We differentiated with colors our responses for the reviewers comments and questions. While the reviewers comments and questions are indicated in black, our responses are written in green. Response for particular comment immediately below the comment.

We revised the manuscript according to reviewers suggestions and remarks. All changes made in the manuscript text are visible in the track-changes file uploaded together with revised manuscript. Figures were also changed and corrected according to reviewer's comments.

Anonymous Referee #1

Abstract: Look out for typographical errors. This is a fine introduction/synthesis of the manuscript but the incomplete English distracts the reader's attention somewhat. The same goes for the rest of the manuscript, which is well written but would benefit from fine tuning by a native English speaker.

We tried to correct the typographical errors and the general language quality of the manuscript. Referee #2 pointed out a several language suggestions and remarks to the particular statements/words in the text. We corrected them all.

Figure 1: Legend is a little confusing. The black circles with values are recalculated ages, yes? What are the black circles without values? It would also strengthen the figure to justify the positions of the LGM and Pomeranian ice margins – whose work confirms that these positions are correct? What about chronology? Without those details it looks a little like guesswork.

We corrected the figure caption with appropriate explanation. We also added reference related to the lines indicating maximum extent and Pomeranian Phase (Marks, et al., 2006).

Line 110: Such things are subjective. How big is massive? Give approx. size.

We specified that we are talking about boulders with perimeter about ≥ 1 m (lines 122 and 199).

Figure 2 caption: Do you mean 'diluvium' (or outwash)?

We changed to "alluvium and colluvium".

Figure 3: The figure is most illustrative. I suggest, for added clarity, that you specify that the yellow 'V' shapes in panel B and the frost wedge casts.

We added a proper explanation in the figure caption.

Section 3 is very detailed, which is excellent. Such attention to detail can help make your paper a methodologic resource for future work. On line 194, suggest you replace ‘decontaminated’ with ‘isolated’, as contamination in the cosmogenic workflow has very different connotations.

We changed to “separated” (line 210).

Section 3.2.1.: Why are you using the Borchers global production rate? The choice of PR is, of course, entirely up to the researchers but given the number of rates now available to use, every choice requires a strong justification in my opinion. This is particularly the case when the Borchers rate contains obviously flawed production 'calibrations' from Scotland. As detailed by Putnam et al. (2019), the inclusion in the global set of production rates calibrated against surfaces of *assumed* (i.e., not actually *known*) age artificially skews that global average rate, making it a little too high and thus the resulting ages unrealistically young. The CRONUS team themselves noticed the weird impact of the Scottish rates on the overall average (Phillips et al., 2016), pointing out some anomaly in that area, but couldn’t (or wouldn’t) pinpoint the lack of robust independent dating as the cause. This paper could be improved significantly, therefore, either by using one of the robustly calibrated production rates from the northern mid latitudes (including Europe) or by using the CREp calculator (https://crep.otelo.univ-lorraine.fr/#/), which allows users to remove dubious calibrations from the global primary dataset. I am immediately sceptical of Late Pleistocene studies that blindly use the published Borchers rate, particularly without a very strong justification.

We used Be-10 production rate from Borchers et al. (2016) as the most recent global production rate in the situation when we do not have any regionally calibrated Be-10 production rate in Poland or generally in Central Europe. Thus, the use of the most recent global data set seems to be the best solution. We note the reviewer’s comment about the slight difference between the production rates taking into account or not the Scottish data set in the calibration of the Borchers et al. (2016) Be-10 production rate. The difference between the primary and the secondary production rate calibration data sets from Borchers et al. (2016) will however not change the outcome of our conclusions as the difference is well within the uncertainties linked to the scatter of individual exposure ages. Also using different production rates than these ones based on the global dataset, e.g. Scandinavian reference production rate would not change our ages significantly (Tab. 1). In addition, the exposure ages in our study only provide “background” chronological data as described by the reviewer and as such, are not the pillar of the manuscript in anyway.

Tab. 1. Be-10 data for analyzed samples and surface exposure ages calculated according to various production rates.

Sample ID	[^{10}Be] (10^4 at g^{-1})	Age (ka)			
		Cronus default production rate (Borchers et al. 2016)	Primary dataset (Borchers et al. 2016)	Secondary dataset (Borchers et al. 2016)	Scandinavian reference production rate (Stroeven et al. 2015)
New samples					
LUB-01	13.11 ± 0.74	25.8 ± 2.4	26.0 ± 1.9	25.8 ± 2.8	26.0 ± 2.1
LUB-02	9.87 ± 0.46	18.4 ± 1.6	18.5 ± 1.2	18.4 ± 1.9	18.6 ± 1.4
LUB-03	7.42 ± 0.47	14.1 ± 1.4	14.2 ± 1.1	14.1 ± 1.6	14.3 ± 1.2
LUB-04	6.31 ± 0.37	12.5 ± 1.2	12.6 ± 0.9	12.5 ± 1.4	12.6 ± 1.0
LUB-05	11.58 ± 0.85	20.9 ± 2.2	21.1 ± 1.8	20.9 ± 2.4	21.1 ± 2.0
Recalculated samples					
LES-5	19.24 ± 1.16	40.3 ± 3.9	40.6 ± 3.1	40.3 ± 4.4	40.7 ± 3.4
LES-6	8.08 ± 0.58	17.4 ± 1.8	17.5 ± 1.5	17.4 ± 2.0	17.5 ± 1.6
LES-7	2.64 ± 0.33	5.8 ± 0.8	5.8 ± 0.8	5.8 ± 0.9	5.8 ± 0.8
LES-8	10.14 ± 1.10	19.7 ± 2.6	19.8 ± 2.3	19.7 ± 2.8	19.9 ± 2.4
LES-10	6.78 ± 0.57	13.0 ± 1.5	13.1 ± 1.2	13.0 ± 1.6	13.1 ± 1.3
LES-11	7.94 ± 0.77	16.0 ± 2.0	16.1 ± 1.7	16.0 ± 2.1	16.1 ± 1.8
LES-12	8.46 ± 0.70	16.1 ± 1.8	16.2 ± 1.5	16.1 ± 2.0	16.2 ± 1.6
LES-13	19.15 ± 1.33	35.5 ± 3.7	35.8 ± 3.0	35.5 ± 4.1	35.9 ± 3.2
LGM-12	11.50 ± 0.53	24.1 ± 2.1	24.3 ± 1.6	24.1 ± 2.5	24.4 ± 1.8

The differences between the arithmetic mean and the standard deviation for the eleven surface exposure ages analyzed in the manuscript is negligible: **18.0 ± 4.3 ka** using the default production rate dataset set in Cronus, **18.1 ± 4.4 ka** using the primary dataset of Borchers et al. (2016), **18.0 ± 4.3 ka** using the secondary dataset of Borchers et al. (2016) and **18.2 ± 4.4 ka** using the Scandinavian reference production rate (Stroeven et al. 2015).

Reference

Stroeven, A.P., Heyman, J., Fabel, F., Björck, S., Caffee, M.W., Fredin, O., Harbor, J.M., 2015, A new Scandinavian reference 10Be production rate. Quaternary Geochronology 29: 104-115, doi.org/10.1016/j.quageo.2015.06.011.

Line 238: I think the paper needs a clear statement (at some earlier point, not here) about the relevance of an OSL age and the sediments it is dating. My understanding of your stratigraphy is that these are minimum-limiting ages for the RzB2 unit, since the wedges were emplaced into that deglaciated surface. Therefore, the OSL ages do not date the emplacement of the till itself (advance) but the abandonment of that till surface (retreat), and the lag between deglaciation and ice wedge growth/infill cannot be known. Unless I am missing something fundamental here (quite possible), all your OSL ages are minimum ages and thus should be explained as such right from the outset.

We stated in the revised version of the manuscript that OSL data are the most relevant and that they are a very solid base for the Bayesian modeling (lines 20, 291-293, 421-422).

We stated clearly in the Results section of the revised manuscript that horizon K1 must has been formed after deposition of the Rz2a till and before the Rz2b till, and that horizon K2 must has been formed after deposition of the Rz2b till and before Rz2c (lines 257-259 and 264-265).

Figure 5b: As the authors point out, the Be-10 ages are not very consistent. This is a huge range, particularly in this day and age. Can authors tell us more about why these specific boulders were sampled? Were they all on specific moraine ridges, or are they randomly distributed? In other words, what is their significance? I'm pleased the authors include the beryllium dataset, for it shows complete transparency and no desire to hide 'ugly' data, but a little background would help readers understand better the rationale for sampling and also the potential problems.

We gave more info about sampled boulders and their geomorphological context in the revised version of the manuscript, in section 3.2 (lines 200-205).

We explained that "*relatively high relief of the study area promotes post-glacial erosional processes, i.e. rainfall washing and/or mass movements along slopes, degradation of the original moraines surface and possible exhumation of erratics from eroded deposits*" in the Results section (lines 302-305). We also provided additional figures with detailed geomorphological location of sampled boulders in the Appendices.

Lines 276-277: This pattern, should it be correct, is intriguing as it pops up in glacial records worldwide. Net retreat (warming) following the LGM was punctuated by brief pauses or readvances (cooling) that could have existed for just a few years/decades. If the pattern is more widespread than just central Europe, what does that tell us about its climatic drivers and importance? A broader exploration of this possible pattern is warranted, beyond the borders of Poland; if you don't explore it, somebody else will.

We argue that a broader exploration of this pattern with explanations of the reasons/mechanisms of cooling and warming could be too much and outside the frame of this paper. This kind of exploration/discussion and wider implications you suggested is tempting, but maybe it can be done involving wider dataset/record in a different paper (???).

Line 286: 'Exposition' is not the correct word here.

We changed to "exposure".

Line 294: I fear the relevance of the OSL ages is being overstepped here. These K1 OSL ages are minimum ages for the till itself, yes? Because the K1 wedges have been emplaced into the till surface *following* deglaciation. You need to make that clear, since the Rz2a till presumably predates the wedges. Yes, you've done bayesian statistics on this age set to get ages for the till, but the truth of the matter is that, from the real data themselves, there is only minimum-limiting age control for the basal till and it could easily postdate 19 ka. Indeed, knowing what we now do about the shape and duration of the LGM, this till could have been emplaced anywhere in MIS-2. Deglaciation subsequently presented a subaerial surface on which periglacial landforms could develop. I'm harping on about this because I think it is important; you could be misrepresenting the age of the basal till by multiple millennia, and that in turn could skew the common perception of when the LGM occurred in these parts (e.g., there is an important difference between, say 24 ka and 19 ka). Already, on Line 338, you are ascribing a concise-sounding age for the basal till (19.1 ± 1.1 ka) that cannot be ascribed based on minimum-limiting OSL ages.

The possible timing for the deposition of the till layers Rz2a, Rz2b and Rz2c was estimated based on Bayesian modeling, so the ages of tills are modeled ages – Bayesian probability distributions based on lithostratigraphic relative relations and OSL ages of the whole sediments sequence – we tried to emphasize this in the revised manuscript. We also changed an unequivocal statements in Results and discussion sections such as "*The first ice sheet advance which deposited Rz2a till dated at 19.2 ± 1.1 ka…*" into descriptions/explanations that this is the most likely timing for the ice advance according to our modeling results (lines 289-293, 357, 368, 376, 421-424).

Figure 6: In panels B and D, how do you know the ice margin retreated outside the study area like this? What is your geologic evidence for that? I don't see any described in this manuscript. Likewise, in panels C and E, what is your geologic evidence for the ice margin having stabilised at these tidy blue lines? Are there conspicuous moraine complexes defining a robust, stable ice margin, or is this conjecture? If the latter, please specify and use dashed lines rather than filled; otherwise, folk might take this as true when it could be little more than speculation. I assume the glacial geology of these parts has been thoroughly mapped?

We modified the maps in panels B and D – now there is ice sheet, but with question marks. Also in the text we stated that the ice sheet retreated to NW of the Rożental site, but we do not know for sure if these were retreats beyond the study area (lines 335-336).

Line 316: Yet, this is essentially speculation. Again, please be careful, as Figure 6 will give the impression that these speculative margins are based on unequivocal geologic mapping - and they aren't. Again, I suggest you use dashed lines instead and state specifically in the caption that these are entirely speculative.

We changed the marked ice-sheet limits into the dashed lines in Fig. 6 and describe/explain that these are the probable ice-margin positions.

Lines 327-329: Again, there's a lot of speculation here. Why not include some detailed mapping? That would make this a much stronger contribution.

We provided a figure with details regarding the interpretation of the ice margin positions (DEM and surface deposits) in our response for the review in the interactive discussion part.

Line 348: Again, this should be defined as a minimum-limiting age.

We stated in the revised manuscript what is the relation of periglacial horizons to till layers (lines 257-259 and 264-265).

Line 385: I understand the logic here, but this relationship is again highly speculative. What is the global nature of this inferred readvance? How is it possible to correlate with a Heinrich event if we don't, as a community, yet know quite what causes H events? (Or why some stadials include them and others don't, or some H events occur without stadials?) And when accurate and precise C-14 dating of H events in marine records continues to elude us due to reservoir uncertainties? I think this is saying way too much given both the uncertainties in your data (excellent OSL ages, but they do have sizeable error bars, as is expected) and the persistent lack of understanding regarding H events themselves. I'm not saying don't hypothesise, rather I just urge more caution.

Yes, we tried to change this part of the Discussion in the revised manuscript to emphasize, that such correlation is probable, that our result suggest this, but that it is not unequivocal (lines 402-406).

References cited:

Phillips, F.M., Argento, D.C., Balco, G., Caffee, M.W., Clem, J., Dunai, T.J., Finkel, R., Goehring, B., Gosse, J.C., Hudson, A.M. and Jull, A.T., 2016. The CRONUS-Earth project: a synthesis. Quaternary Geochronology, 31, pp.119-154.

Putnam, A.E., Bromley, G.R., Rademaker, K. and Schaefer, J.M., 2019. In situ 10Be production-rate calibration from a 14C-dated late-glacial moraine belt in Rannoch Moor, central Scottish Highlands. Quaternary Geochronology, 50, pp.109-125.