not only distracts from their quality, but also from another salient point, the regularity of the historical phonology (more on this below in section 5).

Čašule (2017: 3) speaks of "few coherent semantic fields". It is not entirely clear what he is referring to, but it should be kept in mind that at a higher time-depth you cannot always achieve a "semantic field". English *foot* and Latin *pēs* are from the same Proto-Indo-European root, but English *hand* and Latin *manus* are not. This makes the "semantic field" incomplete, but English and Latin are related languages nevertheless.

The statement by Čašule that there are only eight cognate sets with adjectives is correct. However, the background of this fact is discussed and clarified by Holst (2017: 154, 278f.), and Čašule remains silent about this discussion. It is a finding of typology that there are languages without adjectives, and also languages with only few adjectives (Dixon 1982). Proto-Kartvelian originated from a language with few adjectives, as explained by Holst (2017: 278f.) with recourse to

Klimov (1991: 326) and other literature. Related to this fact, it is also only possible to find few cognates with Burushaski among adjectives.

Furthermore, Čašule (2017: 3) is not satisfied with "only 18" verbs. In Holst (2017), the etymologies are arranged according to part of speech (or "category", as Chomskyists would say), and a quick check reveals that the number 18 is not correct. The section "Verben" (pp. 162–177) has 25 cognate sets. Of these, four are cognate sets with nouns. Three of them refer to animals able to fly which are derivatives of 'to fly' (pp. 166f.), e.g. this set:

Ka. *		*p*	*r*	*i*	*n-*		'bird'
Bur. *		*c*		*i*	*n*		'bird'

The fourth set with nouns is a derivative of a different verb, semantically approximately 'to sing, to cry' (p. 164). My guideline was to place derived nouns after the verbs they belong with, hence inevitably they entered the section on verbs due to this procedure. Subtracting the four nouns, this yields 21 verb etymologies. It is true that also other parts of speech are occasionally represented in their discussions, which, however, is a usual state of affairs. And there is probably nothing wrong in comparing, for instance, 'drop' (noun) in one language with 'to drop' (verb) in another language, as done by Holst (2017: 173):

Ka. *		*c*	*w*	*a*	*r-*		'drop' (noun)
Bur. *		*c*		*a*	*r-*		'to drop' (verb)

Note also the entirely regular four correspondences in this cognate set; clusters of consonant + **w* lose **w* in Burushaski (Holst 2017: 232f.).

It has thus been possible to refute all of Čašule's charges in the quotation above. Čašule (2017: 3) goes on to say: "The small number of postulated correspondences makes it very difficult to establish viable phonological correlations." However, there is a long chapter explaining the historical phonology (pp. 179–242). This topic will be discussed in section 5 below.

3 Loanwords and alleged loanwords

In an argumentation for language relationship, loanwords must not erroneously be used in putative cognate sets; there is consensus about this methodological point (e.g. Campbell & Poser 2008: 194). However, it is not always clear what is a loanword and what is not. In addition, when charging a scholar of having used a loanword in a cognate set, one must make sure that this reproach is correct, i.e. that the status as loanword is certain or very likely.

Čašule (2017: 3) expresses disbelief in six cognate sets out of the 110 of Holst (2017: 105–178) since according to Čašule the Burushaski words employed in them are loanwords. First, two of them are misquoted by Čašule. The word for 'sibling of the same sex' is *-ço* with a dot under the consonant symbol (since it is a retroflex) and not over it; with the dot over it you would get Berger's notation of the dental affricate *c*, IPA [t͡s]. (Nouns with obligatory possessive prefixes, or "inalienable nouns", such as *-ço*, are cited with a hyphen at the beginning in Burushaski studies.) Moreover, *par ét-* 'to fly' does not have an accent on *par*. Another item, Hunza *phuṣ*, is translated as 'free' by Čašule, while 'empty' would be the basic and most important meaning, see Berger (1998: 337) (German 'leer') and Tiffou (2014: 251) (French 'vide').

The main point to be made here, however, is that almost all of the six Burushaski words in question are clearly no loanwords at all and can therefore be used freely for comparisons with Kartvelian.

Čašule claims that -*ço* 'sibling of the same sex' stems from Tibetan, following Parkin (1987), but it had already been pointed out by Holst (2014a: 19) that the brief paper by Parkin can often not be trusted. The alleged etymology rests on only two similar sounds, and the retroflex of Burushaski remains unexplained. Moreover, it would have been good to cite the Classical Tibetan form involved in the statement. Additionally, inalienable nouns in Burushaski are virtually always old in the language; loanwords usually do not enter this category.

Regarding four words, Čašule claims that they are "marked as Indo-Aryan by Berger". In fact, they are not. All Berger does is to indicate that a similar word exists in Indo-Aryan. This does not imply that there must have been a borrowing from Indo-Aryan to Burushaski. In most cases indeed the direction is so, but also the reverse process is possible, i.e. borrowing from Burushaski (or possibly an extinct relative, once geographically close) to an Indo-Aryan language or several. This applies especially when a word is only present in Shina, and not elsewhere in Indo-Aryan. It is known that Shina (a Dardic language) underwent heavy influence from Burushaski and took up a great amount of loanwords from it. Also Khowar (another Dardic language) was influenced by Burushaski to a great extent. Given this, Berger (1974: 128) and Berger (1998: 7) warned explicitly of interpreting his references to Indo-Aryan lexemes in such a way that Indo-Aryan is always the donor language. Holst (2017: 372) reiterates this warning. Possibly Čašule misunderstood Berger who says, as laid out, that partly he only notes connections without claiming a direction for the borrowing in each case.

Moreover, not too rarely are Berger's references to Indo-Aryan highly doubtful or plainly incorrect. This is an important point to know about Berger's Burushaski work, and it is the point that is even more relevant to Čašule's charge that Holst (2017) allegedly employs some loanwords. Already Holst (2014a: 19f.) explains: "Berger (2008) interprets too many Burushaski words as loanwords from Sanskrit and other Indo-Aryan sources, often with far-reaching phonetic and semantic changes." The quotation does not only apply to Berger (2008), but also to other publications by Berger. Many of Berger's connections are indeed very far-fetched as to their semantics or the phonetic changes they imply (or even both), and hence incorrect in many cases. It is necessary that the linguist checks Berger's alleged etymological connections, instead of blindly trusting him. Berger often cites Turner (1966) (indicating this with "T" + number of Turner's etymology). When working on Holst (2017), I checked Berger's claims of borrowing, if any, with Turner (1966), and only if his Indo-Aryan origin was unconvincing did I allow myself to possibly use the Burushaski word for my work. Unfortunately I do not mention Turner (1966) anywhere in Holst (2017), but I did consult it, and I cite it in Holst (2014a). Unless Čašule, or anyone else, can demonstrate in more detail why one should believe in the rather doubtful Indo-Aryan origin of the four Burushaski words in question, I will continue to regard them as independent, and as free for comparisons to the outside (with Kartvelian).

There is only one etymology for which indeed Čašule possibly is making a point which may be relevant. Holst (2017: 122) connects Yasin *baxtá* 'sheep' / Hunza *baqtá* 'type of big sheep' with Georgian *bat'k'-an-i* 'lamb' and other Kartvelian data. Čašule brings a Persian word *bakhta* into the debate, the meaning of which he gives as 'a ram, a fat tail', referring to Steingass (1892: 159). Indeed in this source you find a word *bakhta* with the translation "A ram three or four years old; anything the skin of which has been taken off; a fat tail; a collector (of revenue)." A native speaker of Persian,

Shervin Taheri-Kutanaee, did not know the word, but he kindly searched for it for me and found it in an additional source. Therefore there may arise the necessity to remove this etymology, which would leave 109 rather than 110 etymologies. Or, alternatively, one of the etymologies I did not take up in Holst (2017) could possibly take its place. First, however, further research would have to clarify into which direction the word was borrowed, and other particulars.

Given what has been explained in this section, I stick to most of the cognate sets which Čašule has addressed in his paragraph. Let us have a look at one of these sets:

Ka. *	*p*	*a*	*t'*	*r-*		'empty'
Bur. *	*ph*	*u*	*s*			'empty'

As in most cognate sets in Holst (2017), the correspondences are entirely regular, as we shall briefly discuss. (The two words are not very similar to each other, but this is not the point when arguing for a relationship, since the regularity counts.)

Position 1: The correspondence *p* / *ph* occurs in five sets (including 'empty' itself) (Holst 2017: 196f.). It must furthermore be seen in the context of the "plosive shift" ("Plosivverschiebung") which also accounts for *t* / *th* etc. as well as *p'* / *p*, *t'* / *t* etc. A former system of non-ejective / ejective, which was preserved in Kartvelian, turned into aspirated / unaspirated in Burushaski. The plosive shift has almost 30 examples (Holst 2017: 196–203, Holst 2020: 105f.) and is therefore one of the most secure Burushaski sound laws ever discovered.

Position 2: The vowel correspondence *a* / *u* occurs in 14 cognate sets (including 'empty') and two morphemes, hence it is clearly regular (Holst 2017: 181f.).

Positions 3 / 4: Clusters of consonant + *r* were preserved in Kartvelian, but were fused into retroflexes in Burushaski. To be more concrete: **tr* > *ṣ*, **t'r* > *ṣ* and **pr* > *c̣* (Holst 2017: 220–224, Holst 2020: 105). Compare the set 'bird' mentioned in section 2 above. There are four examples listed by Holst (2017: 221) (including 'empty' and 'bird'), one example which Berger (2008: 79) had discovered earlier on the basis of internal evidence from Burushaski, and probably another example (which had an affricate) discussed by Holst (2017: 221). This results in six examples. The neighbouring Dardic language Shina, a contact language of Burushaski, has similar shifts of consonant + *r* to retroflex (Masica 1991: 210), and there is of course an areal connection between Shina and Burushaski with regard to these shifts (cf. Berger 2008: 18, Holst 2017: 220f., Holst 2020: 105).

4 Semantic deviations

Historical linguists agree that from a general perspective, semantic deviations in proposed cognate sets can be a problem (e.g. Campbell & Poser 2008: 195). On the other hand, the phenomenon of semantic shift exists, of course, and it is therefore perfectly possible that lexemes with different meanings are etymologically identical. The judgement of a particular case must consider the closer circumstances.

Čašule (2017: 4) claims about Holst (2017: 105–178): "There is often too much semantic latitude". He then gives 15 examples for what he means. It seems to me that he tried to pick out approximately those cognate sets (of the 110) which have the most conspicuous semantic deviations. But unfortunately, all semantic discussions in Holst (2017: 105–178), some of which are very profound and of course relevant, are completely disregarded and left unmentioned by Čašule.

Moreover, some data are misrepresented by Čašule. Holst (2017) does not really compare Ka. 'to drink' with Bur. 'to eat', but with a Bur. word which can mean both 'to eat' and 'to drink' – which the text by Holst (2017: 174f.) makes clear.

In order to have a more objective approach, let us have a closer look at the handling of semantics in Holst (2017). For this purpose the "Etymologienindex" by Holst (2017: 413–416) is useful. It contains all 110 cognate sets arranged by the meanings in the "memos" (with references to the body of the book). In 53 instances, there is only one meaning, that is, the meanings compared are identical, e.g. 'eagle' is compared to 'eagle'. In contrast, 57 entries have two semantics separated by a slash, e.g. 'eye' / 'eyelid'. However, even the slightest deviations among the translations are referenced here (mechanically), e.g.:

- 'he, she' / 'she'
- 'to chase' (verb) / 'chase' (noun)
- 'to shine, light' / 'light'
- 'dirt' / 'dirty'
- 'sister' / 'sister of a male person'
- 'drop' (noun) / 'to drop' (verb)
- 'woods, grass' / 'grass'

Deviations such as these cannot seriously be a problem. In other cases, the semantic deviations are so commonplace that they probably should not cause worry:

- 'mountain' / 'top'
- 'elbow' / 'knee'
- 'he' / 'that'
- 'insect' / 'bug'

To be sure, in some of the remaining cases the semantic deviations look more daring. There are somewhere between 10 and 15 such cognate sets (depending on what one counts as "more daring"), and Čašule apparently attempted to cite some sets of this type, as said above. But precisely here it is highly important to study the treatment of each etymology carefully. The explanations given in Holst (2017) should usually make clear why the first impression that the semantics involved are odd is probably erroneous or exaggerated. Moreover, in some of these cases it turns out that one side (Ka. or Bur.) has a derivative suffix or other feature which helps understand the semantic deviation. Regarding 'to know' / 'to see' (Holst 2017: 176), it turns out that Kartvelian 'to know' arose as 'to have seen', which can still be gathered from certain syntactic facts that are synchronically mysterious; the argumentation takes recourse to typological parallels in Indo-European and Dyirbal and is a highlight of the chapter on syntax (Holst 2017: 358f.).

There is another aspect which must not be forgotten: the relation to the historical phonology. If the material that is assumed to be cognate exhibits great regularity, a putative etymology can very well be correct. For example:

Ka. *	z	i	s	x-		'blood'
Bur. *	s	i	s			'people'

One has to admit that all positions match here; voiced sibilants become voiceless in Burushaski (here: *z), and the loss of *x in Burushaski is regular as well. What I have in mind are established cases in historical linguistics such as the following one from the Baltic branch of Indo-European. Latvian

valoda 'language' and Lithuanian *valandà* 'hour' match position for position and are in fact real cognates, even if the semantic deviation seems strange at first (this noun stems from a verb which meant 'to roll', 'to evolve' and similarly).

Čašule (2017: 4) maintains that I should not have posited the following etymology (Holst 2017: 127f.):

| Ka. * | t | x | a | m- | 'head' |
| Bur. * | th | | a | m | 'king' |

The Kartvelian reconstruction rests on Georgian *txem-i* 'head, top' (inadvertently written with another hyphen *txem-i-* by Čašule), among other data. Incidentally, this is one of the etymologies that other researchers had previously claimed before me, e.g. Bleichsteiner (1930: 325). Čašule criticizes the semantic deviation as "strained". However, very frequently words for 'head' are used in a figurative sense for a person who has some sort of leading role, as the discussion by Holst (2017: 127f.) points out. French *chef* < Latin *caput* 'head' provides a well-known example (as well as English *chief*, loanword from French). Compare also English *head* for the headmaster of a school (and other meanings). Holst (2017: 127f.) adduces more examples and also refers to Campbell & Poser (2008: 166), where 'head' > 'leader' (and similar senses) is discussed and exemplified as well. Even in Kartvelian itself there exists Svan *ma-txwm-i* 'leader', which is a derivative of the word in question (Fähnrich & Sardshweladse 1995: 172). Čašule cites some words from Burushaski which are allegedly from the same root as *tham* 'king', such as *thaŋá* 'success, good reputation', but he does not explain why they should be interrelated, and there seem to be no sufficient reasons for this assumption.

The last issue to discuss will take most space here. Čašule (2017: 4) claims that Holst (2017: 109) takes "recourse to a Kartvelian word for 'pillow' to compare it with Bur [Burushaski] 'ear'". This is not quite what happens. In reality, the Kartvelian word for 'ear' is compared with the Burushaski word for 'ear', which is semantically impeccable, of course. Incidentally, Bleichsteiner (1930: 325) and Fähnrich (2003: 71) have this etymology, too – in a nutshell. What Čašule is trying to refer to is that the reconstruction of the Kartvelian word for 'ear' rests on the Svan word for 'ear' and a word for 'pillow' in the Georgian-Zan branch of the family. For treatments of this fact see Fähnrich & Sardshweladse (1995: 315), Holst (2014b: 163) and Holst (2017: 109), among other sources.

I will give another treatment here, emphasizing a few facts and adding some new issues. This is the family tree of the Kartvelian languages, plus what their words for 'ear' are:

Kartvelian
Georgian-Zan
Zan
Svan *šdim*
Mingrelian *ʔuǯi*
Laz *q'uǯi*
Georgian *q'uri*
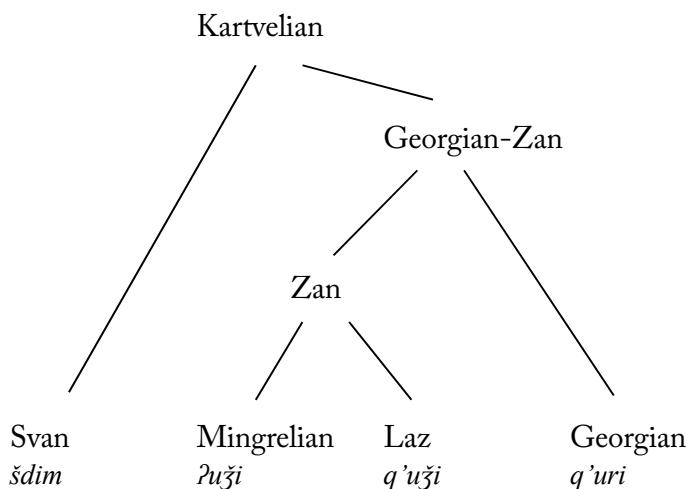
As can be seen, Svan *šdim* has no etymological relation to the other three words. Obviously, two different etyma are involved here. The data from the Georgian-Zan languages stem from Proto-Georgian-Zan **q'uri* 'ear' (in Zan, **r > ʒ* before *i*).

The question arises which branch of the family continues the original word for 'ear': Svan or Georgian-Zan. It is known in Kartvelology that Georgian-Zan **q'uri* 'ear' was derived from a verb meaning 'to perceive', thus the ears were called "perceivers"; the verb is present in Georgian *q'ur-* 'to look at, to watch'. It is the word continued as Svan *šdim* which was the original word for 'ear'. What underlines this is the fact that it survives in the word for 'pillow' in the Georgian-Zan branch: Georgian *sa-stumal-i*, Mingrelian *o-rtumel-i* (with a well-known derivational prefix *sa- / o- < *Ka. **ta-*, and nominative suffix *-i*). This insight leads to the reconstruction Proto-Kartvelian **λumal-* 'ear'. For the reconstruction of the lateral affricate **λ*, which supplements the ejective **λ̓* of earlier research in the consonant system, see Holst (2014b: 159–164) and Holst (2017: 71f.). The paradigm had "syncope", i.e. **λuml-* in certain forms (among them all plural forms), and it is this allomorph which underlies Svan *šdim* (with regular *šd < **λ*, umlaut *i < **u*, and loss of **l* via **w* after a labial; there are parallels for this, Holst 2014b: 163).

An interesting point, which has been discussed by Kartvelologists, is whether words for 'pillow' can derive from 'ear'. They found a typological parallel for this in the Slavic branch of Indo-European, e.g. Polish *pod-usz-k-a* 'pillow' from *uch-o* 'ear', pl. *usz-y* 'ears' (*pod* 'under'; *sz / ch* is a well-understood morphophonological alternation); the Russian data are virtually identical (see Fähnrich & Sardshweladse 1995: 315). A pillow is what you put your ear on when wanting to sleep. In Holst (2014b: 163) and Holst (2017: 109) I added Italian *guanciale* 'pillow' from *guancia* 'cheek', since you can also express it in such a way that you rest on your cheek. After having written Holst (2017: 109), I became aware of three additional typological parallels, from three Germanic languages, in which words for 'pillow' contain 'ear':

- Dutch *oorkussen* 'pillow', cf. *oor* 'ear'
- Swedish *örngott* 'pillow' (outdated), cf. *öra* 'ear', *öron* (pl.)
- Old High German *ôrwengi* 'pillow', cf. *ôra* 'ear'

After having communicated these new parallels to Čašule (by e-mail, March 18th, 2018), he kindly drew my attention to still another example (in an e-mail to me from April 30th, 2018): French *oreiller* 'pillow' from *oreille* 'ear'. I thank him very much for this – all the more so since it helps my case and it was altruistic of him to point this out to me. Interestingly, Čašule (2018: 36) now uses the derivation of French *oreiller* 'pillow' from *oreille* 'ear', as well as the Slavic etymology, for his purposes himself.

To sum up, the Kartvelian word for 'ear' was **λumal-*. The Burushaski word for 'ear', astoundingly, is *-ltúmal* (in all dialects and hence also in Proto-Burushaski). This permits the following comparison (Holst 2017: 109):

Ka. *	λ	u	m	a	l-		'ear'
Bur. *	-lt	ú	m	a	l		'ear'

This cognate set has five "positions" or "slots", all regular, and it is hence very probative. I hardly know any other language in the world with a word for 'ear' with approximately this structure. As to the first position, it had even been known that Burushaski **lt* can be from a lateral affricate **λ* (Bengtson 2008: 246, Bengtson & Blažek 2011: 41f., Holst 2014a: 143–145, Holst 2017: 225).

5 Phonological correspondences

There is general agreement that argumentations for relationship should have a historical phonology that is regular and realistic (e.g. Campbell & Poser 2008: 172–176).

Čašule attempts to criticize some aspects of the historical phonology of Holst (2017), but I would like to show that his assessment is unfounded.

Čašule (2017: 4) criticizes that "in several instances we find single consonant monophonemic or biphonemic correlations in the comparisons which increases significantly the possibility of chance." He thus refers to the problem of so-called "short forms", as Campbell & Poser (2008: 200) call it; these authors explain: "the greater the number of matched segments in a proposed cognate set, the less likely it is that accident may account for the similarity". However, the comparison of short forms can of course also be correct: English *to* and German *zu*, English *I* and German *ich*, English *in* and German *in*, and many others, are cognates, and it is perfectly legitimate to point this out. What Campbell & Poser (2008: 200) intend to do is to warn of *an exclusive use* or *a too high percentage* of short forms, since this increases the possibility that the entire intellectual edifice is mistaken. They would never deny, however, that also short forms can be cognates *if the overall context of the evidence speaks for that*. What also plays a role is whether the historical phonology warrants the connection. The correspondence *t / z* in English *to* / German *zu*, for instance, is regular.

I evaluated all 110 etymologies of Holst (2017: 105–178) and noted how many positions are compared in the "memos", the small summaries at the end of each etymological treatment. This results in the following table:

no. of positions	no. of etymologies
1	2
2	9
3	48
4	34
5	12
6	5
total:	110

Thus, for instance, the number of cognate sets which compare three positions between Kartvelian and Burushaski is 48. An example would be Ka. *qar- 'ox' / Bur. *har 'ox' (Holst 2017: 107). As can be gathered from the table, the number of "monophonemic or biphonemic correlations in the comparisons" (Čašule's wording) is 2 + 9 = 11. This is 10% of 110. The remaining 99 etymologies, or 90%, compare three, four, five or six positions between Kartvelian and Burushaski. These are fairly good figures. It would not be a clever idea to omit the shorter items just because they are short.

Moreover, the example that Čašule attempts to give for "short forms" is not a good one in this context. Concerning Holst (2017: 175) (not pp. 175–176, as he writes), Čašule (2017: 4) refers to Ka. **-γ*- 'to take' and Bur. **-γún*- 'to give' and calls this "a single consonant monophonemic correspondence with a semantic equation which could be disputed, even if it is found in Indo-European". He adds that a causative suffix *-ún*- is assumed in the Burushaski form, but that this assumption allegedly lacks "any real foundation". This is again an instance where his statement gives a confused and wrong impression of what actually is the case in Holst (2017); Čašule's readers will not have a chance to understand the background. The verb 'to give' is suppletive in Burushaski. One root, **-ć*- 'to give', corresponds to Ka. *č- 'to give' (Holst 2017: 175) – by the way a "monophonemic

correspondence", but a fully justified one. Another root is *-ɣún- 'to give'. It is now essential to know the following facts which Holst (2017: 6, 175, 321) lays out. In several languages a stem for 'to give' can be analyzed as 'to take', 'to have' or similar + a causative morpheme, i.e. 'to give' was designated as 'to make take', 'to make have'. Now, interestingly, Proto-Kartvelian has a stem *-ɣ- 'to take', and it has a causative suffix *-un-. It is hence natural to assume that these two morphemes not only existed in Proto-Kartvelian, but also already in the common proto-language of Kartvelian and Burushaski, and that they still survive in Burushaski, not in isolation, but in the shape of *ɣún-* 'to give', which must have been formed with the two morphemes at some point in time. Hence, actually the cognate set not only compares one position, but three.

Čašule (2017: 4) criticizes some vowel correspondences. First he says that Ka. *i* enters into the correspondences *i / i*, *i / a* and *i / e*. The regular correspondence here is *i / i*. As to *i / a*, it is essential to know that Kartvelian has ablaut in verb roots, which includes an alternation of *i* and *e*. The etymology Čašule is referring to is a verb root: 'to chase' (Holst 2017: 176f.). The Burushaski data apparently go back to an allomorph with *e*, and this *e* was shifted to *a* before *r* (a regular sound law with eight examples alongside 'to chase', Holst 2017: 184). Ablaut is discussed by Holst (2017: 190–193). As to *i / e*, this is indeed an irregular correspondence in the set Čašule refers to: Georgian *k'i* 'and' / Hunza *ke* 'and' (Holst 2017: 178). However, I clearly think that these two items are cognate: the consonants match (consider the "plosive shift", mentioned in section 3), and the vowel correspondence is also irregular in English *and* / German *und*.

Čašule (2017: 4) goes on to say that Ka. *e* enters into the correspondences *e / u*, *e / a*, *e / e* and *e / i*. The basic regular correspondence here is *e / e* (Holst 2017: 184f.). But also *e / a* is a regular correspondence; it is the one that applies in final position (Holst 2017: 183). The correspondence *e / u* in 'tongue' is indeed irregular (Holst 2017: 112). However, the claim by Čašule (2017: 4) that in the Burushaski word "final vowel and ending are disregarded by Holst" is not true, see Holst (2017: 345), where nine examples for the suffix in question are given, laid out with material from Burushaski. Finally, as to *e / i*, this is not a real vowel correspondence, but in Burushaski the original vowel is absent due to "syncope", and *i* arose as an epenthetic vowel. Syncope is discussed by Holst (2017: 185–190). There are three cognate sets for which it is essential to know this background, see Holst (2017: 188), and Čašule hit upon one of them.

Čašule (2017: 4) also criticizes consonant correspondences. When he says that some of them "involve radical changes", this is in fact a correct observation: the sibilants, for instance, were shifted according to certain laws in Burushaski, but these are laid out in detail by Holst (2017: 213–220) and should be studied in order to understand the cognate sets. Čašule (2017: 4) also speaks of "frequent consonant loss on either side". For Kartvelian this statement is incorrect, but to Burushaski this indeed applies. However, the losses Čašule is referring to – of *w* after consonant and of *x*, for instance – are regular, and they are of course explained in detail in Holst (2017). Subsequently, Čašule (2017: 4) says: "Holst (2017: 108) himself states that in Kartv [Ka.] *per- 'ashes' and Bur [Bur.] *phet- 'ashes' the correspondence *r:t* is irregular and does not occur elsewhere." This sentence is correct. It leads to the question: how many irregularities are contained in the cognate sets?

In order to answer this question, it is necessary to first know how many correspondences the cognate sets have altogether. Let us recall that the table given earlier in this section shows how many cognate sets compare one position, two positions, etc. Obviously one has to multiply for each line of the table the number of positions with the number of etymologies, and then add the six results. This leads to a modified table:

no. of positions	no. of etymologies	no. of positions
1	2	2
2	9	18
3	48	144
4	34	156
5	12	60
6	5	30
	total:	390

Hence, the entire body of the 110 cognate sets consists of 390 positions. (The chapter on morphology, pp. 243–346, compares about 50 morphemes, hence adds many more instances, somewhere between 50 and 100, but let us ignore this here.)

I then investigated the "memos" of the 110 cognate sets with regard to whether they contain irregularities, considering the historical phonology pp. 179–242. Ka. *per-* 'ashes' and Bur. *phet-* 'ashes', for instance, has to be counted as one such set since *r / t* is not regular (cf. above). For three sets syncope had to be taken into account, and for two sets ablaut; the effects of these morphophonological phenomena could not be counted as irregularities. However, there were sometimes difficult decisions to be made. I arrived at 26 sets with irregularities. Suppose someone else would arrive at 30 such sets, being somewhat stricter here and there, this would still leave 80 sets in which all positions compared match, and that is an excellent result. Moreover, in those 26 sets which contain irregularities in my account, it is almost always only one position which shows an irregularity, while the other positions match – as in 'ashes': one position (the third one) does not match, two do. Suppose someone with a critical attitude would count 35 irregularities, this would still mean that 355 of the 390 positions compared are regular. This, too, is a brilliant result.

Moreover, concerning the irregularities certain issues must be taken into account. Although the matches are irregular, the sounds are similar in each case. Usually only one feature (in the phonological sense) is not like it should, e.g. velar / uvular, voiceless / voiced. Additionally, in many cases I give tentative reasons why the irregularity might exist: e.g. dissimilation with 'lung' (Holst 2017: 121, 197), sandhi with Ka. *da* / Yasin *da* 'and' (Holst 2017: 183) and with 'I' (Holst 2017: 220), or onomatopoetic influence with 'to fly' (Holst 2017: 203). Most importantly, one must acknowledge that occasional irregular correspondences among related languages are simply a fact. They can be found between any two related languages. Also among the Kartvelian languages themselves there are irregularities, of course, and also among the Burushaski dialects. It is impossible not to find irregularities when investigating the more distant relationship.

One could of course remove a set from the argumentation if it contains an irregularity, but this would not make sense since such sets are usually probably correct nevertheless: due to the other, matching, positions, due to the fact that the irregularity affects only one feature (not the entire sound), due to possible explanations of the irregularity, etc. The question that I asked myself when finally taking up a set or rejecting it was: is my overall impression that the set is correct, i.e. that it represents genuine language history? This was for me, after all, the decisive criterion. All my background knowledge on historical linguistics in general and on the case of Kartvelian and Burushaski in particular influenced the decision. Another question is whether the irregularities could be detrimental to the argumentation for relationship. However, with only something around 35 of 390 positions being affected, and usually only in one feature, this is impossible.

Holst (2017) employs by far the most regular historical phonology that someone has ever presented when arguing for a relationship of Burushaski to other languages. This radically differs from all previous approaches. In my opinion this has only been possible because Burushaski is indeed related to Kartvelian.

6 Final considerations

As can be seen from what has been said, the criticism that Čašule (2017) has voiced with regard to Holst (2017) can be refuted. Section 2 of the present paper showed that already Čašule's introductory statements are beside the point. Section 3 showed that by no means does it apply that many loanwords were erroneously employed. Section 4 showed that the semantic latitude involved is largely non-existent, and in the remaining instances it is largely not spectacular. Section 5 showed that the historical phonology is very regular and the few irregularities are natural. Čašule's charges are based on misunderstandings, exaggerations, insufficient study of Holst (2017), wrong expectations about what distant language relationship should look like, and the like. In many instances, new thoughts were added in this paper which are intended to bring the debate ahead.

The crucial claim of Holst (2017), namely that Burushaski and Kartvelian are related, is not affected by Čašule's statements. There is an enormous wealth of evidence in Holst (2017). It could not be discussed here because the purpose of the paper was very narrow in scope: to react to Čašule (2017).

Needless to say, I have an entirely different opinion about Holst (2017) than the one Čašule has. I regard it as a work where all pieces fit together, long, complex and in parts highly demanding, but making its point. A problem of a language isolate is solved. It is a pity that the abundant morphological evidence, on more than hundred pages (pp. 243–346), was not studied by Čašule, nor the syntactic issues and other aspects. As to the other theories on the origin of Burushaski, they may be scrutinized as to whether they partly actually refer to contact phenomena concerning Burushaski.

Interestingly, my earlier book on Burushaski, Holst (2014a), which was not yet concerned with the relationship to Kartvelian but investigates the language in its own right, is judged by Čašule in a positive way. He writes: "His earlier book (Holst 2014) is a significant contribution to the morphonology of Burushaski and is useful for the internal reconstruction" (Čašule 2017: 4). Just two brief comments: "morphonology" is probably intended here as a cover term for both phonology and morphology, and "internal reconstruction" probably is to cover the dialect comparison as well (as in Gerber 2017: 192). It is my hope that Čašule may revise his opinion on Holst (2017) and judge it in a similar way as Holst (2014a), that is to say, as a "significant contribution". Many other scholars already have a positive opinion about Holst (2017).

ABBREVIATIONS

Bur.   Burushaski
Ka.   Kartvelian
pl.   plural

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