

We would like to thank The Cryosphere Editor for and opportunity to revise our manuscript, and the reviewer for their constructive comments on our writing. We carefully considered all comments and suggestions, consistently adding them into the revised version of the manuscript. Below we list those comments and detail our changes in response to those comments.

## Response to reviews: “Reorganisation of subglacial drainage processes during rapid melting of the Fennoscandian Ice Sheet [The organisation of subglacial drainage during the demise of the Finnish Lake District Ice-Lobe]”

### Editor

We thank the editor for noting an error in our experimental reporting and for communicating a detailed and helpful series of suggestions for improving our manuscript. Below, we describe our changes in response to the experimental error, and in the response to reviewer 1 go in to more detail regarding our changes to the overall manuscript.

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**Comment 0.1:** I have also found an error in your experimental design: The Wake and Marshall 2015 (WM15) paper used mean hourly data to determine their sigmaPDD not mean daily as you implicitly assume in your reasoning above. As such, there is no justification to use  $-5^{\circ}\text{C}$  as your reference (0 point) value for computing PDDs. Your justification of your PDD formula is also misleading. WM15 found significant variation in skewness and kurtosis and their derived distribution (“M2”) for PDD accounts for this and is therefore not Gaussian, but you make no mention of this when you cite them for your choice of  $\sigma(T_{\text{month}})$  that you apply to a Gaussian.

**Reply:** We have corrected the error in our experimental design, adjusting Line 239 to make clear that we are following van den Broeke et al., (2010) in setting our threshold to be  $-5^{\circ}\text{C}$  for the PDD. We have also added a line on 235–238 to explain why we did not take into account the changes in kurtosis and skewness with the M2 distribution from the Wake and Marshall 2015 paper, reasoning they become more significant where  $T_M < -20^{\circ}\text{C}$ . The line in question reads:

“We used  $\sigma_M$  from Wake and Marshall (2015), but did not take into account variations in kurtosis and skewness with temperature, as these become significant where  $T_M < -20^{\circ}\text{C}$  (see Wake and Marshall, 2015), temperatures below those derived from our depressed MAT. Instead we used the calculated  $\sigma_m$  to add Gaussian noise to a daily temperature record estimated by linearly interpolating our depressed MAT record.”

# Response to reviews: “Reorganisation of subglacial drainage processes during rapid melting of the Fennoscandian Ice Sheet”

## Reviewer 1

We thank reviewer 1 for the detailed second round of reviews, and we have changed our manuscript significantly in response.

### General comments

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**Comment 1.1:** While the authors have worked to address the comments, in my opinion, some work remains in making the manuscript more readable and addressing the underlying scientific questions presented in the manuscript’s introduction.

The authors did not seem to discuss the role of greater seasonal variability or diurnal variability in water discharge in the revised manuscript, despite in being mentioned by both reviewers. I believe that this could be important in the sediment dynamics controlling the formation of murtoos below the glacier.

Despite the uncertainties of the variability of the hydraulic forcing, I believe that model application is generally rigorous and that method is appropriate to address the research objectives. However, the writing remains hard to follow and does not fully address the research objectives. Reviewer 2 spoke to this in the last review, and with the benefit of hindsight, I should have pushed this matter further as well in my original review.

In the first section of the results, for example, it seems that much of the presentation is about the sensitivities of the model and description of the base case as opposed to addressing the questions posed in the introduction. Likewise, much of the first part of the paper summarizes the state of the research, as opposed to creating an imperative to address the research questions. For example, the link between the murtoo formation stages in section 2 and the model output that could represent these processes is not clear to me.

In my opinion, for this to be an impactful and unique work the authors must show how their results fit into the two research objectives at the end of the introduction. I think that both aims, particularly the second, can be accomplished. However, the paper will need some substantial rewriting and streamlining.

**Reply:** We have extensively rewritten the manuscript with a view to streamlining and improving the readability of the manuscript as well as making it clearer what questions we are posing, how we have sought to answer them, and what our answers suggest. Changes have been made in every section, but guided by these comments and additional communication with the editor, we have removed substantial superfluous portions of the text, rewritten our introduction to better set up the research imperative, placed information of limited interest into the appendix, merged our results and discussion section, and tried to streamline the text throughout. In response to a specific suggestion from the editor, we have changed our aim to

“to explore the ability of GlADS, a process based subglacial hydrology model, to explain murtoo formation in both space and time”

and have restructured our text around that single aim. In merging the results and discussion, we have paid particular attention to ensuring we describe our results in reference to this aim, rather than describing the model outputs in abstract terms and then moving on to the discussion. We hope the writing is easier to follow throughout.

Addressing the more specific comments about seasonal and diurnal variability, we have expanded upon why we did not use diurnal forcing in both our methods and discussion. In our methods, (Section 4.1.1, Lines 249–253), the new text reads:

“Without a detailed record of daily melt variability we neglect to include daily and diurnal changes in melt, which are known to drive rapid changes in hydraulic head on the Greenland Ice Sheet (Andrews et al., 2014).

Smoothing melt variability reduced model size and improved the stability of GlaDS over the ~27 year model runs, and we note that the inclusion of an englacial storage term in GlaDS acts to restrict the influence of diurnal variability to within 2 km of moulins with a limited influence on the overall pattern of channelised drainage (see Werder et al., 2013).”

and in the discussion (Section 5.2, Lines 419–426), in elaborating on why we do not see the periodic isolation of murtoo cavities, we have added:

“We did not include diurnal variability in our modelling on the grounds of model stability and the limited influence diurnal forcing has on catchment scale drainage in GlaDS (Section 4.1.1 & Werder et al., 2013). Diurnal forcing would be critical in order to represent rapid changes in the flow regime within murtoo-forming cavities. However, GlaDS is also a model in which the subglacial system is assumed to be pervasively hydraulically connected, and there is no mechanism which can lead to the hydraulic isolation of specific areas of the bed (e.g., Rada and Schoof, 2018; Hoffman et al., 2016). As a result, even if diurnal forcing were to be included, we do not expect to be able to reproduce the rapid changes in meltwater discharge necessary to form upper and lower flow regime deposits (see Section 2, & Hovikoski et al., 2023) or laminated muds in marginal murtoo channels (e.g., Ojala et al., 2022).”

We did not account for seasonal variability compared to the present day (a point raised by Reviewer 2) because our model domain represents the end of the Younger Dryas, when such seasonal variability gave way to a markedly warmer climate with similar seasonality to present. This text was present in the previous version, but has now moved to Lines 221–225.

## Specific comments

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**Comment 1.2:** Velocity. Throughout the manuscript please clarify if velocity refers to water velocity or sliding velocity

**Reply:** We have made this clearer throughout, specifying either water or basal at every mention of velocity.

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**Comment 1.3:** Ln 1. glacier thinning – > increased glacier melt? Melt, in addition to thinning, impacts basal characteristics.

**Reply:** We have changed thinning to melting, which more accurately represents the point we are trying to make.

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**Comment 1.4:** Ln 9-10. This sentence is strangely worded. I had the thought: if it is “hypothesised” then how can it be “ignored”

**Reply:** We removed this sentence whilst tweaking the abstract

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**Comment 1.5:** Introduction- Put e.g. in front of some citations. van den Broeke et al., 2023 were not the first to establish widespread melt in Greenland and Antarctica.

**Reply:** we have changed this as suggested, now found on Line 31.

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**Comment 1.6:** Ln 55. detailed treatment-> This challenge is also in part due to the computational resources, number of parameters, etc.

**Reply:** The wider text amongst which this was part has been removed, and this point with it.

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**Comment 1.7:** model ability -> model ability of what? to represent water pressure? Location of channelized vs pressurized flow?

**Reply:** We have changed this specific text, but the point remains and the new text (line 65–66) now reads: “we are not aware of previous work which has evaluated their ability to reproduce the subglacial conditions (e.g., water pressure, channel location) associated with glaciofluvial landform formation.”

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**Comment 1.8:** Table 1. Reference labels. i.e.  $q_s$ ,  $V_w$ ...

**Reply:** We are not sure how to interpret this comment, but we have made sure to check that all of the symbols are correctly formatted.

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**Comment 1.9:** Section 2. Title, I do not see how these two matters are connected, thus deserving their section together. The murtoo formation descriptions are pretty dense and I did not follow them. Maybe just include citations about the formation? Also, I was looking for a concise description of the model conditions that would be indicative of murtoo development (this is in Table1, which I appreciate and wish was addressed more directly in the results.)

**Reply:** We merged murtoo description with the study site in direct response to a comment on the previous round, but we have since separated them into section 2 (The glaciofluvial significance of murtoos) and section 3 (Study area). We have also significantly reduced bulk in this text, reducing it down to three paragraphs on murtoo location/geomorphology and murtoo sedimentology/formation sequence. In the results and discussion section, we now are more explicit about how certain model conditions are or are not indicative of murtoo development.

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**Comment 1.10:** Ln 277. The GIA discussion could fit in the discussion at the end if it is important. To me, it distracts from other messages here.

*and the related*

Ln 703. GIA is discussed here. Streamline the manuscript by removing the above.

**Reply:** We removed the GIA discussion from the methods and left it in Section 5.4 (Line 523–527)

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**Comment 1.11:** Ln 316. I do not follow what was done with the PDD model here. Were Braithwaite and Olsen used or not? Please streamline and clarify.

**Reply:** In responding to the Editors comment above, we have reworked this section to clarify that we did not use Braithwaite and Olsen.

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**Comment 1.12:** Section 3.1.2- Maybe a sentence or two about the purpose of the sensitivity tests and how this will interact with the murtoo observations and paleo conditions.

**Reply:** We have changed the last sentence of the opening paragraph in what is now Section 4.1.2 (line 261–262), adding detail about assigning confidence to findings where present across multiple tests which reads:

“We can assign higher confidence to our baseline model when similar model outputs (e.g., similar channel lengths or patterns of water pressure) are evident across multiple sensitivity tests”.

We have also added text in our restructured results and discussions section about why we have more confidence in some parameter runs and less in others.

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**Comment 1.13:** Ln 364- Basal velocities -> Are these prescribed or output of ISSM? Please adjust the sentence to reflect this.

**Reply:** The text in question (now Line 283) now includes ‘prescribed’

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**Comment 1.14:** Section 3.2 - Much of this section could fit in the discussion if it impacts the results. Some matters, such as the model representing a single time slice, might fit in other sections as well.

**Reply:** We have largely removed the text in this section, and as suggested moved much of it to the discussion (e.g., see Lines 428–440).

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**Comment 1.15:** Section 4.1.1 - This section should be reworked to link back to the research objectives at the beginning of the paper. While these results somehow seem reasonable, it is uncertain to me what the aim is. Given the content in Figure 2, it seems that channel distributions would be a reasonable place to begin.

**Reply:** We have heavily reworked this as suggested, doing so by folding the results into the discussion section and more explicitly linking our results to the research objective.

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**Comment 1.16:** Section 4.1.2 - Consider moving to the first section- Could this be used to show the viability of the model?

**Reply:** As per above, we have reworked this section and it now falls into Section 5.3 in which we compare modelling to the mapped meltwater routes in the FLDIL. Our primary finding from this comparison, that GlaDS reproduces the concentration of meltwater in mapped murtoo/meltwater routes compared to areas of the bed without evidence of meltwater, forms the first part of this new section and we do believe this shows the model is viable. We then move on to possible explanations for the statistically significant differences between murtoo routes and meltwater routes.

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**Comment 1.17:** Figure 3. Can these variables be linked back to conditions in Table 1 and discussed? This is what I was hoping for when I looked at Table 1.

**Reply:** We cannot easily link the contents of Figure 3 (now Figure 2) to the murtoo developmental stages in Table 1. Figure 2 is instead intended to show our modelling reproduces the spatial pattern of murtoo formation; namely that they form where water pressure is equal to or exceeds ice overburden 40–60 km from the ice margin, at the onset of channelisation. Table 1 describes the developmental stages of an individual murtoo, and we link this directly to Figure 3, which shows the evolution of specific nodes through time. We make direct comparison between the two in Section 5.2 (Comparison to the murtoo developmental phases).

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**Comment 1.18:** Paragraph 464-470 - This is a hard paragraph to follow.

**Reply:** The content of this paragraph can now be found on Lines 452-466, which we have reworked with the goal of improving its readability.

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**Comment 1.19:** Ln. 490 - This sentence is almost exactly like the one at the top of 3.1.1.

**Reply:** This text was removed as part of our rewrite.

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**Comment 1.20:** Paragraph 553-572 - The findings of the model results are not discussed here. I am curious about the interaction of the result with this existing knowledge.

**Reply:** We have removed this paragraph, instead more explicitly linking the murtoo features to model output throughout Section 5.

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**Comment 1.21:** Ln 580. For reasons regarding channel shape discussed in my last review, I believe that these findings about the channel radius are highly speculative.

**Reply:** Because GlaDS represents channels as a fixed semi-circle, this reference to channel radius was intended to communicate that channel radius compared well to the maximum cavity height inferred from supra-murtoo boulders. However, in response to this comment we have moved references to channel radius and our findings therein.

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**Comment 1.22:** Section 5.3 Isn't this topic introduced in the paragraph starting at 573?

**Reply:** In Section 5.3 we were communicating direct comparison between murtoo routes and modelling outputs, whereas in the paragraph starting at 573 we were comparing predictions about murtoo formation (in terms of distance from the ice margin and proximity to channelisation). We have changed our results and discussion heavily, but the section which most closely matches this is now called 'comparing model output to meltwater routing beneath the Finnish Lake District Ice-Lobe' in an effort to clarify this comment.

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**Comment 1.23:** Ln 690. Thin sediment, low sediment supply -> this needs a citation.

**Reply:** We have added a citation on Line 486 to Bradwell, T, 2013: Identifying palaeo-ice-stream tributaries on hard beds. Mapping glacial bedforms and erosion zones in NW Scotland, Geomorphology.