



Ollscoil
Mhá Nuad
Ollscoil na hÉireann
Má Nuad



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Proceedings of the Thurneysen Fanclub: issue 29

Records of the discussions in the ChronHib Bar on 24-01-2018

In attendance: David Stifter (chair), Fangzhe Qiu, Bernhard Bauer, Elliott Lash, Siobhán Barrett, Romanas Bulatovas, Lars Nooij (scribe)

Apologies: Theodorus Fransen, Nicole Volmering

Practicalities

We have only a very limited supply of cookies. Angry shouts of 'blame the suppliers!' resound. This was clearly effective, as David promptly bought an ample supply of new cookies afterwards. As to the meetings starting next week, they will probably take place on Tuesdays so that it will not clash with matters at TCD, although the time has yet to be settled.

We bid Elliott welcome upon his return and congratulate him once more upon his wedding. He confirms that he definitely attended his own wedding and that it was enjoyable. We were messing with him last week.¹ We also welcome Siobhán, who has joined us again for the first time in a while.

David had not noticed Cormac Anderson's comments on academia.edu ahead of the meeting. We will discuss them next week, after having had the opportunity to study them more closely.

a-glides (§87)

"Before neutral consonants no glide is indicated in O.Ir. (for the development $\acute{e} > ía$, see § 53)." We would now say non-palatal consonants. The development $\acute{e} > ía$ reflects a specific breaking of that vowel into a diphthong, rather than the insertion of a glide vowel – this might be stated more clearly. Otherwise, Thurneysen is correct: there are almost no examples of non-palatal glides being written during the Old Irish period.

¹ Shortly hereafter, Elliott was chatting with Bernhard whilst David was speaking, prompting the latter to exclaim: "Simply because you're married doesn't mean you can chat!" Upon which Elliott replied: "We were just trying to solve things. We were discussing secret things."

“The *a* that appears after *ē* in Mid.Ir. is found in Ml. once in a stressed syllable: **con·ru·sleachta** ‘so that they have been slaughtered’ 53^d11...” What to make of this? It certainly looks like the later glide, but does this represent a phonetic vowel? Or is there some consonantal effect before *ch*?

“(it occurs three times, however, in unstressed syllables: **coíneas** ‘who weeps’ 102^a23, **aipleat** ‘let them die’ 104^b2, **·erladaigear** 1 sg. subj. ‘I may obey’ 106^c6).” There is a further example in the Milan glosses (Ml. 3a9), *co·fotheasa* a form of the verb *fo·teinn*, which Aaron Griffith described as ‘very unclear’ in his database. He takes the *co* as the nasalizing *co*, since the lenition of the *t* indicates that it is compounded and *-easa* must therefore be in an unstressed syllable. Perhaps it stands for *·thés-sa* or *·thías-sa* (cf. dil.ie/24148).

The latter example occurs towards the very beginning of the manuscript. The three other examples all occur close to each other, towards the end of the manuscript. It is again uncertain what to make of them. The manuscript was copied in the early 9th century and the glosses appear to be some 30 or 40 years older than that. Whatever these spellings imply, they are certainly rare, even in Milan.

Before continuing, we briefly return to last week’s *i*-infection hypothesis. Take the example of the genitive sg. of the word for ‘son’ *maicc*, which goes back to **makkī*. According to the *i*-infection hypothesis, there would have been an intermediate stage where the final *-i*, before being lost due to apocope, not only causes palatalisation, but also the infection of an *i*, or *j* in the first syllable, i.e. **maⁱk’k’*, which may have been retained into Old Irish and the later language as a diphthong of some kind, but manifestly different from inherited /aj/. This would be parallel to *u*-infection.

In Modern Irish, words like *dáil* contain something more akin to a diphthong than a pure *a* followed by a fully palatal consonant [nota bene: this is our aural impression as non-native speakers]. The proposed *i*-infection might explain this phonetic reality. However, one can also envisage this arising out of a later, allophonic vowel variation before specific consonants. As such, the hypothesis would need to be tested. A good way to go about that would be to start with one text, preferably one that is as early as possible (to limit the potential interference from analogy as much as possible), e.g. the Cambrai Homily, and see whether the orthography accords with such a hypothesis. It would be important to take the surrounding consonants into account, as different consonants might have an influence on the outcome. It is also important to note that the orthography was decidedly unstable in the early period, which might come into play in such an investigation. Finally, it would be good to compare all relevant examples with examples of *u*-infection: do they show similar developments, or not? We leave the matter for now.

This all leads to the question of whether there might then also have been an *a*-infection? That is to say, would there have been something like **uirah* > **uerah* > **year* > <fer> /fe^ar/ > <fear> /f^a ar/. In other words, does the Modern Irish pronunciation reflect the presence of a diphthong, or a phonetically real glide vowel of some kind already at the Old Irish stage? Probably not. Unlike the *i*-glide, spellings of *a*-glides in Old Irish are extremely rare, apart from the bare few examples in the Milan glosses.

It is noted that if one accepts that the later phonetic reality of *a*-glides arose only in the later language and quite independent of any prehistoric vowel infection, this offers a parallel for the development of phonetically real *i*-glide vowels at a post-Old Irish stage as well.

It is wondered whether there are any *a*-glides in the *Vita Columbae*, the text of which was originally written c. 695 AD and the earliest manuscript copy of which (the Schaffhausen manuscript) was copied very shortly thereafter, before 709. However, it seems that most of the examples that look relevant on first sight are actually examples of the breaking of *é* to *éa* (later: *ía*), which has nothing to do with *a*-

glides. There is the placename *Ceate*, for later *Cette* as well, but this is unhelpful as the origin of this name is unclear.

Are there any more examples in Milan? Elliott seems to recall that there are notes in the *Thesaurus*, stating that there are forms which were written with an *a*-glide in the manuscript, but with a *punctum delens* over the *a*, which were not included in the edited text (on which Aaron Griffith's database is primarily based). This would potentially be very interesting.

Turning to the German, we note that Thurneysen did actually include the breaking of *é > ía* as containing an *a*-glide at that time, which is incorrect, but does explain why he bothered mentioning it in the GOI. Other than that, the text is the same, apart from the fact that he offered one example less of an *a*-glide spelling in an unstressed syllable.

David mentions that he recently investigated five Middle Irish glosses in a Vienna manuscript by Marianus Scottus for an article in *Peritia*. The manuscript is very long, but has only very few glosses. Only one of them is a gloss on the text, the other four are requests to pray for the poor soul of the author, but these four contain very precise dates. This allows one to trace his copying speed fairly accurately. At first, he copied just a few pages a day, but after about a month he sped up dramatically, copying about 18 pages a day! This all goes to show that it might have taken a very long time to copy a manuscript like that of the Milan glosses and it is not unreasonable to expect that a scribe might not be entirely consistent in his spellings throughout.

David argues in jest that the scribe might have been drunk when he added those three words with *a*-glides towards the end of the Milan manuscript. However, although there are indeed other manuscripts famous for including glosses stating that the scribe has a hangover, this does not then seem to adversely affect their ability to write – in fact, in one famous instance the scribe took the trouble to carefully note his drunken state in *ogam*. Moreover, one wonders why a drunken scribe would revert to a type of orthography completely unknown to him, unless he was truly randomly scribbling down things. Perhaps he had just read a manuscript in which *a*-glides were used more often when he copied the last section of the Milan manuscript?²

“*ei* for *e* before neutral consonants is peculiar and very rare; e.g. **feir** Wb. 13^a20, 22^c10, acc. sg. and gen. pl. of **fer** ‘man’; **teicht** ‘going’ Thes. II. 296, 3.” Such spellings are indeed very rare and peculiar.

We now turn to the *-iar* passive endings which Elliott had raised earlier, but which had been postponed. These endings are found for example in *Amra Choluim Chille*. Jasanoff built a Proto-Indo-European morphological argument on the basis of these spellings. However, at that time it was believed that this text was very early (6th century). Nowadays, after Bisagni's study of the text, this is no longer the case. As such, one must be careful not to base too much on this one text in particular.

Attention is brought to spellings such as *Cassial*, *foídiam*, *Luigthiach* and *Leathain*, where the “*a*-glide” is entirely unexpected. David mentions that in Blathmac, which is transmitted in a very late manuscript and therefore typically has a very modern orthography, the ending *-set* is regularly written *-siot*. Such spellings in *-ia-* may simply be artificial, ‘fancy’, ‘archaic’-looking spellings and are not informative.

² David adds that of late he has been reading Maigret crime novels in his spare time and that he has noted that the policemen are drinking astonishingly much in these books, both on the job and afterwards – assuming that this literary fiction reflects the real-life practice of the time! Perhaps Irish scribes were similar?

In the Turin Gospel Commentary (c. 8th century? similar date to the Würzburg glosses?) there is the form *immerumediari*, from the verb *imm·ruimdethar*. It should be a 3sg. deponent pret. ending and should end in a palatal consonant (the *-e* after *imm-* expresses relativity). We assume this is a scribal slip for *-air*. The form is weird in other ways as well, as it restores an unstressed syncopated syllable (*-med-*; if it were stressed and original it would have been *-míd-*). Perhaps a syncopated long vowel was more readily restored than a short vowel? At any rate, it shows that the verb was been modified as part of the simplification of the verbal system.³ In general, David states that the *-iar* endings should at this stage be considered artificial, rather than anything old.

***u*-glides (§88)**

“Before *u*-quality consonants *u* is inserted after *ǣ ǣ ǣ* under the same conditions as *i* before palatal consonance.” Thurneysen is talking about phonetics again. As to his statement, this needs to be checked; the matter might be far less clear-cut than he makes it out to be.

“Between *e* and *ch* it is sometimes replaced by *o*.” Might there be a chronological factor to this?

Thurneysen clearly links *u*-quality strongly to the *i*-glides, but it should be noted that “*u*-glides” are nowhere near as consistently written as the *i*-glide is.

For Thurneysen inherent *u*-quality was real, e.g. one would have three different *k*’s:

k *k^j* *k^u*

The second, *k^j* arose out of being followed by a palatalising vowel. The question is, how did the third, *k^u*, arise? It certainly did not arise out of inherited Celtic **k^u*, which was lost during Primitive Irish (moreover, if it had arisen out of that, this would not explain the *u*-quality of most other consonants, given that they had no such preform). Rather, this third quality arose out of apocopated **u*, e.g. **makkū* > *mak^uk^u* which one might expect to have been written ***maucc*, but which is never attested as such (we rather find *macc*). This is actually also the strongest argument against any full-scale *u*-quality: it is not consistently found in all the contexts liable to give rise to it, i.e. it often fails to turn up, even when a **u* had been apocopated. Given the nature of our example, one wonders whether geminates are resistant to *u*-quality; however, **katus* ‘battle’ should then give ***cauth*, but again, such a form is not found.

The alternative view, that *u*-quality did not exist, but that *u*-infection (which essentially arises out of the same condition, namely that a **u* had been apocopated) occurred is more plausible, simply because it is more acceptable for a vowel infection to fail to occur – or to be soon simplified/reduced – than it is for a full-fledged consonant quality category to be that haphazard. If *u*-quality did exist on some marginal level, it was certainly not on the same level with the two other consonant quality classes, as there are too many restrictions to its occurrence. Essentially, we would expect a consonant quality class to arise out of allophones after the loss of the conditioning vowels and then be inherent and (more or less) unconditionally present in all affected consonants. The evidence does not seem to bear this out. The many restrictions would rather point to them being allophones.

³ Elliott: “Here is one [referring to another example]! Or no, I think it’s not a good one either.” And indeed, it was an example of analogy.

Cormac Anderson, whose comments we will go into at next week's meeting, champions the three-series system. His explanations may work descriptively in a synchronic sense, for a set corpus at least. However, to our understanding it does not seem to combine well with reconstructions and therefore does not explain the material diachronically. Speaking more broadly, we do not need *u*-quality diachronically. Instead, the attested material can be explained by means of a combination of *u*-infection, analogy and the natural, phonetic influence of various types of consonants.

The question is briefly raised whether there might be a parallel of some sort between the restrictions on the raising of *o* and the restrictions on *u*-infection. Is there? Is it significant?

A further argument against the existence of a full-fledged *u*-quality consonant category is that whereas the distinction between palatal and non-palatal is morphologically very important, the velarised consonants are marginal at best in this respect. If it were on a par with the other two consonant qualities, one would expect it to have been liable to be used more extensively to mark morphological distinctions. Of course, Thurneysen would probably argue that its significance had been reduced by waves of the loss of *u*-quality, but why then insist that it was a full quality series in the first place?

It is raised that palatalisation was less well marked in early Irish orthography than later on, which could imply that palatalisation was less important at that stage – perhaps it only really took off once the vowel system was further reduced in the following centuries? However, it could also be argued that since the script developed slowly out of a British-Latin orthographical base, which did not mark palatalisation at all, it simply took the Irish some time to adapt it to the needs of their own language.

“If the *u*-quality consonant belongs to the following syllable the glide is usually omitted...” Again, Thurneysen argues for a parallel with *i*-quality. However, there are too many exceptions to this ‘rule’ to pronounce it with any authority. For example, *fiugor* also occurs in the Stowe Missal (although *figor* is the more common form in that source as well).

It is considered whether there are any other examples of *-ou-*, aside from *routh*. Suggestions include *at-choüs*, *loür* and *roüt*, which all have hiatus and are therefore irrelevant. The same probably goes for *foünn* ‘under us’ and perhaps for *souñd*. The word *aithg[e]ouin* is also irrelevant. Attestations of the diphthong *-ou-* are certainly very rare indeed.

“In Mid.Ir. *eo* is written for *ē* in open syllables before *ch* and *γ* where these consonants had *u*-quality in O.Ir...” But why are these consonants actually supposed to have *u*-quality?⁴ Where did they get it from? The example of *do-deochuid* is problematic as this derives from **de-ko(m)u*... Could **o* trigger *u*-quality? Or might it be caused by the **u* in this particular case? When did Thurneysen actually believe that *u*-quality arose? Allowing for such further triggers only increases the already vast amount of apparent exceptions to *u*-quality. We ourselves would rather argue that the velar *-ch-* itself simply velarized the vowel secondarily. A similar example would be *deug*, *deoch*.

The example of *becc*, Mod. Ir. *beag* /b'og/ and Sc.G. *beug* is also brought up; if this is considered to have *u*-quality it is very problematic indeed, as it derives from **biggo-*, with nothing *u*-like in sight.

The same goes for Thurneysen's Mid.Ir. *ro-geoguin* < O.Ir. *·geguin*, which derives from **g^ueg^uone*.

⁴ Lars: “Because they spell it like this.”

“After long vowels this glide is never found, except that compensatorily lengthened *é* and *í* are represented by *éu*, *íu* (§§ 55, 71 b), and the other *é* by the diphthong *ía* (§ 53). Consonants in this position lost their *u*-quality at an early period.” This must have been very early if it is to account for all the exceptions.

Diachronical stuff (§89-94)

Given that David was unhappy to have only covered less than one page of the grammar at this stage, we proceed to rush through the next four pages in about fifteen minutes time. This was possible only because the following paragraphs deal almost exclusively with diachronical matters, which are irrelevant to a synchronic grammar.

Summarizing matters very briefly, §89 deals with PIE **ō* > **ū*, §90 discusses the development of PIE **ē*, §91 discusses what happens to word-final vowels, §92 discusses what happens to short vowels followed by a consonant in word-final position, §93 deals with long vowels in the same context and §94 mentions PIE **i̯V-* and **i̯V-* become **i̯V-*, although it argues the latter rather weirdly. Thurneysen claims that the **u* is retained e.g. in a sequence **i̯i̯us*, excluding this from the general loss of short vowels in apocope. We would rather say that the *u* is not preserved as such, but survives due to *u*-infection in the preceding syllable. Similarly, Thurneysen’s weird claim that the **a* of the sequence **i̯ias* survives as *e* is easier to explain as the **i* being lowered to *e* due to the **a* before the latter was lost.

-o > -a and interchange of -o and -u (§95)

“From about the beginning of the eighth century on, retained *-o* is interchangeable with *-a*.” This is essentially correct, as final *-o* then merged with *-a*. However, one should not say ‘interchangeable’ as it is not a two-way process. Original, final *-a* does not start to be written *-o* when original, final *-o* begins to be written as *-a*.

The Annals seem to support an early 8th-century start for such a change (from about the 720’s). Late examples of the preservation of *-o* are usually stereotyped, or consciously archaised, for example in names. An example mentioned later by Elliott, *bás muirchatho maicc...* ‘death of Muirchad son of...’ falls into this type of artificial spellings.

We do as a matter of fact find *túatho* very rarely for *túatha*, but this is so rare as to strike one as being artificial, or hypercorrect. The relevance of the variant spellings of *trícho* and *trícha* is uncertain as the preform is not firmly established, and there is morphological analogy at work there as well.

“Occasionally *-o* is found instead of *-u*, especially after *e*...” We seem to be dealing with a two-staged development here:

1. word-final *o* merges with and essentially becomes *a*
2. with the loss of word-final *o*, the only rounded vowel left in word-final position is *u*. This *u* may well have had a rather broad phonetic range at this stage and this may explain why it could be represented by both *u* and – at times – *o*.

As for the examples, *leo/leu*, this word, probably originally disyllabic, may have been diphthongised early under the influence of *béo*. Moreover, Blathmac shows that hiatus was optional in verse by the mid-8th century, implying that hiatus had already been lost in some registers by that time.

As to *a gnimo-som*, might there be some kind of a lowering effect of *-som* at play here as well?

As to the examples *baullo*, *gnímo* and *fírto*, these are all from the Stowe Missal. David and Lars recently went through this source and it seems that its scribe had lost the distinction between word-final *o* and *u*. This goes to show that one really needs to study each text and, ideally, each and every scribe in their own right when dealing with such matters.

Still, it would be worthwhile to investigate whether the interchange of *o* and *u* did indeed happen after the change of final *o* into *a*.

Pages read in this session: c. 5.