More *Inebra*: An unnoticed meaning of PIE √*kelH and a bit more[1]

Alexis Manaster-Ramer

> PIt. *-or- is reflect both as *or* and as *ur* in Latin; no phonetic conditioning has been found. — De Vaan (2009: 158)
>
> There is increasing concern that most current published research findings are false. — Ioannidis (2005)

I had only very limited dealings with Evgenii Helimskii, less even than with the Moscow Nostratic schools as a whole — but I am as familiar with his work as a mere outsider can hope to be. And I believe that I am not wrong when I say that, whereas he might not have agreed with much of what I say here (which extends the same basic approach that I began to sketch out in the 1980s and 1990s in my work on the Nostratic and Altaic questions, where I did not endorse either theory but rather refuted numerous constantly-repeated but entirely invalid aprioristic objections to them, suggested ways of improving the case for both by cutting out some clearly unwholesome parts,[2] and even found one or two pos-

[1] Many thanks for comments and bibliographic and editorial assistance to Roberto Batisti, Alexander Nikolayev, Brent Vine, Rémy Viredaz, and Michael Weiss.

[2] Most of this works seems to me to remain valid and current, namely the many detailed refutations of various aprioristic, circular, contradictory, or even simply factually incorrect claims used as blunt weapons in those debates since 1940 or even before — and apparently still being so used. However (as also noted in part in Manaster Ramer 2021b), most or all of the superficial and uncritical (as I now see them) Altaic comparisons cited in Manaster Ramer & Sidwell (1996) and Manaster Ramer *et al.* (1997) have to be wrong (as argued, though in a very different SPIRIT, by Georg 1999), and even those that may be right (e.g. the 'tooth' word) have to be redone. More generally, I now believe it is wholly unreasonable to expect very ancient language relationships to be reflected by e.g. shared numerals (because people would hardly have counted with words) or by almost ANY shared WORDS, including the allegedly "basic" ones (because of the passage of time and the massive replacements of inherited vocabulary by kennings typical of just such archaic cultures) — as opposed to shared PIECES of words (to be found by careful analysis of derived and especially

Valentin Gusev, Anna Urmanchieva, Aleksandr Anikin (eds): *Siberica et Uralica: In memoriam Eugen Helimski*, 293–315 (Studia Uralo-altaica, 56). https://doi.org/10.14232/sua.2022.56.293-315

sibly supportive facts),[3] he might at least have found some of my remarks amusing — though I would of course have tried to show him that what I defend here is not JUST fun, but also the ONLY way open to us first to redo (which is essential) the prehistories of the universally recognized language groupings and second to figure out (if this can still be done) their interrelations (which means borrowings too of course).[4]

Inebra,[5] it appears, were all the things taken by Roman augurs as the ostensible justification for stopping whatever actions (of the magistrates) they wished to stop. It has for decades been my observation that in every domain of human action outside of science and in almost all areas of science too[6] there are "augurs" who behave at least some of the time just like their Roman predecessors (not always and maybe not even most of the time,[7] but definitely some of the time, and especially on SELECTED KEY points). In such cases, in almost every field of science (like in every field outside of science), that which should be proven is routinely assumed instead — thus corrupting everything else and allowing fake claims (myths, disinformation, whatever you want to call it) to run rampant (Trifonova & Manaster Ramer 2019, Manaster Ramer 2021a) and at the same time creating a nearly impenetrable wall protecting these errors from any challenge that would actually be heard, much less listened to, much less acted on.

---

compound words, such as has hardly even begun). A simple example: the Turkic word for '5' does not come from a word for 'hand' or 'fist' but rather one for 'wrist' or 'joint'. It may then derive from a root meaning 'to join' or 'to bud', which may or may not have cognates in Uralic and Indo-European, but neither the numeral sense nor even a trivial sense like 'hand; fist' can help us make these deep connections. Any such claims (including my own *apud* Johnson 1995 and in Manaster Ramer *et al.* 1998) are doomed and can only distract us from reaching that goal (if it can be reached).

[3] I refer to the Nostratic clusters I proposed in Manaster Ramer (1994) and *apud* Johnson (1995), which still seem to me a reasonable idea — of course subject to such normal scientific discussion and testing as has of course still not taken place.

[4] I am now sure that most (though not all) of the commonly cited Turkic-Mongolic comparisons are in fact borrowings.

[5] Neuter plural. The feminine plural **inebrae** referred more narrowly to the flight of birds so interpreted.

[6] In my case, theoretical linguistics of the Chomsky (or Chomsky-Montague) type was the first science I worked in, followed by Native American (specifically Uto-Aztecan) historical linguistics some years before getting involved in Turkic and then (quite separately) Nostratic (and only through that Altaic and Indo-European), and much later various other fields from (rather basic) mathematics (in so-called "mathematical" linguistics and of course in computer science) to history to medicine (e.g. Manaster Ramer 2020a).

[7] Critics of medical research routinely assert that there error prevails MORE THAN 50% OF THE TIME (e.g. Ioannidis 2005) — and their claims are neither rejected nor met with derision and studied silence. On the contrary, they are insanely widely read.

My very first contact with the historical linguistics of Northern Eurasia in fact revolved these quite basic issues: someone introduced me (then either still a graduate student or a fresh PhD) to Robert Dankoff (then also rather younger than now), who asked me two questions: could any language have three vowel lengths and could any language undergo zetacism (the change of some rhotic sound to a voiced fricative like the final consonant of Turkish **kaz**). While I was able to assure him that both were rare but documented occurrences to my own knowledge (the first in Estonian and one dialect of Hopi; the second in some few dialects of Polish),[8] I maintained (and still maintain) that that is not what matters. A rare phenomenon might after all not have occurred at all, or occurred but not have come to the attention of this or that (or any) linguist. And even what is and is not rare cannot be taken for granted — and especially not in relation to prehistory (because we do not know in advance which features of language are actually connected to culture and technology). We anyway, logically, cannot insist that any phenomenon be denied unless it has already have been documented somewhere else before (and accepted by the nomenklatura, too), since that leads to a logical regress.[9] If we did that, we would not even be using fire much less stone knives and bear skins.

We should then not even be asking these kinds of APRIORI questions, but rather the SUBSTANTIVE questions of whether, in the first case, either extant Khalaj, medieval Karakhanid, or Proto-Turkic did have three or just two vowel lengths (or something in between),[10] and, in the second, whether the Shaz branch of Turkic did undergo zetacism or

[8] Of course, with education, almost all speakers of Polish today have a different sound in these words instead (reflecting a much more widespread dialect type), a sound rather more similar to Turkish **j**. But it is trivial for such a sound to change to [z], as we know again from a third (this time quite large) Polish dialect group.

[9] Anyone who doubts the existence of a nomenklatura that believes it gets to decide everything should read more widely, e.g., Brian Joseph's remark in Johnson (1995) about "the more mainstream linguists" who may or may not "decide to reject Nostratic". Who appointed whom a "mainstream linguist"? Why notably were fanatical apriori rejectionists supposedly "mainstream", whereas supporters were not? And above all why was I, who was simply trying first to break down the wall of silence and then to refute the logical and factual errors told about the Nostratic (and Altaic) work (including those of the supporters!) not "mainstream"?

[10] In light of the work Dankoff and I and later I alone did, I feel sure that Karakhanid and Proto-Turkic had two phonemic vowel lengths but not whether or not there were significant allophonic length differences as well. As for Khalaj, I feel rather sure that (1) the linguists who heard it did hear three kinds of vowels, two of them clearly phonemic and reasonably correlated with the two-way length contrasts in some other Turkic languages, and (2) that the work that would be necessary to establish a three-way phonemic contrast had not been done (which does not mean that no such phonemic contrast actually existed but merely that it remains unproven). The distinction between phonemic and allophonic is key here. As I have already mentioned before (Manaster Ramer 2020b), IE linguistics has for decades failed to even separate the question of a PIE *[a] allophone (which ob-

whether on the contrary the Lir branch went through rhotacism (or maybe something else again).[11] For some fifteen years before roughly 2000 I was involved in various efforts aimed at refuting the various forms of this sort of aprioristic, circular kind of reasoning[12] in Turkic, Altaic, and Nostratic studies, but did substantive work only in Kartvelian and some tiny corners of Indo-European (mostly Armenian).[13]

viously existed) from that of a PIE */a/ phoneme (which almost certainly did not because it was an allophone of the so-called */h_2/ phoneme) and more generally that of consonantal (so-called "laryngeal") allophones (which again obviously existed in some positions) vs. that of a series of three "laryngeal" phonemes distinct from the corresponding three vowel phonemes (for which I have never seen any evidence, only question-begging, notably some strange assumptions that given a correspondence between a vowel in one language and a "laryngeal" in another, the "laryngeal" has to be the original sound).

[11] I feel reasonably sure that Proto-Turkic did not originally have either a simple **z** sound or any simple rhotic (the so-called *$\mathbf{r_2}$) but rather a special (surely palatalized and fricativized) allophone of the ***r** (i.e., *$\mathbf{r_1}$) proto-phoneme, this allophone being conditioned by (some) following consonants and/or a following ***i** (with these conditioning segments being subsequently lost or absorbed into the allophone in question). Of course, almost everything remains to be done on this — as on so many topics, but the idea of course is directly inspired first of all by Street's (e.g. 1980) work on the parallel problem of ***š** vs. *$\mathbf{l_2}$, where the evidence for a similar (though not identical) allophone solution is to my mind irrefutable.

[12] The general issue is not so difficult to understand. Small children do. In *Alice in Wonderland* the "King of Hearts" makes up on the spot a "rule" which he says is number 42 and at the same time "the oldest rule in the book", designed just to get Alice expelled from the room. In exactly the same way — which even quite young children do see through — senior (and other) scholars keep making up all kinds of "rules", e.g,, that related languages must share lower numerals and/or "basic" body part terms. What is the factual or logical basis for either of these rules? Does anyone even know where either one comes from? I myself do not about the second one. The first comes clearly from Messerschmidt's (and it was definitely him, as discussed by Manaster Ramer & Bondar' 2018) realization, not *a priori* but *a posteriori*, that, for the languages he was trying to classify in the first quarter of the 18^{th} century, the first time in history such work had ever been undertaken, numerals seemed an ideal basis for classification. From this of course it does not follow that this should be an absolute requirement for every language classification problem, and in particular not for relationships much remoter than those he was able to detect. Right here we see the whole difference between science and the science fiction that has for too long dominated the discussion of these kinds of issues in connection with, notably, Altaic. I have repeatedly called for an end to such made-up "rules" and "criteria" — evidently to no effect whatever.

[13] I got into Armenian simply because on one occasion I asked the late Eric Hamp directly just what it was he had against Nostratic, and all he managed to say was that "Illich-Svitych got the Armenian plural ending wrong". So I got into this topic, found out that Illich-Svitych had indeed gotten it wrong (but that he did so because all Indo-Europeanists had, so there is no methodological difference between the Nostraticist and the received Indo-Europeanist approach, since the man after

After utterly failing to get any of these fields (and others) to hear this message, I anyway found myself taking a sabbatical of nearly two decades, which was interrupted by my 2019 lecture "Türkoloji 4.0"[14] at Dicle University and then the first publication in two decades at all connected with this field (Manaster Ramer 2021b, in honor of Marcel Erdal), the present contribution in memory of Evgenii Helimskii being just the second. In both of these I am trying a new approach, a "mote and beam" one. The idea is to show specialists in fields like Turkic, Altaic, and Nostratic that the methodological horrors so prevalent in these fields also go on in other fields (in Fs Erdal it was Biblical exegesis; here it is Indo-European linguistics) and that (which is what really matters) these can easily be remedied. Whether seeing the beam in the Indo-Europeanists' eye will help the students of Turkic, Altaic, and Nostratic to recognize the mote in their own, and to get to work on removing it, remains to be seen.

Out of many equally possible examples both of the problems and the available solutions, I decided to present here my work[15] on an IE root of the form ***kelH** (which is almost certainly not even a "new" root but merely a previously unnoticed semantic range of a known one), the meaning (apparently unrecognized so far) being that of 'to scratch (*or otherwise* injure) the surface, *or the like*; to just barely (*e.g.* emerge, approach, touch, hurt/cut, *etc.*)'.[16] There are in various branches of IE very many words starting[17] with re-

all was a distinguished IEnist), and solved it myself (Manaster Ramer 1996), only of course to find in 2022 that the solution is still simply being ignored.

[14] The title alludes to my idea that, in addition to the Pro-Altaic and Anti-Altaic factions (without saying which one is 1.0 and which one is 2.0), there is a very large group of scholars (3.0) who sit on the fence without helping to move the field forward or even just to moderate the worst excesses (methodological, substantive, and rhetorical) of the first two, and therefore that the only way forward that is left is the one I advocate (4.0) — even if for now I stand alone.

[15] This of course is to be seen in the context of a much larger body of work, exemplifying a unified approach and methodology.

[16] The state of PIE semantic reconstruction is such that I see no benefit to saying more. Whatever root this is, the meaning of that root will have to be completely rethought — as I believe is the case with most PIE roots anyway. The meanings we are taught are for the most part vague, often abstract, mechanically derived from those of the attested reflexes as a sort of lowest common denominator, and often entirely anachronistic. This is certainly so in the case of what Mayrhofer (*EWA* I: 321f) correctly refers to as "einer unüberblickbaren Menge idg. *(*s*)*kel*-Ableitungen mit vagen semantischen Ansätzen ('schneiden' ~ 'schlagen' ~ 'stechen' […]". And of course 'to hide', also. If I am right, of course, the last two may actually involve one and the same root.

[17] In fact, once one opens one's mind, even just barely, to the possibility that any part of what I say might be right, a great many other hitherto mysterious words start making sense, but I just mention a very few here. There are also compounds ending in a postpound (later analyzed as a suffix) derived from this root. All in good time — if there is time.

flexes of *$\mathbf{kel}(H)$-, *$\mathbf{kol}(H)$-, or *$\mathbf{kl}(H)$- that can now be immediately explained, some completely, others so far only in part. But it all began when I thought of these:

1. Celtic ***koligno**- 'pup, small animal', which is then neither "borrowed from some non-IE language" nor yet "highly speculative[ly]" derived "[t]heoretically [...] by dissimilation from *koni-gno-" (Matasović 2009: 213), whereas I would have thought that it would be obvious to anyone seeing the postpound -**gno**- that it must be precisely a QUINTESSENTIALLY IE compound. And so it is: simply *'just barely born' — than which nothing could be more perfect formally as well as semantically.

2. Latin **colustrum** (later **colostrum**) 'beestings (i.e. the first milk)', hitherto etymologically "uncertain"[18] (de Vaan 2008: 127f) surely makes sense as ***kol-uestrom** (or some similar form, which is not my concern), the postpound being an instrument noun from **vēscō** 'to feed (with)', hence *'first thing used to feed (a newborn)', which is semantically just as perfect, and formally too, though there are of course issues here involving the verb itself and in particular its long vowel (see e.g. de Vaan 2008: 669), though on most or even all imaginable scenarios ***uestrom** (or, if necessary, ***ustrom)** should be possible as a derived noun, and that is all that we need.

3. Old Indic **kalā́**- 'small part of anything, any single part or portion of a whole, esp. a sixteenth part' (unexplained according to *EWA* I: 321f.) if taken from ***kolHah$_2$**-.

4. Greek (Hesychius) **κελεβρά** (pl.) 'weak and dying heards', which need not be corrupt for *$\boldsymbol{\kappa\varepsilon\nu}^0$ (as tentatively suggested by Beekes 2010: 668),[19] but of course the postpound has to refer to eating and so is yet another example of the rule of laryngeal loss (since otherwise it would be **†κελεβαρα** or the like) that Beekes not only denied (which is logically fine) but also (which is not fine at all) circularly rejected any and all (old or NEW) examples of, simply on the grounds he had rejected the rule to begin with (see e.g. Beekes 2010: 963 *ad* **μολοβρός**, a word I disucss below). Instead, it can be *'just barely eating' — than which again nothing could be more per-

[18] If anything could make me speechless, de Vaan's idea about this word would: "It is tempting to connect *colustra* with *color* 'colour' < *'cover', and to postulate a semantic link between 'colour' or 'cover' and 'beestings'. Yet this is not semantically straightforward [...], nor is the suffix -*tero* normally used to derive comparatives from any adjective. THIS is what historical linguists find "tempting"!? So de Vaan says — and who am I to disagree?.

[19] Can anyone not see a pattern, given that Matasović was willing to consider a similar thing for the Celtic word? The pattern is that data count for less than the maintenance of a system in which much of the theory (such as the PIE roots) is simply taken as given — and at best only a special cadre is authorized to make changes. And since no root such as the one I am describing had been described before, the data had to be fitted to the theory (either by being altered or or else taken as borrowings).

fect formally as well as semantically — but only provided the IEnists are willing at long last to give up the (very common) practice of taking some position and then accepting only data (or putative data) supporting that position simply because it supports that position and rejecting counterexamples simply ON THE GROUNDS THAT THEY WOULD BE COUNTEREXAMPLES.[20]

5. Latin **celeber**-, in its earliest sense of 'where there is a multitude', and said to be without an etymology (*DELL* 110; Bader 1983: 43;[21] Nussbaum 1999: 388f, 411 n. 67; de Vaan 2009: 104), can now be explained as ***kele-dʰri**- (or ***keli-dʰri**- or even ***kelH̥-dʰri**-) 'barely holding',[22] than which I can imagine no more perfect fit between a proposed etymon and the attested form and meaning of the word whose etymon it would be. Of course, since this solution, as we are about to see, contradicts what is to be found in the writings of some authorities far greater than mine, some will object to my saying this (or simply ignore what I just said, thus seeking to prevent any discussion of it at all), again simply on the circular grounds that what I say would be a counterexample to those authoritative claims. And it is for this reason really that I am writing the present contribution, since here, instead of being able to focus the rather exciting discovery a long list of etymologies that have suddenly fallen into place, I find myself forced to deal with the same methodological issues again in my life — the refusal of an entire field of science to accept that theories ultimately are based on data and that data may not be rejected just because they conflict with someone's theory, forcing some revision thereof.

And because this is the whole problem with the work on such deeper questions as Altaic, Nostratic, and so on, I thought this would be a perfect place to explain in detail what I see as yet another example of the same basic miscarriage of methodology. This should actually become apparent as we read de Vaan saying (inter alia):

> The etymology of *celeber* is unknown, cf. Nussbaum 1999[] and Bader 1983. Phonetically, **kelesri*, **kelisri-*, maybe **kelVdʰli-* are possible.[23] [...] But a suffix **-dʰli-* is unlikely,

[20] This qualification is crucial. It is perfectly fine to question putative data if there are INDEPENDENT reasons for doing so. But this is precisely what Beekes does not do in the places quoted (and many others). And he is in no way atypical of the field as a whole. On the contrary, he is entirely typical.

[21] Though Bader seems to believe she can derive it from a basic sense of SOUND!?

[22] Maybe ***kele-bʰri**- or ***keli-bʰri**- 'just barely carrying' could also be considered.

[23] He does not say under whose rules these etyma would be possible. Under the rules of Nussbaum (1999) that he cites immediately below that, only ***kelisri**- would be possible. De Vaan is evidently mechanically (but incompletely) copying Bader's (1983: 43) list of ***keledʰli**-, ***kelHdʰri**-, ***kelHsri**-, and ***kel**(*H*)**esri**- as possibilities (of which she apparently had a preference for the last), before switching horses to Nussbaum (1999).

> since *celeber* does not show the instrumental meaning which adj. in *-bilis* and *-bris* usually have [...][24]; phonetically, **kelH-bli-* should yield **kelabri-* > **koliber.* Thus, Nussbaum 1999: 388 is probably right in positing **kelisris* which yielded a non-velarized **l*, and with lowering of **izr-* > **-ezr-.*

The circular and/or contradictory[25] double-standard reasoning that is either explicit or implicit here occurs at several different levels. At the third most general level, everyone who studies Latin (including of course de Vaan and Nussbaum) recognizes that this language (like every language I have ever studied, and I suspect the same is true of every language they have studied, too, and likewise for every other linguist and every other studied language) shows numerolect a complex history where (supposedly) at a stage prior to what we observe (and accorus examples that do not follow a single "regular" set of sound correspondences, and these examples are then mostly explained as borrowings from various nearby closely related languages, dialects and even sociolects, analogies, contaminations, sporadic dissimilations, assimilations, etc. — or in some cases are actually admitted to be unexplained. Consider e.g. the realization of PIE *-r̥- as both -**ur**- and -**or**- in Latin, according to de Vaan (2009: 158 s.v. **currō**), as quoted as one of the epigraphs to this essay, which could be endlessly supported by quotations from any etymological dictionary or historical grammar of Latin (or any other language). Why could the same not be the case with **celeber** if it should turn out that this involves one or even two sound correspondences different from what Nussbaum and de Vaan assume?

At the second most general level, the whole concept of regularity of sound laws is here (and throughout historical linguistics) being misunderstood and misused as a blunt weapon aimed at completely valid results (even as others, either equally irregular or far more so, are widely lauded). The theory of regularity of sound change never claimed (and never could claim given the universally known facts) that in any observed group of dialects or languages the observed data will show an absolutely regular set of correspondences. Rather, the theory claims that the actually somewhat (or even very) irregular correspondences that we often find refding to some, an unobservable stage in principle) there WAS regularity,[26] which was then obscured by borrowings, analogies, and all the rest of it.

[24] This argument of course is moot if the word is, as I propose, a compound noun and not an adjective of the type de Vaan is considering.

[25] The contradiction can be purely logical or it can involve contradicting facts that the author in question accepts as facts. This is a crucial qualification because it is of course also a constant problem in this, as in any field, that non-facts are accepted unquestioningly as facts, and real facts are simply rejected/ignored.

[26] There are of course those, including me who disagree with this theory. But that IS the theory. On the other hand, there is no theory in historical linguistics that says that every OBSERVED will reflect the "regular" sound laws. There just is not.

At the most general level, of course, all science involves an interplay between data and theory, where it is not permissible to simply dismiss a piece of data solely on the grounds that it conflicts with a particular theory — since it might be that it is this is the very data that shows how the theory needs to be revised instead.

To be sure, here there is anyway no conflict between the datum (**celeber**) with the theory at all. And the reason it does not conflict with the theory is twofold. One, as we already said, even if the sound laws predicted a form other than **celeber** (namely, †**coleber** or maybe †**coluber** or whatever you want) in Latin "proper", the attested form would (as I have already shown) normally be treated as a "borrowing" from a closely related language, dialect, or sociolect, or perhaps a sporadic "assimilation" of the vowel color, or whatever other kind of "irregularity" the specialists in this field routinely invoke to explain other words. So, even if, under the rules assumed by de Vaan (which of course themselves should be, but are not being, subjected to testing and criticism based on FACTS), **celeber** were not the "regular" output of ***kele-d**^{h}**ri**- or ***kele-d**^{h}**ri**-, the methodology of historical linguistics in general and of its IE and Latin branches in particular immediately provides us with the tools to explain such "irregularities", without throwing the data out the window.

Moreover, in fact, there is no basis whatever for regarding **celeber** as "irregular" anyway, because there is actually no sufficient factual basis for any theory of Latin historical phonology predicting that the form would HAVE to be OTHER than **celeber**.

Let us examine this. There are three separate issues here. One is Latin vowel reduction in medial syllables (and whether this will give us -**e**- or -**i**-). The second is the two different **l** sounds of Latin (which were not generally distinguished in writing),[27] a darker, velarized (*pinguis*), sound and a lighter, neutral or palatalized (*exilis*) one, and the conditions under which we get the one or the other. The third is the change of *-**eł**- to -**oł**- in the first syllable of a word. Specifically, the issue is how to avoid getting the word to begin with †**col**- on the one hand and how to get the middle vowel to be -**e**- and not †-**i**- (or possibly something else) on the other.

Now, in order for to get the form **celeber** as it is, the -**l**- supposedly has to be light (given the sound laws formulated by Nussbaum 1999), because otherwise the first vowel would have changed to -**o**-.[28] Next, Nussbaum claims that the -**l**- is light before an original

[27] If they had been, all the vast quantities of writing on this subject by Modern historical linguistics would have been devoted to some other unsolvable problem instead.

[28] This itself is not entirely straightforward. Nussbaum (1999: 390) claims that the fact that **scelus** 'crime' does not change the -**e**- to -**o**- before an apparently velarized -**ł**- "suggests that *e* > *o*/__*ł* failed to operate after an initial *sk*-. But there is an immediate phonetic plausibility to the hypothesis that *s* would have made an immediately following *k* both fronter and less roundable than the average *k*, with the result that the *sk*- cluster would work against the backing and rounding of a following *e* to *o* before *ł*. It thus seems reasonable to conclude that *sk*- inhibited *eł* > *oł*, while lone ve-

*-**i**- but velarized before an original short *-**e**-. However, the only example cited is the suffix "-*ulentus*, which hardly reflect[s] anything but *-*ento*- meaningfully" (Nussbaum 1999: 409 n. 47), which is perhaps not the strongest sort of argument imaginable. In reality, the -**l**- of this suffix is not original, though. The suffix is assumed (plausibly) to be derived from *-**o-u̯ent-o**- via a metanalysis to *-**ou̯ento**- and then via a (by definition irregular) dissimilation of the *-**u**- to -**l**- after a stem containing a labial sound (Weiss 2021: 317). But wait: if this instance of -**l**- is derived from *-**u̯**-, it stands to reason that it would have been dark, just from what we know of general linguistics. It does not at all follow that an INHERITED *-**le**- sequence would have given dark -**łe**- as well.[29] Still, let me for the sake of domestic peace grant all this and give up ***kele-dʰri**- in favor of ***keli-dʰri**-. This way, of course the -**l**- will be light, and maybe I will final be allowed to breathe and be heard? Not

lars did not". This does not seem at all phonetically plausible to me, and anyway that is not the issue. According to the "rules" constantly invoked (notably to combat anything I have to say) one is not permitted to base anything on such speculative "plausibility". Instead supposedly one has to show numerous examples of a sound law, which evidently has not been done here. So, if there is simple human equality, by the same token, then, and indeed *a fortiori* I get claim that there is "an immediate phonetic plausibility" to -**br**- from *-**dʰr**- and/or *-**bʰr**- having the same lowering effect of *-**i**- > -**e**- as does -**br**- from *-**sr**-, even if I should have but one example. I say "a fortiori" both because in fact this IS plausible and because in any case subtle changes of unstressed medial vowels in a language that anyway massively (and somewhat variably) reduces these are much more initially plausible than a much more pronounced effect on the (prominent) initial syllable vocalism. What is sauce for **scelus** is far more sauce for **celeber**. And to not accept this would be worse than a *scelus*, it would be a mistake (except sociologically of course). I would add that the evidence cited by Nussbaum for ***kel**- sequences supposedly giving **col**- rather than **cel**- is very weak anyway (and it is only sociology that can explain why it was ever accepted). Neither **color** 'color' nor **columen** 'pillar' nor above all **culter** 'knife' (which anyway now gets a new and a REAL etymology for the first time!) are clear examples of *-**el**- (and a careful reader will see that Nussbaum concedes as much). The evidence is actually better (though apart from **scelus** also not decisive) for the opposite conclusion, which moreover is really phonetically plausible because a fronted initial **k**- preceding the front vowel -**e**- would naturally help keep that vowel from backing. And the evidence of **scelus** precisely IS decisive. The whole infrastructure of speculations and assertions about the -**e**- in **gelus** and **gelidus** likewise has no foundation. Finally, of course, we do not actually seem to have any reason for saying that the -**l**- is a word like **scelus** remained (even if it once was!) dark. This is just simple confusion of categories, the categories of cart and horse.

[29] As far as I can tell (and Michael Weiss, p.c., confirms this) there is not decisive evidence for velarisation before short ***e**. Of course one can assume that there was because there was before the long **ē** but this is not self-evident. The short vowel may have lower than the long one centuries later (as we see in Romance), but it does not follow that it had been so in prehistoric times. Compare the Slavic *jat'* vowel, which is some places must have been low (Eastern Bulgaria f.ex.) and in others high (e.g. Ukraine).

so fast. There is still remaining the vowel reduction. What about that? All that Nussbaum writes is this:

> *celeber, -bris, -bre* […] falls short of establishing that **eł* was, exceptionally, preserved after velars in initial syllables. Since the word has no obvious etymology,[…] there are no real grounds for rejecting the idea that the *e* in the first syllable was preserved simply because the *l* that followed was non-velar.
>
> This amounts to assuming a pre-form like (**kelisri-* >) **kelizri-*, which is as likely as anything else, and a regular development of **-izr-* to **-ezr-* (> *-ebr-*) in Latin, which there is some reason to believe:
>
> a. **-z-* and *-r-[…] each had the effect of lowering a preceding heterosyllabic internal **-i-* to *-e- : *kepizam > ceperam* (cf. *cepisti* etc.), **keniz- > ciner-* (cf. nom. *cinis,* Gk. κόνις 'dust'), **Faliziio- > Falerium* (cf. *Falis-cus*); **legi-rup- > legerupa, *keliri- > celer(is)* as above,
>
> b. **-rz-* (or *-rr-)* as a medial sequence also seems to have exercized a lowering effect — at least in comparison to **-lz-* (or *-ll-*): **sakrisamo- > *sakar- zamo- > sacerrimus* but **faklisamo- > *fakəlzamo- > facillimus*; and if **-ərz-* (or already **-ərr-)* gave *-err-* while **-əlz-* (or already **-əll-)* gave *-ill-*, it does not seem unthinkable that **-izr-* might end up with an *-e-* as well.
>
> […]
>
> However any of this may be, *celeber* obviously cannot establish a failure of *eł* > *oł* after velars in the absence of a cogent etymology and morphological analysis.

This prose can hardly be considered as proof of anything, given Nussbaum's own turns of phrase: "Since the word has no obvious etymology,[…] there are no real grounds", "a pre-form like (**kelisri-* >) **kelizri-*, which is as likely as anything else",[30] "However any of this may be", "obviously cannot establish […] in the absence of a cogent etymology and morphological analysis", and so on. Also, we should be considering the CONTEXT: Nussbaum is only trying to get **celeber** out of the way as a possible counterexample to his general (including after velars!) rule rounding of -**eł**- to -**oł**-, and not looking to find the etymology of the word itself or even the applicable rules of vowel reduction.[31]

So the question really is what ARE the rules applicable to the middle vowel of **celeber** < ***celebri**- if this did NOT come from Nussbaum's ***kelizri**- < ***kelisri**- after all but rather from my ***celedʰri** or ***celidʰri**-. Obviously, he is assuming that an original *-**i**- before any consonant cluster other than *-br- < *-**zr**- < *-**sr**- would NOT lower to -**e**-, and in particular before *-**br**- < *-**dʰr**- or *-**bʰr**-. So presumably for him ***kelidʰri**- would have given

30 It is perhaps worth noting that this form (taken of course to be a suffixed formation and not a compound and so without any cogent semantics) actually is from *DELL* (I do not know its earlier history if any).

31 Nussbaum does discuss in detail the rules for vowel reduction before the dark -**ł**- but not otherwise.

†celiber.[32] But should we be granting THIS assumption (the way de Vaan and others are) in the absence of any data or argument?

It gets worse. The facts of vowel reduction are well-known, and in general, *-**i**- is assumed to stay unchanged in these contexts, both in open and closed syllables (Weiss 2021: 126). And it seems that Nussbaum also assumed that *-**i**- does not change to -**e**- in closed syllables generally. However, there is some evidence (perhaps only Quintillian's Old Latin **magester** and of course our friend **inebra**(**e**)) that sometimes in closed syllables it did.[33] Fortunately, I do not have to hang my argument on such slender reeds, because even if we stipulate ***celiber** (and ***celibr- too**) as the regularly predicted outputs, there are still ways out. And no, not just the ones already mentioned (such as a borrowing from another dialect or a random "irregularity").

There is above all a well-known phenomenon (applying inconsistently, so yet another example of my general position) known as the "ALACER rule" whereby the middle vowel may assimilate its color to the preceding vowel, hence **alacer** in place of expected **†alicer**, **sepeliō** instead of expected **†sepiliō**, and few other examples (Weiss 2021: 128f). This really should put an end to the quibbling about **†celiber**, because even if such a form HAD been inherited, the "ALACER rule" would allow us to change it to **celeber**. QED.

But what about **inebra** and **inebrae**? The various related **forms** in Old Latin, as we know from glosses (duly discussed by de Vaan, too) had -**i**- (and once even -**u**-)[34] in the middle syllable.[35] While it is often supposed that these words come from ***n̥-hab^{h}ro**-, it is

[32] Of course, this is assuming, with Nussbaum, that ***keled^{h}ri**- would have given a form beginning with **†kol**-, so this would be irrelevant anyway. But it is noteworthy that this assumption is unjustified.

[33] If this is so, then the stem found other than in the nom.sg. (**celebr**-) would have -**e**- regularly, and this could then have been generalized to the nom.sg. the same way that we (maybe) get **integer** instead of **†intiger**. Rather similarly, de Vaan claims that originally (at a purely hypothetical stage of development that nevertheless he purports to have time-machine-like insight into) the supposed medial *-**a**- of ***n̥-hab^{h}ros** had just such two different realizations: (1) as a high vowel (-**u**- or -**i**-) if the syllabie was open, so in the masc. nom. sg. **eniber**, ***enuber** < ***en-həƀros** but (2) as the medial mid-vowel -**e**- in a closed position (as in all the other forms of these words), and then "[t]he resulting alternation **enu/iber* : **enebr*- was levelled in different directions". And I say what is good for the **inebra(e)** geese should be good for the **celeber** gander. But as noted it is not at all clear that the closed syllable forms (**celebr**-) can regularly have -**e**- from *-**i**-. So I make this point only for the sake of METHODOLOGY. So too the point that automatically follows: whatever explains **integer** (and **inebra** et al. on de Vaan's etymology) will also obviously give us **celeber** from ***kelH̥-d^{h}ri**- anyway.

[34] This variation, of which there are many examples, is of course itself an example of variation within or across the dialects that result in our (ideal picture of) Latin that cannot be reduced to a single sound law.

[35] The attested forms are **enubrō** in Paulus (ex Festo) as well as **eniber**, **enibra**, **enibrum** in Pseudo-Philoxenus (the latter apparently a much older and therefore more important source than has been assumed till now, based on an unexpectedly old papyrus fragment identified by Dickey 2021).

also possible that they come from *(**e**)**n**-**ib^{h}ro**- (if the meaning of ***iebh** was not, as is generally assumed a priori 'to penetrate' but rather as I show in detail elsewhere 'to bend/ bow down'.[36] The idea of an etymon with *-**a**- is anyway doubtful, because why should Old Latin change that *-a- to a HIGH vowel? It should not. Instead on my analysis this word set is another example of *-**i**- lowering to -**e**- in later Latin before -**br**- that is precisely NOT from *-**sr**- but from *-**b^h**r-, just as I suggested above.

In any case, I do not even deny that it is possible that SOME varieties of Latin may have travelled by time machine to 1999 and, after coming back, decided to follow Nussbaum's rule for *-**i**- and *-**e**- after all, but neither this nor indeed several of the other rules could have so in EVERY kind of Latin. Just this way there are American English speakers who absolutely distinguish **roses** from **Rosa's** (and in fact have a number of distinct "reduced" vowels, as reported among others by Bloomfield),[37] while others do not make (and are astonished by) these distinctions. So, even if in our favorite variety of Latin (the perhaps slightly imaginary standard that all the theories focus on) did follow Nussbaum's rules even in those cases for which we have little or even literally no evidence, there were certainly other varieties that did not (apart from the fact that some of these rules may have been «optional»). Hence, **celeber** could perfectly well be from such a variety (even if, for the sake of domestic peace, I were to grant that there could well also have been other varieties of Latin that had, or would in time have had, if they had survived, either **†celiber or †coleber** instead). But of course there is no evidence for any such complication being even required.

---

[36] This is not the place to go on about this at length, but all the meanings of this root normally discussed are immediately derivable from the original sense of *'to bend/bow down' (the way one did to first enter a residence, and then a second time to enter the women's quarters in the back, through a low/narrow passage), so that notably the sexual sense that has so totally constrained the semantic space one is allowed to discuss (as I have learned over more than 20 years of attempts to discuss this) is derived from that of *'go to the bedroom'. On the other hand, taking the obsessive and completely mechanical reconstruction of the proto-meaning as *'to penetrate' will not explain several OTHER meanings of words derived from this root, notably Old Indic words for elephants and vassal kings — both of them notable for precisely bowing down and not so much for penetration. This is a perfect example, out of literally thousands, of how my approach to historical semantics and pragmatics differs from the dominant one. The choice between the two of course is the Reader's. I obviously lack the power to compel adherence.

[37] When I first studied linguistics I was less astonished to discover that I have the same distinctions that he did than I was that for half a century (by now a century of course) his work was either ignored or savaged. He was in particular accused of inventing phonetically non-existent distinctions based on the spelling or morphophonemic alternations (what Chao [1934 [1958: 44] ever so delicately called "strong forms [...] which are rarely heard even in deliberate speech"), when he was reporting with perfect accuracy a pronunciation that must have been real because I somehow subconsciously learned it decades later in the same part of the US) or ignored. I had no idea that this sort of thing happens in academia all the time.

And if all this sounds like a discussion of how many angels WOULD fit on the head of a pin IF there were angels determined to perch on the head of a pin at all (or as in the Jewish joke, the question of whether your brother WOULD like noodles IF you had a brother), that is exactly what it is. And it is not MY fault at all, but rather the fault of the existing system, which when convenient admits variations and uncertainties of the sort I have emphasized,[38] but when inconvenient insists on a dogma of regular sound correspondences that was never stated as anyone's theory and anyway obviously does not fit anybody's idea of the facts — while at the same time not even bothering to find data for the correspondences it claims.

There simply is no way, given the data available, to decide, with the kind of confidence that is being claimed, that ***keli-dʰri-** (or ***kele-dʰri-**, not to mention ***kelH̥-dʰri-**) would SUPPOSEDLY have given not **celeber** but only **†celiber** or **†coleber** (or the like). What we see here is the usual academic mixture of circularity and contradiction, aiming at holding **celeber** to an entirely different (and an impossible) standard than the rest of the Latin language — a standard that it seems would accept an etymology for this word in only one of two cases: (1) if we had a time machine and could go back and check, though I fear that even this would not be accepted if the results contradicted what the scholars (or the particular faction) want to hear, or (2) if someone quite unlike me had the gumption to come up with an etymology that those scholars (or that faction) decided to be enthusiastic about.

We are in general then dealing with what I call OCKHAM'S HATCHET or OCKHAM'S BLUDGEON, a very common methodology in this field (and not just this one) where a totally invalid criterion is adopted for use only to bury (here preventively) results that for some reason someone wishes to bury. Now, I cannot know for sure that my etymology will be treated this way. I predict that it will with a high degree of confidence, but of course I would not be doing the writing I do if I did not believe that there is a tiny chance (perhaps long after I am dead) that the validity of this — and hundreds or thousands of other — results of this kind, and of this overall approach to doing science will be recognized by at most one scholar in some one small field of scholarship (or outside of it). And, if I am proven wrong, and it is by many and while I am still here, all the better. So go ahead prove me wrong and get busy tearing down those walls.

And while I am waiting for that to happen, I will continue with the work that the IEnists and others so graciously left to me. There is quite a lot of this because the self-

[38] I would add that Nussbaum never rules out in principle the possibility of "simply irregular" reflexes. On the contrary, he mentions just this possibility using these very words in the same article in the discussion of vowel reduction itself (p. 410 n. 50), thus proving that he does not in principle exclude such. And how could he, when as we said Latin (and surely every language he ever studied) is chock-full of such? And so why should **celeber**, just when I find the etymology of it, not be one of these (even though it actually is not)?

designated etymological dictionaries leave many more words unexplained that obviously CAN be explained. Thus, it soon struck me that there are ALSO words that refer to various ways of inflicting (relatively) superficial damage by removing or injuring the skin of a person, the bark of a tree, etc. Most of them though require more work, regarding either the suffixes or the postpounds they end in. These include **κέλωρ** (in at least one of the three senses recorded by Hesychius)[39] 'eunuch';[40] Celtic ***klamo**- 'sick, suffering from leprosy' (Matasović 2009: 206) and Greek **κελεφός** 'leprous' (Beekes 2010: 669), assuming we can make sense of each of these words as a compound directly referencing an affliction attacking the SURFACE of a human body;[41] **κέλῡφος** [n.] 'husk or skin of fruit, skin of an onion, eggshell' (Beekes 2010: 670f.), where again I have not yet fully identified the postpound, but I do not see why we should be thinking of a Pre-Greek "suffix" at all, and others.[42] And then there are words referring to the surface layer itself, presumably etymologically qua something that is (commonly) removed or injured, e.g., Latin **cōleī** just mentioned and Greek **κέλευθος** 'road, path, course; journey' (Beekes 2010: 668f.), which need no longer "remain without etymology" if it referred to the damage done to the earth when making a path (which to prehistoric people must have been rather more obvious than to us), though I do not yet know for sure what to make of the postpound.[43] Most or all of these words require more work, obviously. But some do not:

---

[39] The sense of 'son, descendant' is immediately explained if it originally referred to a new growth sprouting or budding (indeed, likely originally of a plant, like English **scion**). The third sense is 'voice', and here I have no non-trivial explanation to offer, unless this is related to the semantics of Latin **celeber** somehow (perhaps as referring to a sound that barely reaches a crowd or the like).

[40] This could reflect the idea was that castration involves injuring a relatively superficial part of the body (which if you think about it, is so), but perhaps more likely if the word did not refer to castration at all but to another technique for achieving sterility that involved an much more superficial injury. There are two twists here. One is that there are methods for removing the testicles from the scrotum, without cutting either the scrotum or testicles themselves off, that suffice to make the latter non-functional and eventually to degenerate. The other twist, perhaps related to the first, is that Lat. **cōleī** (pl.) 'scrotum', without a plausible etymology as per de Vaan (2009: 124) could be immediately related (presumably as vṛddhi).

[41] Surprisingly, we may be also able to explain Latin **clam** 'secretly' this way as well — if this originally meant something like *'barely seen' and only later 'unseen → hidden'. But this can be trivially "explained". It is the Celtic word that is the real test, where the rubber meets the road.

[42] At the eleventh hour I learned that Merritt (2021) has an analysis of this word, operating with the previously (conventionally) recognized roots and root meanings. I found out about this work too late to take it into account, but in any case it would now have to be re-thought to see whether, as I conjecture, it really should not be re-done so as to involve the "new" meaning I discovered.

[43] I speculate that it was ***-udʰ**, referring to the ground as *'carrier'. On the other hand, it might perhaps be that ***kele-udʰ**- meant *'just starting to carry', which could imply an original sense of something like that of itinerary, then extended to the actual path traversed or the traversal (the

6. Greek **κελεός** ‘green woodpecker’ (which need not be Pre-Greek anymore, pace Beekes 2010: 668), since we know what these birds do.

7. Greek **κολεόν** ‘sheath, scabbard’ (Beekes 2010: 735), which is self-explanatory.

8. Greek **κόλον** ‘colon’ (Beekes 2010: 739), re which I need only say that the name would be alluding to prolapse (the protrusion of part of the colon), which is discussed in some detail below.

This naturally brings me to another easy example (one of hundreds or thousands I could report on) of the same issue of circular reasoning, though perhaps rather more obvious (even to non-specialists), which is why I conclude with this, namely with Greek μολο- and κολόβριον.[44] Regarding the etymology of **μολοβρός**, a “scornful or ignominious qualification, referring to Odysseus, who has not yet been identified […]”, with the derivates μολόβριον and μολοβρίτης, both referring to young pigs, Beekes (2010: 963) rejects Neumann’s (1992) etymology as ‘dirt eater (*Dreckfresser*)’, which (and I cannot stress this enough) is absolutely perfect formally as well as semantically and pragmatically, a model etymology in fact. That of course is one thing. But Beekes actually without any compunction admits that he is rejecting it just BECAUSE this etymology “does not explain the second member[45] -βρος» because ON BEEKES’ VIEW of IE and Greek phonology (NOT ON NEUMANN’S!) “-g^{w}*r̥*$H_{3}o$-would have given *-βαρο- (I do not accept the loss of laryngeals in compounds)”. Such an explicit admission is another thing entirely, and a capital one, from one of the leaders of a field that thus it is not ME saying runs (some of the time, not always!) on circular reasoning.

journey) itself. In any case, the prepound is different, but the postpound related, to those in Slavic ***kolovozъ** ‘rut; August’, presumably from *‘one that carries wheels; the time when wheel-carrying [i.e., rutting] occurs’ (and obviously not from *‘wheel cart’, as seemingly implied by Trubachev (1983: 150), which would make no sense. It is thus a compound parallel to ***kolěja**, a formation basically left unexplained by Trubachev 1983: 131), who seems to take it for granted that it is NOT a compound.

[44] Many thanks to Brent Vine for comments that I shamelessly incorporate into the argument. First, then, the word is Mycenean, an old formation that forms part of a pattern of “negative naming”, another brilliant example of which is Mycenean ku-mo-no-so = /gumn-orsos/ “Naked-ass” (Neumann 1999), which would be difficult to dismiss as non-native “Pre-Greek” because its Greek etymology is even more transparent. On a separate note, I simply cannot understand that some (e.g. Hawkins 2013: 115) do not see that Chantraine’s (1972) attempt at etymologizing **μολοβρός** is NOT a viable alternative to Neumann’s, just to begin with because it is not funny and does not make sense of the epithet applied to Odysseus. The great value of Chantraine’s proposal seems to me METHODOLOGICAL: the very fact that Beekes cites Neumann’s but not this one suggests that in spite of his aprioristic refusal to accept the latter, he did realize that it IS a very good etymology. It is between the lines and out of the box that we must read.

[45] What I call a POSTPOUND.

Let us be clear: the problem is not that Beekes did not accept the theory of loss of laryngeals in compounds — any more than it was a problem in 1940 that Benzing could not make himself accept even a possibility of zetacism. The problem is that he ASSUMES that one theory is incorrect and another incorrect and then uses this very assumption to reject, as a matter of principle, any possible evidence that could be presented to show that his is wrong and the other one right. What would have been the right thing for him to say? It would have been to say this:[46]

> Till now, based on such data and arguments that I have seen/heard and/or come with myself, I have rejected this theory. Now that I see a new piece of evidence for the theory and against my view, I am obligated to review the entire dossier again to see whether this new piece of evidence is enough to show me that I have been wrong.

And then to ACT accordingly, which would mean:

1. to weight the evidence fairly, i.e., not just absolutely even-handedly (which is nearly impossible for a human being) but rather always (which is much easier) giving the benefit of the doubt not to oneself and one's own views but precisely to the other side, its evidence and its arguments,
2. to be clear just what evidence (or how much of it) would be enough to prove to one that one has been been wrong (so that supposing the currently presented counter-evidence is not enough, the other side can know what it must do and also so that everyone can see whether one is not simply begging the question),
3. to consider precisely all the evidence and not to reject each individual counterexample, but above all
4. never ever under any circumstances — not even under torture or for ready money — to reject counter-evidence just BECAUSE IT IS COUNTER-EVIDENCE, that is, simply because one has assumed in advance that one must be right.

And of course Beekes does exactly the opposite on each of these points not just on this page but throughout his book and elsewhere. As does (at least at times) virtually every worker in this field, both in print, in lectures, and in discussions (e.g. at conferences I have attended and in other forums). For example, every time I have presented to someone who does not accept any of the phenomena listed below a new example thereof (which logically might shift the balance against the rejection), the example itself is rejected precisely on the grounds that the phenomenon itself is one that that scholar (and invariably an entire school behind or around him) have decided not to accept: Eichner's Law, voicing by a following *$\mathbf{h}_3$ (notably but not only in the case of the possessive "suffix" discovered

[46] I have on several occasions said or emailed these exact words to various leading IEnists — as well as Turcologist etc. So far, to no avail.

by Hoffmann and Hamp), preaspiration by a following *$\mathbf{h}_{1/2}$, "Breaking"[47] in Greek, etc. The list goes on. The phenomenon is all-pervasive, on big issues and small.[48]

Speaking of errors, Beekes, taking the word as "Rather a Pre-Greek word", seems to take as support for his conclusion the fact that there is another supposedly Pre-Greek for a young pig that ends in the same postpound: **κολόβριον**. So, if it were me, and it was me, even when for a moment of five minutes or so I could not explain what this **κολο**- would be (if the word were a compound similar to the one reconstructed by Neumann), I would have said (and did say to a few email correspondents) that on the one hand the burden of proof is gladly accepted by me to find a REASONABLE[49] native Greek etymology parallel to Neumann's — and on the other that that, if anyone could come up with an actual Pre-Greek etymology (rather than hand-waving assertions that the word «must» or «may» be Pre-Greek), then that would count as actual evidence AGAINST my position.

Having finished writing at I did not see what the prepound was at 10:51 AM, at 10:56 I found out (from a reputable veterinary source) that pigs are particularly prone to rectal prolapse, with the following three possible outcomes:

1. It rapidly returns into the anus.
2. It remains outside the anus and, due to the constrictive effect on blood and fluid drainage, it generally swells up. It is thus easily damaged by trauma on pen divisions, feeders etc.
3. It is eaten by other pigs in the pen. It is not uncommon to find blood in a pen and around the mouths of pigs but with no obvious prolapse in any other animals, i.e. the prolapse will have been completely chewed off.

---

[47] I put the term in scare quotes because it seems to me as clear as anything can be without a time machine that the supposedly "broken" realizations are the original, proto-glottalic realizations of these phonemic sequences.

[48] Years ago when I was "invited", for the first and last time, to a conference on Slavic and/or IE linguistics, the organizers were shocked that my etymology of Slavic ***čelověkъ** ~ **čьlověkъ** (as *'one whose time is (but) a while') was an "entirely new" one! For them the traditional etymology had evidently become an axion, a piece of dogma, which could only be repeated or at most slightly adjusted — but in principle never replaced.

[49] At the time I first wrote this, I added "where **kolo**- would be from a root known from Greek or other IE languages, where the form would follow the known sound laws, and where the meaning would either again be what pigs (or young pigs) distinctively consume or (since the prepound does not have to be patient/direct object of the postpound) something else that characteristically fits the way pigs (or young pigs) feed themselves, e.g. instrument ('with a snout', f.ex.) or manner ('greedily, indiscriminately')". And the prediction turned out to be confirmed within less than five minutes. To me this is science. What I report in the next footnote is the opposite.

Option 3 of course gives us the etymology of the prepound of **κολό-βριον** (and of course suggests that of the 'colon' word itself!). And Neumann and I win, and Beekes and the whole question-begging nomenklatura loses. That was never in question. The question instead is whether (a) anyone will accept this outcome[50] and (b), for those who are not members of Beekes' school, whether they will accept that this is not about Beekes or his followers (and their celebrated obsession with making obviously native words (not just in Greek!) be something else or anything at all specific to their school, but rather about the methodology of science and the fact that is routinely violated in this field, and that I really could use a break from being ignored or abused when I point this out.

And there is, as often, a bit more. Neumann concludes by noting:

> Die Übersetzung ‚Schmutzfresser, Unratverschlinger' hatte schon Ameis [1895: 114] vorgeschlagen. Curtius [...] hat sie mit unzureichended Gründen zurückgewiesen, wodruch sie in Vergessenheit geraten war. — Die Priorität gehört also Ameis.

So the circle of error and abuse keeps turning. Curtius suppresses Ameis' obviously correct etymology for a century. Neumann rediscovers it (I am guessing, independently)

[50] This is the difficult part. As soon as I showed these results to a handful of IEnists, I was informed by one, with sadistic glee, that since **κολόβριον** refers to a piglet, whereas the reference to pigs eating each other's colon tissue does not mention this as a behavior of specifically YOUNG animals, therefore I was wrong. Now, the scholars of IE know that **-ion** is a Greek diminutive, and should realize that *'colon eater' would be the meaning of the unattested ***κολοβρός** (entirely parallel to **μολοβρός**), while **κολόβρ**ION would get its specific sense of 'pigLET' from the **-ion** suffix (just like **μολὀβριον**, as was evident already to Ameis in the 19th century). Next I was informed, with more glee, that what proves me wrong is that Aristarchus referred to (someone using) these terms as being for piglets having a wild boar for a father. Apart from the fact that traditionally people did not keep intact adult male pigs but rather tied the females in estrus out where wild males could mate with them (so the father of any piglet was by definition a boar), there is also the fact that never in the history of IE linguistics did anyone deny that words for wild boar and domestic swine may (and do) interchange — not until the denial of this commonplace appeared to someone as an Ockham's Bludgeon to be used in a futile attempt to shut up the voice of one crying in this wilderness. But you'd have to get up prett-tty early.... Briefly, when people did start using domestic male pigs for procreation (presumably alongside wild ones for a considerable time), it is possible that there was a special term (or two) of particular opprobrium applied to the domestic offspring of the wild males (perhaps, though this of course cannot be known for a fact, stereotypically viewed as less picky eaters and/or as more aggressive than the ones with a domesticated sire). This is another exercise in angels on pins that I only feel forced to go into because, as I say, as soon as I showed my results to just a handful of Indo-Europeanists, I was immediately attacked on utterly trivial points that do not bear on the proposals and the like of which is not a standard to which the work in that field is usually held (except precisely for any work that for some reason some group decides it needs to try to suppress).

only to have his work trashed by Beekes, who of course has the enormous advantage of being the author of a reference work and moreover one published in a world language and so bound to be much more widely relied on than a journal article by Neumann written in a former world language. I rediscover Neumann and provide new and decisive evidence to support Neumann, and what happens? That remains to be seen for sure, though the experience of decades of my life (and what I can read of human experience since the invention of writing and a little before) does not inspire immediate-term optimism. Maybe a little later in the year?

In any case, though, the issue is not this or that etymology, whether in IE or elsewhere (although of course one common trick to avoid dealing with the general issues is to try to focus on one particular example at a time and avoid the issue itself). For those interested in other Northwest Eurasian language families besides IE (but necessarily including IE if we are concerned with the perennial Nostratic question or with the issue of borrowings to or from IE),[51] do you now see what I have tried to show you for decades, that you en-

[51] Here I would like to be permitted to report that I believe I may have located in IE the mysterious source of at least some of the set of Turkic words identified by Vovin (2004, 2010) as neither native nor yet borrowed from any of the obvious suspects. Thus, ***dwant** 'horse' (usually taken as ***yunt**, ignoring the oldest Lir, Danube Bulgar **dvan**-, a year name that has to be either horse or rabbit, and **dvalma** 'groom') surely looks immediately IE (literally *'runner'). Since there is only one other word in Turkic ending in -**nt** (identified by Stefan Georg *apud* Vovin), namely, **ant** 'oath', I submit that this also an IE **nt**-participle, this time from the root conventionally reconstructed as √***$h_2$emh$_3$** 'anfassen, anpacken' ⟶ 'schwören' (*LIV*^{2} 265–266), which I take to have actually meant *'to grab with both hands', which surely was a gesture involved also taking an oath. The difficulty is that the rather small set of words seem to come, if the IE connection is not just an artifact of chance, from at least two different languages, mostly Iranian, but also it seems some Tocharian. This makes the probabilistic argument (that all this is not just noise in the data) much weaker. On the other hand, the fact that several of the 12 animal names used to name years are from such a source is probabilistically very good, though again a short word like **ud** 'ox' is not very significant. On the other hand, **küskü** 'rat', which I hope (but am not sure) is ***guz-ka- or** ***guz-kiya-** or the like, i.e., 'little hider', and especially **lagzïn** 'pig' perhaps are. The latter would be the correct Scythian reflex of Iranian ***ada-gžan-** vel sim. 'food destroyer', which corresponds exactly to the significance of the wild boar for traditional human societies. The case becomes rather stronger if we can explain the Turkic imperial titles. The feminine **khatun**, for decades asserted to be Sogdian, cannot be that of course (1966: 30-33), but can be from a hitherto all-but-unknown Iranian language (***hwa-tāwān-** vel sim. *'one CONNECTED TO the *auto-crat*'). If I can (and I think I can) also explain the male titles **khagan** and **khān** as well, as *'*auto-crat*' (***hwa-tuwān**- vel sim.), the case will be even better. On the other hand, Turkic **kaŋ** 'father', which has bothered me for many years, now seems to me only compatible with a Tocharian-like reflex of PIE ***ǵonós** 'progenitor' or maybe better a diminutive ***ǵonkos**. I would hope that, with maybe just one or two more such words, the IE character of all this will be harder to dismiss than it may seem today (though of course sociologically easier). All

gage in the same kind of circularity and contradiction as the Indo-Europeanists, and that there is a very simple way out of all this? And that the answers that have so long been elusive (in large measure because too many scholars claimed to have them long before they had anything and so just confused the issues) are now within reach?[52]

All I know is what I have witnessed for the half-century or so that I have been involved in scholarship, and what I have read going back as far as the written records reach. Everyone I know seems to find what I have decried (and shown the easy alternative to) to be the normal and proper conduct of science and human relations. I do not. I will never accept any of this as either science or humanity. I have of course said this many times before, eliciting at best just supercilious smiles of pity. And yes it is a pity, a very great pity indeed. But there could be better times ahead. There are literally thousands of new small (and big!) results just waiting to be picked off — provided anyone in any of these (or other) fields is willing to follow the scientific method and give up the circularities and contradictions that play such a prominent role today. And once more: there are already known to me two or more dozens of other hitherto unexplained words (and secondary roots, or THIEMES, taken as roots in the handbooks and dictionaries) derived from the root under discussion here; I just do not have the time or space to so much as list them here. But of course these etymologies are not the big game we really want anyway.

---

this I hope to discuss elsewhere. The reason for mentioning this here is to show in yet another way how PRODUCTIVE the approach I have been advocating it can be — in contrast to the conventionally prevalent one. Of course, some part of these results may prove wrong, but first actual work has to be done — rather than suppressed.

[52] This is just one example of dozens I could cite of where historical linguistics assume all manner of details totally ahead of the available data, and of course whatever they assume is taken to be an incontrovertible fact (and in particular used to rule out any other facts that might contradict this one) — until the wind changes. In a field closer to home, can we not all at least agree, after all the wasted decades, that the obsessive repetition of the claim that in the Lir languages Proto-Turkic *-**z**- and *-**d**- both changed to -**r**- at the same time is an example of just this totally invalid methodology. Even if we did not know that this is cannot have been so (as shown by borrowings into Hungarian and in Mongolic, in addition to the sparse but sufficient sources for early Lir), no one should ever have permitted themselves in the first place to make any such assumption, first proclaimed by Benzing (1940, 1944) and yet with its impossible consequences still eagerly embraced even as recently as Georg (2003) and since. But the point again is not so much that this particular pair of sound changes in Lir languages cannot be the way Benzing, Georg, and others have wanted and insisted. The problem is rather the very idea of adults WANTING and INSISTING on this or that result in the first place, and on this or that "rule" cut from whole cloth so as to get that pre-determined result. No, I am wrong: that is not the problem. The problem is an entire field of scholarship (like all the others alongside it) cheerfully encouraging this to go on — and when not this, then a dozen or a hundred other (but exactly analogous) abuses. Yes, THAT is the problem. And the solution I have shown you, yet again.

Bibliography

Ameis, Karl F. 1895. *Anhang zu Homers Odyssee*, Schulausgabe, III (Erläuterungen zu Gesang XIII–XVIII), 3rd ed. (C. Hentze, ed.). Leipzig: Teubner.

Bader, Françoise 1983. De l'« auscultation » à la « célébrité » en latin: formes de la racine *kel-. In: *Hommages à Jean Cousin: rencontres avec l'Antiquité classique*. Paris: Belles lettres, 27-60.

Beekes, Robert S. P. 2010. *Etymological Dictionary of Greek*. Leiden, Boston: Brill.

Benveniste, Émile 1966. *Titres et noms propres en iranien ancien*. Paris: Klincksieck.

Benzing, Johannes 1940. Tschuwaschische Forschungen II: Tschuwaschisch r // alttürkisch đ. *Zeitschrift der Deutschen Morgenländischen Gesellschaft* 94: 391 — 398.

Benzing, Johannes 1944. Die angeblichen bolgartürkischen Lehnwörter im Ungarischen. *Zeitschrift der Deutschen Morgenländischen Gesellschaft* 98: 24-27.

Chantraine, Pierre 1972. Le témoinage du mycénien pour l'étymologie grecque: δαΐ, Κοπρεύς, Κυκλεύς, μολοβρός, μόλυβδος. *Minos* 12: 197-206.

Chao, Y. R. 1934. The non-uniqueness of phonemic solutions of phonetic systems. *Bulletin of the Institute of History and Philology*, Academia Sinica 4, 4: 363-97. Reprinted (1958) in Martin Joos (ed.) Readings in Linguistics: *The Development of Descriptive Linguistics in America since* 1925. New York: American Council of Learned Societies, 38-54.

De Vaan, Michiel 2008. *Etymological Dictionary Of Latin*. Leiden, Boston: Brill.

DELL = Ernout, Alfred & Antoine Meillet 1985. *Dictionnaire étymologique de la langue latine. Histoire des mots*. 4th ed. Paris: Klincksieck.

Dickey, E. 2021. The history of bilingual dictionaries reconsidered: an ancient fragment related to pseudo-Philoxenus (P.Vars. 6) and its significance. *Classical Quarterly* 71, 1: 359-378.

EWA = Mayrhofer, Manfred 1986–2001. *Etymologisches Wörterbuch des Altindoarischen*. Heidelberg: Winter.

Georg, Stefan 1999. Haupt und Glieder der altaischen Hypothese: die Körperteilbezeichnungen im Türkischen, Mongolischen und Tungusischen. *Ural-altaische Jahrbücher, N.F.* Bd 16: 143–182.

Georg, Stefan 2003. Japanese, the Altaic theory, and the limits of language classification In: Alexander Vovin & Osada Toshiki (長田俊樹) (eds.) 日本語系統論の現在 (Perspectives on the Origins of the Japanese Language). Nichibunken sōsho, 31. Kyoto: International Research Center for Japanese Studies, 429-448.

Hawkins, Shane 2013. *Studies in the Language of Hipponax*. Bremen: Hempen.

Ioannidis, John P. A. 2005. Why most published research findings are false. *PLoS Med* Aug;2(8):e124. doi: 10.1371/journal.pmed.0020124.

Johnson, George 1995. Linguists debating deepest roots of language. *The New York Times*, June 27, Section C, Page 1.

LIV^2 = Rix, Helmut (ed.): *Lexikon der indogermanischen Verben*^{2}. Wiesbaden: Reichert.

Manaster Ramer, Alexis 1994. Clusters or affricates in Kartvelian and Nostratic? *Diachronica* 11: 157–170.

Manaster Ramer, Alexis 1996. Armenian -k` < PIE *-(e)s. *Journal of Indo-European Studies* 24: 361–398.

Manaster Ramer, Alexis 2020a. It's not the mortality rate, stupid! *Reumatologia/Rheumatology* 58, 2: 63–66. https://doi.org/10.5114/reum.2020.95357.

Manaster Ramer, Alexis 2020b. Whey to go: Slavic **kъsьnъ** and the roots *KUḰ and *KWAHT in Slavic and beyond. In: Martin Henzelmann (ed.): *Sprachwissenschaftliche Perspektiven der Bul-*

garistik: Standpunkte — Innovationen — Herausforderungen (Festschrift für Prof. Dr. Dr. H.c. Helmut Wilhelm Schaller anlässlich seines 80. Geburtstags). Berlin: Frank & Timme, 79–124.

Manaster Ramer, Alexis 2021a. Boris Parashkevov on professorial etymologies, especially those of кѐстен 'chestnut' плѝндер 'bladder (the air-filled inside part of a ball)'. *Bulgarica* 4: 167–172.

Manaster Ramer, Alexis 2021b. Crying shame, or old whine in old bottles: Ps. 56: 8. In: Irina Nevskaya et al. (eds.): *Ayagka Tegimlig Bahşı: Festschrift in Honor of Marcel Erdal.* Türklük bilgisi araştirmalari / Journal of Turkish studies, Special ed. 1: 325–330.

Manaster Ramer, Alexis, Peter A. Michalove, Karen S. Baertsch, Karen L. Adams 1998. Exploring the Nostratic hypothesis. In: Joseph C. Salmons and Brian D. Joseph (eds.): *The Nostratic theory: Sifting the evidence.* Amsterdam, Philadelphia: Benjamins, 61–84.

Manaster Ramer, Alexis, Paul Sidwell 1996. The Altaic debate and the question of cognate numerals. *Wiener Zeitschrift für die Kunde des Morgenlandes* 87: 153–175.

Manaster Ramer, Alexis, Alexander Vovin, Paul Sidwell 1997. On body part terms as evidence in favor of the Altaic hypothesis. *Ural-altaische Jahrbücher N.F.* 15: 116–138.

Matasović, Ranko 2009. *Etymological Dictionary of Proto-Celtic.* Leiden, Boston: Brill.

Merritt, A. 2021. 'κέλῡφος and καλύπτω'. *Indogermanische Forschungen* 126: 305–324.

Neumann, Günter 1992. Griechisch μολοβρός. *Historische Sprachforschung / Historical Linguistics* 105: 75–80.

Neumann, Günter 1999. Zwei mykenische Personennamen. In: J. Habisreitinger [et al.] (eds.): *Gering und doch von Herzen. 25 indogermanistische Beiträge Bernhard Forssman zum 65. Geburtstag.* Wiesbaden: Reichert, 201–205.

Nussbaum, Alan 1999. *Jocidus: An account of the Latin adjectives in -idus. In: Heinz Eichner [et al.] (eds.): *Compositiones Indogermanicae in memoriam Jochem Schindler.* Praha: Enigma, 377–419.

Street, John 1980. Proto-Altaic *-l(V)b- > Turkic Š. *Central Asiatic Journal* 24, 3/4: 285–303.

Trifonova, Krasimira Georgieva, Alexis Manaster Ramer 2019. Four myths for visitors to Bulgaria's Black Sea Coast. In: Genka Rafailova, Stoyan Marinov (eds.): *Tourism and intercultural communication and innovations.* Newcastle: Cambridge Scholars, 198–213.

Trubachev / Трубачев, О. Н. 1983. *Этимологический словарь славянских языков, 10 (*klepačь — *konь).* Москва: Наука.

Vovin, Alexander 2004. Some thoughts on the origin of the old Turkic 12-year animal cycle. *Central Asiatic Journal* 48, 1: 118–132.

Vovin, Alexander 2007. Once again on the etymology of the title qaγan. *Studia Etymologica Cracoviensia* 12: 177–187.

Vovin, Alexander 2010. Once Again on the Ruan-ruan Language. In: Mehmet Ölmez [et al.] (eds.): *Ötüken'den İstanbul'a Türkçenin 1290 Yılı (720–2010), 3–5 Aralık 2010, İstanbul, Bildiriler / From Ötüken to Istanbul, 1290 Years of Turkish (720–2010), 3–5 December 2010, İstanbul, Papers.* İstanbul: İstanbul Büyükşehir Belediyesi, 1–10.

Weiss, Michael 2021. *Outline of the Historical and Comparative Grammar of Latin.* 2nd ed., 2nd corrected printing. Ann Arbor, New York: Beech Stave Press.

White, Mark 2009. Rectal prolapse and rectal stricture: Pig veterinarian, Mark White, explains the causes, identification and treatment of these conditions in the July 2009 Health Bulletin from NADIS. The Pig Site, 28 July 2009, https://www.thepigsite.com/articles/rectal-prolapse-and-rectal-stricture [accessed 17 September 2022].