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

Reviewer 1

We thank reviewer 1 for their encouraging and helpful comments. Below we detail our response to each and in turn the changes we have made to our manuscript. We believe that these changes have improved the manuscript.

General comments

Comment 1.1: I found the title a bit misleading about the topic of the paper. I think the manuscript speaks to the past location of hydraulic conditions below the glacier, but I found little in the manuscript about "reorganisation of subgalacial drainage processes". Maybe "organisation of subglacial processes." Additionally, "rapid melting" does not seem like a big part of the paper, especially by reading the abstract.

Reply: Both reviewers queried the appropriateness of the title, we have modified the title to now be:

"The organisation of subglacial drainage during the demise of the Finnish Lake District Ice-Lobe".

We hope this better reflects the contents of the paper.

Comment 1.2: Throughout, but especially in the abstract and introduction, the authors make statements of "parame- terizing and testing models of subglacial hydrology", "basal hydrology in models", or "basal hydraulic conditions." These conditions or parameterizations can cover a wide range of features describing sub- glacial processes, include channel size or shape, water pressure, sediment transport, water velocity, distributed or channelized drainage. I believe the authors must be more specific and deliberate in describing the specific subglacial hydraulic features they aim to examine and link these features to murtoo development and persistence.

Reply: We have altered the abstract and introduction as suggested to be more explicit about our approach and aims. . Particularly with regard to our aims. The aims now state:

"

- Compare the subglacial hydrological conditions proposed for murtoo genesis and their associated landforms against model outputs from GlaDS.
- Sensitivity test GlaDS across a range of possible parameter values to explore the influence of these parameters on our outcomes in order to evaluate the potential of such models to be used to interrogate palaeo-hydrological systems more broadly and in turn motivate future work in this area.

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Comment 1.3: Related to the last point, it seems that the description of murtoo formation could be improved. At times, it seems that hydraulic processes associated with different stages of murtoo development are in contradiction. An examples are given below.

Reply: We have addressed this by responding to the specific comments below.

Comment 1.4: Section 3.1.1: From my reading of this section, it seems that no diurnal forcings were used. While this makes sense in a paleo setting, I am concerned about the impact on results. For instance on the GrIS, hydraulic head can very over 150m and bed separation can be in excess of 25cm (Andrews et al., 2014). It seems like such short temporal changes in subglacial hydrology could impact the formation of murtoos and move from one murtoo sequence to another over a very short time period (stages mentioned in Introduction, Hovikoski et al., 2023). I realize that application of such variable water discharges to hydraulic models can be difficult and in many scenarios not necessary. However, it seems like it could be important in this application.

Reply: Yes, we did not include any diurnal forcing. In reality, diurnal forcing may well be extremely important in murtoo formation right at the very onset of channelisation and is absolutely a target for future work on this subject. We have added it to our limiations. However, for the work presented here which aims primarily to describe the catchment scale processes in hydrological development, we note that in Werder et al., 2013, their diurnal experiment results in channels that largely follow the same spatial expression as in other runs. They do experience large fluctuations in discharge and pressure throughout the 24H cycle, however, at distances greater than 2 km from channels (and moulins), pressure fluctuations are minimised by the englacial storage term, which reduces the spatial influence of changing meltwater inputs at short timescales. Within the murtoo forming zone then, we expect that the pattern of pressure across the full-width of the domain (especially when averaged seasonally) would likely be comparable to our forcing runs without diurnal fluctuations, with changes in the pattern restricted near to channel heads and specific moulin inputs.

Comment 1.5: To the best of my knowledge GlaDS uses a semicircular channel geometry that is fixed (i.e. shape of the channel does not evolve). However, it seems that a key feature of murtoo development is low broad channels, potentially with changing channel shape. This seems to be discussed in Hovikoski et al., 2023 and in the manuscript at lines 511 to 523. Hooke et al., 1990 speaks to the effects of channel shape on subglacial hydraulics. I am aware that certain trade offs can be made between the friction factor and channel shape to end up with similar hydraulic characteristics. In some applications this may minimize the impact of the semicircular assumption. However, because sediment transport relationships are scaled to unit width of the channel, sediment deposition can be sensitive to the width of the channel floor, and thus the general shape of the channel. However, please comment on how this may impact the results. Is this such a consideration with the development of the drainage system? What are the impacts of the semi-circular and potentially fixed channel shape on the formation of murtoos?

Reply: GlaDS does indeed model channels as semi-circles, and we have added a line to this effect in the methods (Line 245) which reads:

“channelised flow—describing uniform, semi-circular Röthlisberger channels (R-channels) that are allowed to change diameter—along element edges”.

Because GlaDS does not include any explicit treatment of sediment dynamics we are not truly modelling murtoo formation. Instead, we are attempting to reproduce the conditions associated with murtoo formation, particularly water pressure throughout the melt season. It is therefore difficult to evaluate what effects changing the channel geometry may have on murtoo formation. Further, without a detailed understanding of exactly how channel geometry varies in both space and time it is difficult to imagine how we might robustly explore this. Nonetheless, we have also more explicitly raised this as a limitation of our work. This can be found in Section 5.4.

Comment 1.6: More out of curiosity, how do the murtoo fields persist given the retreat of the glacier and the presumptive movement of the channelized drainage area up the glacier? Might retreat have occurred too rapidly to “destroy” the murtoos?

Reply: That is an interesting question, and one subject to ongoing investigation, but it is beyond the scope of this manuscript.

Specific comments

Comment 1.7: Ln 46: to the best of my knowledge Werder et al. (2013) examines hard bedded characteristics below glaciers, "subsurface material" needs clarifying. Would an alternative be sediment floored channels or canals.

Reply: We have replaced this reference with one more appropriate, specifically Chu, V.W., 2014. Greenland ice sheet hydrology: A review. *Progress in Physical Geography*, 38(1), pp.19-54.

Comment 1.8: Ln 59-71: the modeling work of F. Beaud is likely relevant here, as is the manuscript Hewitt and Creyts (2018) about eskers. Consider adding.

Reply: We have added the references to both in an expanded portion detailing previous modelling work in the palaeo setting, as well as work by Boulton et al. This can be found on Lines 88–100.

Comment 1.9: Ln 87: what does "more dynamic" mean also, I can imagine what "interlobate joints" are, but please clarify.

Reply: We have changed this sentence to clarify that 'more dynamic' means faster, and removed the reference to interlobate joints in response to a comment by Reviewer 2, the sentence now (Line 163) reads:

"...which are in turn concentrated in faster flowing, *warm-based* sectors of the FIS including the FLDIL..."

Comment 1.10: Enumerated 1-4 in Intro: I found this useful, and closely linked to Figure 10 in Hovikoski et al. 2023. Would the authors consider applying the cartoon in this manuscript? Additionally, it was difficult for me extract in the enumerated section the model output that would be indicative of this process in murtoo development. Please clarify. Might a table with one column of subglacial hydrology model output help?

Reply: Thank you for this suggestion. We have added a table (Table 1) linking the murtoo developmental stages to expected model outputs, and added callbacks to this table in our results and discussion. We have referenced the Hoviloski et al., paper more explicitly, but have not included their specific figure so as to avoid any copyright reproduction issues.

Comment 1.11: About points 1-2, I am a little curious about the idea that there is sediment deposition at the onset of melt. It seems that the conduit could be small, thus increasing water follow could increase sediment transport capacity, rather than cause sediment deposition. Although available observations of sediment transport are from the terminus, there can often be an increase in sediment transport at this time of the season.

Reply: We envisage that the subglacial water flow is in pulses against an overall backdrop of increasing discharge through the melt season. When flow fluctuates parts of broadening/low conduits become rapidly clogged by sediment-rich flows in shallow flow space and sediments are periodically slightly deformed by ice. During conduit widening, the marginal channels of murtoos seem to have the highest transport capacity. We have clarified our stages 1–2 to reflect this. Stages 1–2 are now:

"

1. With the onset of spring melt, pulses of water deposit the murtoo body within an increasingly large conduit. As each pulse increases in discharge and then wanes they promote the deposition of sand lenses, sinusoidally stratified sand, and poorly-sorted gravel, with silt commonly draping ripple-scale features. In this phase of formation, cobbles are the largest clast size, which places an upper limit on water depth of $\sim 25\,\mathrm{cm}$ (Hovikoski et al., 2023).

2. As the melt season continues through summer, an increasingly enlarged pond forms in response to higher discharge. In turn, the increasing grain size indicates higher water velocity and sediments on the upper slope appear consistent with high velocity, upper-flow-regime deposits and the boulder size-distribution suggest a maximum flow space of 1 m (Hovikoski et al., 2023).

”

Comment 1.12: Ln 120: “higher water velocity” and “development of an englacial pond.” To me these processes should not happen at the same place and time. Also, please define “upper-flow-regime.”

Reply: Our original phrasing was a little muddled, though we note our original manuscript contained “development of an *enlarged* pond...” and not an “englacial pond”. The expectation is that the pond already exists before higher velocity flows reach and form upper-flow regime structures. We have corrected the text to reflect this:

“3. As the melt season continues through summer, an increasingly enlarged pond forms in response to higher discharge. In turn, the increasing grain size indicates higher water velocity and sediments on the upper slope appear consistent with high velocity, upper-flow-regime deposits and the boulder size-distribution suggest a maximum flow space of 1 m”

Regarding the definition of sedimentological terms, in this section we briefly describe the sedimentological architecture of murtoos as background, referencing several papers with this as their main focus. Accordingly, we have not defined upper- (or lower-) flow regimes, both of which are relatively common concepts in sedimentology but whose specific meaning is adjacent to the main focus of our study. However, we have adjusted the section such that the deposits we reference are more specifically linked to the regime in which they form so that the line (189–192) now reads:

“proximally is comprised of alternating sequences of glaciofluvial deposits, with current ripples (formed in low discharge, lower flow regimes) giving way to transitional cross-bedding (transitional flow regimes), and antidunal sinsuoidal lamination (formed in higher discharge, upper flow regimes;...”

Comment 1.13: Figure 1: Could estimated glacier flow lines be added?

Reply: Yes, we have added arrows indicating the approximate ice flow direction to each panel in Figure 1 and updated the caption accordingly.

Comment 1.14: Ln 195: “modified digital elevation model”... can the section where this is described be referenced?

Reply: We have added the section reference so that the line (Line 235) now reads:

“Then, using GlaDS parameterised by this input ice geometry and a modified digital elevation model (DEM) of the region (see Section 3.1.1)”

Comment 1.15: Table 1: I am curious if “mean annual velocity” is really an “input” or a model result or output, given the coupling with ISSM.

Reply: In this work, we are not using GlaDS coupled to ice dynamics. Instead, ice velocity (and other ISSM parameters, such as the ice rheological properties) are effectively model inputs. We have clarified this in response to one of Reviewer 2’s comments (Comment 2.52:). The line to this effect (Line 270–271) now reads:

“Finally, in the iteration used here, GlaDS is not coupled two-ways to a model of ice dynamics, and instead we prescribe an ice velocity and geometry that is not variable in response to hydrological forcing”

Comment 1.16: "Fixed cross section" or "to the bed at every node." Does this go well here? or is somehow part of experimental design?

Reply: We have left these in place, as we are describing the GlaDS model design and believe this to be the most relevant section for this.

Comment 1.17: Ln 361: "At node 3,842": maybe make clear that these nodes are representative of their surrounding.

Reply: We have added a clarification to this effect. The sentence now reads:

"At node 3,842, chosen to be representative of surrounding nodes,..."

Comment 1.18: Figure 3c: should "D" be written as distance? also it seems like this is the end of the melt season of one year. Would it make sense for an "average" to be represented? Also would it make sense to add A-E in the plot in C as to clarify which plots go with which points?

Reply: We have changed the inset plot in what is now Figure 4C to read 'Distance from the ice margin (km)'. We have also added A–E labels into the plot as was also suggested by reviewer 2. We chose to illustrate the end of the melt season to give an idea of what channels looked like spatially at the end of the melt season and to give context to the time-series plots from four points. We also note that the summer average model condition is shown in what is now Figure 3.

Comment 1.19: Ln 505–506: why 10^0 in one line and 1 in the next?

Reply: Thank you for noting this, we have changed the former to keep consistency.

Comment 1.20: Ln 515: "limited cavity expansion" might this be channel floor width?

Reply: We have changed the line (now 584–586) to now read: "however the agreement in dimension suggests that the limited cavity expansion or restricted channel floor width within which murtoo form is captured within our model"

Comment 1.21: Ln 520: "The reason... sediment supply..." My initial reaction upon reading this is that I normally do not consider a distributed drainage system able to transport large amounts of sediment, thus I am unsure about how sediment supply up glacier could impact the results here.

Reply: Murtoos occupy a semi-distributed environment where movement of subglacial sediment is an important factor as indicated by the sedimentological studies of murtoos. We have expanded on this idea within our rewritten discussion, which can be found on Line 690–696:

"Murtoos appear to form within a semi-distributed drainage environment, and sedimentological studies indicate the movement of sediment is important in murtoo formation (Peterson Becher and Johnson, 2021; Mäkinen et al., 2023; Hovikoski et al., 2023). The reason that murtoos are not present in an area of the FLDIL where our modelling suggests they should form may be a preservation issue or due to limited sediment supply. Sediment cover in this area is very thin, and the large areas of exposed bedrock likely limited the supply of sediment from which murtoos could form (Figure A1B), an interaction not yet accounted for in our modelling. Modern lakes are also abundant in the centre of the FLDIL and may also act to mask murtoo routes."

Comment 1.22: Ln 524: "More broadly" good pun after speaking of broad channels... "More generally"

Reply: The pun was unintended and we have changed the text as suggested.

Comment 1.23: Ln 544-549: I might be missing something. However, melt water input location also seems like a control. For instance, Gagliardini and Werder 2018 may speak to this.

Reply: We have added this reference and a pointer to lower crevasse density at higher elevation also inhibiting meltwater input. The line (570) now reads:

"shallow surface gradients engender low hydraulic potential gradients, while low crevasse density limits meltwater input to the bed (Gagliardini and Werder 2018)"

Comment 1.24: Ln 558-567: From reading this paragraph, the authors seem to point out the differences between GrIS and the runs here. However, I seem to miss the analysis of the causes of this difference between the two systems.

Reply: We expect that the difference arises because of the low-relief of our domain. We have added a sentence to this effect on Line 621–623 which reads:

"The FLDIL is relatively low-relief compared to the steep margins of Greenland (e.g., Wright et al., 2016), and the shallow topography may act to reduce the hydraulic gradient between distributed and channelised drainage."

Comment 1.25: Ln 593: "sub-lobes they bound" something funny grammatically, also I am not sure what is meant.

Reply: We have modified the sentence (now on Line 720) to read:

" As a result, landforms within the FLDIL have previously been divided into three sub-lobes. The boundaries between these three sub-lobes are demarcated by particularly large esker deposits suggesting a concentration..."

Comment 1.26: Ln 606: "1-2 day. . . walltime?"

Reply: Yes, thank you for noting this. We have clarified to this effect which now reads: "could run to completion *with a walltime* of 1–2 days while"

Comment 1.27: Ln 613: $\sim$0.75°C how much more water does this result in?

Reply: Raising the model MAAT by a uniform $\sim$0.75°C changes the mean moulin discharge (at every non-zero moulin through the full model run) by $\sim$10% from 2.544 to 2.774 $m^3 s^{-1}$ (an upper limit based on running our PDD scheme 5 times). However, this represents an upper limit because uplift and tilting was non-uniform in our domain and the actual change in domain-wide MAAT is likely to be lower. We have not reported this in the manuscript, but we have changed the wording to make clear that the $\sim$0.75°C is confined to the areas of highest uplift. These changes can be found in both the methods (Line 280–285) and the limitations (Line 730–734) sections.

Comment 1.28: Ln 627: "macro conditions" what are these precise conditions?

Reply: In response to several comments from Reviewer 2 we have significantly changed our Conclusions section, in doing so removing this term and defining the conditions to which we are referring.

Comment 1.29: Figure A2. what is a median discharge? Also, it seems like units are missing.

Reply: Thank you for noting this, we have rectified both the missing units and clarified what is meant by median discharge.

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

Reviewer 2

We thank reviewer 2 for their detailed and helpful comments. Below, we list each comment, our reply and the changes we have made in response. We have made significant changes throughout to our writing and we thank reviewer 2 for their diligence.

General comments

Comment 2.1: In principle, this paper puts forward an exciting approach to compare conceptual ideas (ie geologically-driven) of landform (murtoo) formation with model predictions of hydrological settings. I see a lot of potential in such data-model exploration, particularly in relation to murtoos, since they are hypothesised to represent a transition in drainage style. However, I found this manuscript hard work due to "heavy" text, to the extent it is difficult to evaluate the significance of what has been found. The manuscript is suitably constructed in its overall structure, but I struggled with a lack of direction and clarity in the writing. I wasn't sure by the end of the Introduction, or the Methods, what the study was designed to explore, specifically, or what question was being asked; I had to read the Results to try and work out what the authors were actually trying to achieve (beyond a generality). Parts of the Discussion are more a summary of results than an exploration of their significance, and important insights are hidden or implied rather than explicitly stated. The same is true of the Conclusions, which ultimately don't say anything concrete about what has been learned.

Reply: We have significantly rewritten the introduction of our paper, including adding specific aims which we hope clarifies what our work was designed to explore, which specifically is a comparison of the specific hydrological conditions proposed for murtoo genesis against model outputs from GlaDS, a model capable of resolving the transition in drainage styles. We have tried to clarify heavy text, particularly in the introduction, placing more focus on why the transition between distributed and channelised drainage is important for glacial hydrology and ice dynamics, and of the potential for palaeo beds to shed light on the transition. We have made major changes to the writing in the discussion. We have also heavily rewritten the conclusions

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Comment 2.2: The study area for modelling – and the distribution of murtoos – encompasses the whole Lake District lobe but the entire focus of the results is in the distal few 10s km. I realise the duration of the model effectively only considers one ice time-slice in the development of the whole lobe's landform system, i.e. when the ice margin sits at Salpausselkä II. However, I think this needs to be stated explicitly, and I think the paper needs to discuss what the modelling findings imply for the formation of the other (most of the) murtoos in the domain. Do your results imply (or reject) that the upstream murtoos could form at the same time as those you consider in the near- marginal zone? Do your results imply that they must form time-transgressively (headward) during margin retreat?

Reply: We have stated in the discussion that because the area of high $overburden_{\%}$ is restricted to 40–60 km in most of our model outcomes, murtoos are implied to be time-transgressive, the new lines (586–588) read:

" If we accept the hypothesis that murtoos form where $overburden_{\%} \approx 100\%$ our modelling supports the idea that the murtoos mapped >70 km from the ice margin postdate 12 cal. ka and that murtoo formation is time-transgressive (Ahokangas et al., 2021)"

We also now note throughout that our model corresponds to the 12 kyr time slice. As this model isn't run as a transient hydrology/ice dynamics coupled configuration we are only able to comment on the conditions at the time slice we are examining and therefore we can't make any further arguments about formation of murtoos from other time periods. Future experiments, involving a more complex model setup could address this interesting question.

Comment 2.3: Related, considering the distribution of murtoos over your model domain and the 40-60km band you focus your analysis on, one might wonder why (if) this particular time-slice is well-suited to the investigation – murtoo fields are actually rather few/sparse in this band, compared to elsewhere up the trunk of the Lake District lobe. Could you offer some justification for your approach?

Reply: Markers of ice margin position are relatively sparse within the Lake District lobe, so rather than arbitrarily picking a margin position based on murtoo density we chose to bound our model with the clearest ice margin marker at the second Salpausselkä. We have changed the study site section significantly in response to additional comments below, and we do elaborate on the fact that the FLDIL is a particularly well constrained ice lobe, and that there is no ice margin markers beyond Salpausselkä 2.

Comment 2.4: Some recent work is suggesting that the YD may have experienced extreme seasonality, with relatively warm summers but extreme winters (e.g. Schenk et al. 2018 Nat Comms, 2020 QSR; Amon et al. 2022 Clim. Past). You replicate a Younger Dryas climate by lowering present monthly temperatures uniformly by 15°C. I wonder how extreme (or simply, different) YD seasonality would impact your results? How important is this choice of climate forcing for your hydrology conclusions?

Reply: We recognise that by simply depressing the climate by 15 degrees we are heavily simplifying the complex climate seasonality during the YD as indicated by these papers. Anecdotally, shorter summers and more extreme winters would likely reduce the length of channels and their discharge, restricting the duration and extent of the murtoo forming zone. It is difficult to say decisively what influence this may have had on our results because neither suggested paper gives a prescriptive climate record on a annual resolution, however, we do acknowledge that the repetitive annual signal here is likely in part responsible for enabling the biannual signal to appear. Formally including this seasonality (in a statistical sense) is potentially a very interesting avenue for future work. However, we note that in fixing our margin to the second Salpausselkä, our domain is representative of the end of the YD during which this seasonality gave way to a markedly warmer climate (e.g., Mangerud et al., 2023).

To acknowledge this, and the important work mentioned above, we further elaborate on our choice of forcing (Lines 310–314), which now reads:

"In simply depressing the climate we are neglecting to include the complex seasonality (short, warm summers with extreme winters) that characterised the Younger Dryas cold reversal in Fennoscandia (Schenk et al., 2018; Amon et al., 2022). However in fixing our domain to the second Salpausselkä our model is representative of the end of the Younger Dryas at which time this extreme seasonality rapidly gave way to a markedly warmer climate with similar seasonality to the present day (Mangerud et al., 2023)"

Reference: Jan Mangerud, Anna L.C. Hughes, Mark D. Johnson, Juha Pekka Lunkka, Chapter 46 - The Fennoscandian Ice Sheet during the Younger Dryas Stadial, Editor(s): David Palacios, Philip D. Hughes, José M. García-Ruiz, Nuria Andrés, European Glacial Landscapes, Elsevier, 2023, Pages 437-452, ISBN 9780323918992, https://doi.org/10.1016/B978-0-323-91899-2.00060-7.

Comment 2.5: Section 3.2 implies that only the zone 40-60km upstream of the ice margin is analysed, on the basis that this zone is favourable to murtoo formation. This seems a little circular to me. By focusing only here, do you not exclude the possibility of identifying conditions that would suit murtoo formation elsewhere? And exclude possible comparable hydrological/glaciological conditions elsewhere that may or may not support murtoos?

Reply: We have modified Section 3.2 to make clear that we did indeed analyse the full domain, but that given the time-integrated nature of the landform record, we did not expect to (for example) be able to compare modelled channels within our domain (representative of ∼12 cal. ka) against eskers formed across the domain and likely formed long after 12 cal. ka. Regarding the murtoos specifically, we isolated the zone

40–60 km from the ice margin because that is the hypothesised area of murtoo formation, and it provides a readily testable set of conditions with which we can begin to explore our model results.

It is true that in doing so, we ignore the potential for murtoo-suitable conditions elsewhere. However, across the suite of our model sensitivity tests we note that the area of $overburden_{\%} \approx 100\%$, a key characteristic of the hypothesised murtoo formation environment, is largely confined to this 40–60 km area ±10 km. In tests where this is not true, for example when sheet or channel conductivity are at the tested limits, modelled channels are extremely short and densely spaced, or not present at all, which provides an independent query of those specific parameters. The modified text to this effect can be found in Section 3.2.

Comment 2.6: One of your results is rather surprising, which calls into question the appropriateness of comparing model output with specific mapped geomorphology: if channel discharge in late summer is significantly higher outside mapped meltwater routes (line 395) than where meltwater routes have been recorded, then this suggests the spatial distribution of channels predicted by the model is offset from where channels are known to have existed. This mismatch therefore also questions the validity of comparing hydrological parameters with where murtoos have been mapped or not mapped.

Reply: Thank for noting this, which is a major error on our part. Figures 4 & 5 show that channel discharge is not higher outside of mapped meltwater routes than within mapped meltwater routes, and in fact the opposite is true. We have fixed this text which now reads:

"There is no significant difference between any group in terms of Q_c with the exception of between June–October (Table A3), during which Q_c is significantly higher within murtoo and meltwater routes than beyond"

Comment 2.7: A further uncertainty in this regard concerns what is meant by a meltwater route? Section 3.2 suggests these are based on Ahokangas et al. As far as I understand, those authors keep eskers separate from their meltwater route classes – how did you treat eskers in your classification of "meltwater routes"?

At the very least, I think the presentation of results relating to geomorphological classifications needs to be preceded by a justification that the model performance is adequate in terms of what we know of the geomorphology: does the model do a good job of replicating channels? If it does, then it's a valid tool to explore other landforms, and if not, then it's not. This partly is presented in the Discussion (524- 540) (though the earlier result I've queried here is not addressed) and I think would better serve the Results section if it were brought earlier. I also wonder, if the exact location of channels is sensitive to mesh geometry (536) then does this also not suggest that specific site-to-site data-model comparisons may not be appropriate?

Reply: In response to the first point about meltwater routes, Ahokangas et al., do treat meltwater routes separately from esker deposits (including them as "channelised routes"), however in many places they are coincident and cross or follow other meltwater routes, suggesting they are later features. Without an age-control on each of these, in this classification, we do not make a distinction between eskers and meltwater routes, reasoning that all are geomorphological indicators of meltwater flow. We have added two sentences to the end of this section (3.2) which now read:

"Ahokangas et al., (2021) mapped eskers separately to meltwater routes, including these as "channelised routes" in their datase, however, they note that many of these routes fall within meltwater routes and likely correspond to a later time of formation. Accordingly, without age-control, we do not make a distinction between meltwater routes and channelised routes here. "

To the second point, we have added a sentence to the results saying that the spacing and appearance of model channels compares well to mapped esker deposits:

"...with channels arranged perpendicular to, and extending up to $\sim$ 50 km inland of, the ice margin and comparable in structure and spacing to the location of esker deposits in the FLDIL..."

We have also moved what was figure 6 (which shows the comparison of eskers to modelled channels) into the results. Regarding their specific location, and what it means for the model to be "doing a good job", though the exact location of channels varies according to mesh geometry, within reasonable bounds (i.e., not when edge lengths exceed tens of km) the spacing and length of channels remains robust, and channels largely fall in similar locations. Any modelling work has to make a decision on what level of fidelity is an reasonable approximation of reality, balancing this against the greater computational cost of highly refined meshes. As such, where the mesh is fixed in space and time, the exact location of channels will always depend on mesh geometry to some extent. Note recent work by Felden et al., 2023 which uses an adaptive mesh in order to attempt to overcome this, however, they do so using a grid structure which imposes directional biases.

We have added a sentence in the discussion to explain why mesh dependency arises is the case and we also do note that the spacing and alternating pattern of larger and smaller eskers is similarly reproduced within our model. The text (Lines 626–629) in question now reads:

"Modelled channels in our baseline model (Figure 2) and many of the sensitivity tests have similar locations as eskers mapped by Palmu et al (2021), particularly in terms of their lateral spacing, length, and the observation that smaller esker deposits are alternately found between large features (Figure 2)"

and on Line 632–637:

"In the baseline model specifically, at several locations, modelled channel outputs closely track the location of several particularly large esker deposits (Figure 2B–C). We caveat this by noting that because our model operates on a mesh, the resolution of which is a balance of suitable fidelity against the increased computational cost of resolving finer details, the exact location of these modelled channels is sensitive to mesh geometry. Channels cannot form where no element edge exists. Differences in the exact channel location also arise because of moulin density and location, bed topography, velocity, and basal bump height. Nonetheless, the spacing and length of channels remains robust against the parameters tested here"

As elaborated upon on in our limitations section (Section 5.4), many of our decisions and restrictions mean our representation of the FLDIL is idealised, as to some extent all modelling is. Accordingly, we do not argue in our manuscript that our modelling is capable of resolving specific murtoo fields or esker deposits, rather in our idealised representation of the FLDIL we hope to be able to represent the broad patterns of drainage.

Reference: Felden, A. M., Martin, D. F., and Ng, E. G.: SUHMO: an adaptive mesh refinement SUbglacial Hydrology MOdel v1.0, Geosci. Model Dev., 16, 407–425, https://doi.org/10.5194/gmd-16-407-2023, 2023.

Comment 2.8: Overall, the manuscript would have benefitted from a thorough proof-read – there are numerous typos, left-over words from earlier constructions, unit errors, muddled variables.

I note these in a rather lengthy list of line-by-line technical comments in the attached pdf, where I also identify issues with writing clarity, section by section, and suggest how the direction and framing of the work could be improved.

Reply: Below, we address each of these technical comments.

Specific comments

Comment 2.9: The title doesn't seem appropriate: neither "reorganisation" nor "rapid" melting are explicitly examined or discussed.

Reply: Please see our response to Comment 1.1:, the new title is: "The organisation of subglacial drainage during the demise of the Finnish Lake District Ice-Lobe" .

Comment 2.10: Introduction – there is useful and relevant content here, but its presentation is disjointed and doesn't build towards a specific research question. The murtoo section is important but reads like a fact dump, while the rest of the hydrology overview flags various unknowns without us really knowing which, if any, of these you hope to address. From your "In this paper" statement, I am not sure what you hope to achieve or what you will actually do: "exploring conditions for murtoo formation" and "evaluating models" are rather vague ideas. I suggest: (i) pull out the bulk of the murtoo background and move that to the Study Area section (e.g. "Study area and significance of murtoos for basal hydrology"), leaving just a brief intro and summary of the significance of murtoos for your study in the Introduction; and (ii) revise the remaining sections to build a more coherent narrative that poses a problem and sets out a specific goal of this work.

Reply: As suggested, we have removed the bulk of the murtoo detail and folded it into the study area section (Section 2). We have also heavily modified the introduction, which we hope now builds towards our specific research aims, listed at the end of the shortened introduction and which now read:

"

- Compare the subglacial hydrological conditions proposed for murtoo genesis and their associated landforms against model outputs from GlaDS.
- Sensitivity test GlaDS across a range of possible parameter values to explore the influence of these parameters on our outcomes in order to evaluate the potential of such models to be used to interrogate palaeo-hydrological systems more broadly and in turn motivate future work in this area.

"

Comment 2.11: At the end of the Study area section, I am still not clear on what the purpose of murtoos is to this study. Are you testing the conceptual (sedimentology-based) models of how they form? Are you testing numerical model performance, if we accept the sedimentology conceptual model? What is the purpose and scope of this work? Given that scope and research question, what murtoo traits are specifically important to your study, and in what way? Why is e.g. distribution with respect to lineations important, since you don't return to this later in the Discussion? In presenting murtoos as a tool for understanding hydrology, make explicit what information they give and what's important to your study... and come back to this in the Discussion.

Reply: Following the previous comment, we reworked the introduction and study area section as suggested. Our aims to compare predictions of murtoo genesis to modelling, and sensitivity test the model, are laid out in the introduction and we have moved the murtoos to a separate section (Section 2). In Section 2, we detail the key developmental characteristics other murtoo studies have suggested are associated with murtoo formation. Following a suggestion from Reviewer 1, we have added a table (Table 1) which lists the murtoo developmental stages, the sedimentological evidence, and the expected model outcomes. We refer to this throughout the discussion, using the table as a signpost to do so.

Comment 2.12: Method section 3.2 – here, as above, I still don't follow what the actual research question or strategy is. What are you looking for when you "compare the GLaDS output to geomorphological evidence"? Are you trying to learn something about the landforms or the modelled hydrology? Why are you choosing a select zone in which to make model-landform comparisons? The Method should outline what you are trying to achieve and why, as well as how.

Reply: We have rewritten this section, and changed the opening sentence to echo the language of our specific aims given in the introduction so that Line 380 now reads:

"Finally, we compared the GlaDS output to the subglacial hydrological conditions proposed for murtoo genesis".

Regarding the specific zone of 40–60 km we have added text (Lines 387–390) explaining why we did so, which now read:

"Similarly, and assuming that the mapped murtoo distribution is also representative of a time-transgressive mode of origin, we examine the performance of our model within the hypothesised zone of murtoo formation (e.g., Ojala et al., 2019) by specifically isolating model nodes falling within 40–60 km of our ice margin representative of the FLDIL extent ∼12 cal. ka"

Comment 2.13: Results - most of the substance (that's developed in the discussion) comes from the baseline model. For greater clarity, consider splitting the baseline experiment results into two subsections, with the split at line 373: 4.1.1 – model behaviour; and 4.1.2 – hydrology in the hypothesised murtoo formation zone.

Reply: As suggested, we have added two subsections to our description of the baseline model, one on model behaviour and the other on hydrology in the hypothesised zone of murtoo formation.

Comment 2.14: The sensitivity reporting in 4.2 reports trends, but in neither this section of the results nor in the Discussion is there any evaluation of the sensitivity tests. In the absence of any evaluation or any narrative connecting the results to a research question, it is difficult to see what the work contributes. (In this regard, it doesn't help that all the figures are supplementary.) Your Abstract and your Conclusions state that sensitivity testing leads you to a specific parameter space for murtoo formation, but you haven't demonstrated this through any evaluation of sensitivity test outcomes. Which parameter space do you find most plausible, and why? Which parameter space best produces features that fit the geomorphological record (channels/meltwater routes and murtoos), and which best matches the sedimentological interpretation for murtoos – and do these preferred parameter spaces align?

Reply: The purpose of sensitivity testing in GlaDS is to determine a reasonable range of outputs and we have changed our abstract and conclusions. We have added discussion to this effect in our methods, results, and we also further discuss this in the discussion. GlaDS has already been extensively sensitivity tested by a number of previous authors, so we did not repeat this effort by carrying out and reporting extensive fresh sensitivity tests here. Instead, we used knowledge from contemporary ice sheets and hydrology model outputs to determine what the 'most likely', or plausible, model outcome is. For example, maximum channel lengths of less than 1 km long are not realistic, and water pressures consistently below 75% of overburden 60 km from the ice margin are not realistic. Our baseline model (particularly in terms of channel and sheet conductivity terms, which are known to be the most important in GlaDS) therefore presents the most likely output given this range of sensitivity tests that we have conducted. The most plausible parameter space is therefore the baseline run. We intended to report the dependence of our findings on these parameters in Section 4.2. Our finding that without reasonable ranges, i.e., both those found by other modelling studies and runs which remain numerically stable, our conclusions (about channel spacing, length, and the pattern of overburden) are largely insensitive to specific modelling choices, accordingly we did not spend a great deal of text explaining this. However, we acknowledge that we clearly dedicated too little text to this, and have added throughout, in the methods (Lines 340–345) we explicitly state how we arrived at our baseline model parameters:

" We set the parameters in our baseline model (default values listed in Table 2) following the default values in these studies which provide a reasonable approximation of contemporary ice sheet subglacial conditions. We then explored the sensitivity of our specific model outcomes to the available parameters (e.g., conductivity terms)"

Comment 2.15: In the Discussion, there is some repetition (summary) of results but little "so what" exploration that explicitly follows, while other parts of the Discussion are framed around how well the model performs, rather than what the model finds and what insights that gives us. The discussion of biannual patterns seems like it could be an important finding, but is confusing: one paragraph argues this behaviour is a model artefact, but the next gives a glaciological explanation for it – what's your argument? Some key ideas are lost in heavy text.

Reply: We have rewritten the discussion extensively to more clearly discuss our findings, which as per our aims is primarily focused on seeing whether GlaDS produces the hypothesised conditions of murtoo formation. We also have reworked our discussion about the biannual signal to argue that the biannual pattern arises from spatial variability in our forcing combined with flow direction divergence within the ice lobe.

Comment 2.16: The Conclusions focus on what you've done rather than what you've found, and consequently fall flat. Could you revise to e.g. We assume murtoo formation near the headward onset of channelisation, where we find the following conditions: i, ii, iii, iv... Murtoos aren't universally present where those conditions exist, which we interpret in the following way... / which we interpret to mean they also need conditions a, b...

Reply: We have rewritten the conclusions, which we do agree were not sufficiently clear before. We hope we have addressed this concern by adopting the suggested structure. Although we note that we do not assume murtoo formation near the headward onset of channelisation, within a range of our sensitivity tests our modelling results *support* this hypothesis.

Comment 2.17: 17: unless I've missed something in the manuscript, I don't recall "water depths in terrain surrounding murtoos fields" being explored or discussed – what does this refer to?

Reply: We have deleted this text in our modifications of the abstract

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Comment 2.18: 28: delete one instance of "as"

Reply: We have deleted the first as.

Comment 2.19: 34: wall melt and channelisation will lower the water pressure, which raises the effective pressure

Reply: We have corrected this as suggested.

Comment 2.20: 43-58: the specific topic(s) of this passage and the problem areas or unknowns shift back and forth, making it hard to follow what the limitations are and what "however", "instead", "yet" actually refers to in each instance. E.g. basal hydrology – topography – hydraulic properties – hydraulic connectivity (same meaning?) – back to subglacial hydrology (does "instead" contrast directly with hydraulic properties of sediments, or all knowledge of basal hydrology?) – channelised drainage extent (relation to previous points?) – bed characteristics – "basal parameters". With this rather vague term ending the passage, I'm not sure what the key point was or what the "fundamental challenge" is.

Reply: As part of the rewrite we have heavily reworked this, removing the troublesome last sentence entirely. We have rewritten the introduction with a view to 1) emphasising the importance of basal hydrology in modelling ice sheet mass loss, 2) stating such models are rarely included in full ice sheet models because of significant uncertainties as a result of overlying ice cover, 3) putting forward the idea that in order to correctly model basal hydrology, we must use all available sources of data to understand the parameter space of these models.

Comment 2.21: 50: "However" should start a new sentence in this construction. (Also line 241, 365)

Reply: The 'however' in this construction is a parenthetical aside providing additional information not essential to the main clause. It therefore does not need to be a new sentence. Nonetheless, as part of the rewrite line 50 was removed, but we have left the however on line 365 (now 451) as is. We have changed added a new sentence before the 'however' on what was line 241 (now 295) to break up the otherwise long sentence.

Comment 2.22: 59: change to glaciofluvial, as elsewhere in the manuscript

Reply: This is the only instance of 'fluvioglacial' in the manuscript we could find. We have changed it to glaciofluvial.

Comment 2.23: 60: delete "during periods of rapid ice loss" – this requires a confident and explicit link between a glaciofluvial landform and specific (high) mass loss estimates, which aren't given.

Reply: We have deleted this as suggested.

Comment 2.24: 62: define "meltwater routes" – listed here, they sound as if they are different from eskers and tunnel valleys – are these not also meltwater routes?

Reply: There is an unfortunately hazy terminology in this respect between different studies, and to avoid this we delete the reference here to meltwater routes. We introduce the concept later on and define it there (see the comment below).

Comment 2.25: 64-66: this sentence about distributed systems sits out of place with both preceding and subsequent passages dealing with channelised

Reply: In changing the intro, we have positioned this sentence about the distributed system more as a direct comparison to the abundance of channelised forms.

Comment 2.26: 65: with high water pressures (give low effective pressure)

Reply: We have made the suggested change

Comment 2.27: 73: awkward wording, suggest "assumptions about the water pressure, prescribing..."

Reply: As suggested, we have simplified this wording

Comment 2.28: 86: here you define meltwater routes (needed earlier) but this idea was formulated long before Dewald et al. 2022 – use a more appropriate reference (e.g. Utting, Peterson, Lewington, Ahokangas...)

Reply: For the specific terminology meltwater route, we have added references to Lewington et al., 2020, and Ahokangas et al., 2021 as suggested. This is now the first reference to meltwater routes, as so we leave the definition here.

Comment 2.29: 93: delete "closely associated" – it's redundant here

Reply: We have deleted this as suggested

Comment 2.30: 98-99: several ideas need further explanation or definition here: transition to – or from?; what is "semi- distribution drainage"?; why does the spatial proximity of murtoos with eskers "therefore" indicate "repeated and brief pulses of meltwater"?

Reply: We have clarified that semi-distributed represents a transition between distributed and channelised. We have also removed the reference to repeated and brief pulses of meltwater, which does have no relevance in this particular sentence.

Comment 2.31: 103 and other instances throughout: reference should be Peterson Becher & Johnson 2021

Reply: We have corrected this throughout.

Comment 2.32: 105-6: here do distal and proximal refer to across a single landform, or the field of murtoos?

Reply: We have added the word individual, although we did use murtoo (singular) throughout.

Comment 2.33: 106: for consistency with rest of the sentence structure – "proximally is comprised of glaciofluvial deposits with structures such as current ripples....".

Reply: We have reworked this sentence as suggested.

Comment 2.34: 116: Stage 2 – what about the diamict that is interbedded?

Reply: Interbedded diamicton (often trough-shaped structures in Unit 2b by Mäkinen et al 2023) primarily corresponds to late summer melt and so we have added this to Stage 4.

Comment 2.35: 123: define "upper flow regime"

Reply: Please see our changes made in response to reviewer 1's similar comment (1.12)

Comment 2.36: 131: why does laminated mud indicate sudden cessation of murtoo formation? What's the environment for that mud settling from suspension? This sounds unlikely in a subglacial environment, which is presumably where the murtoo is forming. Why should the water in channels or linked cavities be sufficiently still for suspension settling?

Reply: Laminated muds are widely documented in the marginal channels of murtoos (see Ojala et al., 2022) and are interpreted as a change in the local hydraulic connectivity of that channel. We know beneath contemporary ice that adjacent area of the bed can be hydraulically isolated and respond differently (or not at all) to localised meltwater inputs (e.g., Rada and Schoof 2018). Whether it by localised sediment deposition associated with the murtoo shape or otherwise (e.g., at the end of the melt season) the appearance of laminated muds suggests that what was a connected channel becomes isolated and the remaining water sufficiently still for suspension settling and any additional water is rerouted elsewhere. We have modified the text to suggest this which now reads:

"Finally, murtoo deposition is abruptly terminated and marginal channels are abandoned. The final sedimentation within these marginal channels is characterised by suspension settling and laminated muds, indicating that the depositional space (0.6–0.8 m) remained open and water filled but no longer hydraulically connected to the wider meltwater system (Ojala et al., 2022, Hovioski et al., 2023)."

We also note in the discussion that GlaDS is unable to represent changes in hydraulic connectivity, and that we do not observe such changes in our model outputs.

References: Rada, C. and Schoof, C.: Channelized, distributed, and disconnected: subglacial drainage under a valley glacier in the Yukon, *The Cryosphere*, 12, 2609–2636.

Ojala, A.E.K., Mäkinen, J., Kajuutti, K., Ahokangas, E., Palmu, J.-P. (2022) Subglacial evolution from distributed to channelized drainage: Evidence from the Lake Murtoo area in SW Finland. *Earth Surface Processes and Landforms*, 47(12), 2877–2896.

Comment 2.37: 132: sudden decay of what?

Reply: We have modified this sentence to now read:

"The sedimentological architecture within murtoos suggest an overall increase and abrupt reduction in meltwater discharge which indicates that murtoo development occurs within a single melt season"

Comment 2.38: 135: "comprising a main body"

Reply: We have adopted this suggestion

Comment 2.39: 139: "interbedding... is suggested to result from"

Reply: We have adopted this suggestion

Comment 2.40: 141-2: the "small size" of murtoos hasn't yet been presented...

Reply: We have added "small" to the introduction where we had already described murtoos as low-relief.

Comment 2.41: Cont:... and the "onset of channelization" is an interpretation (rather than observation) that murtoos form synchronously with eskers, but upstream. I think this point needs to be rehearsed more fully since it is a fundamental assumption you draw on in interpreting your model output. What supports synchronous formation of murtoos and downstream eskers, rather than murtoo formation at a later stage (i.e. the whole landform assemblage is time- transgressive)?

Reply: Considered in isolation, the link to eskers does not solely imply murtoo/esker formation was synchronous, however the sedimentological evidence, murtoo distance from the Salpausselkä ice marginal features, and their geomorphology all support the idea that they do not post-date eskers (e.g., formed at or nearer to the ice margin than the limit of channelisation). We have modified this section (Lines 119–126) to clarify this, which now reads:

"Murtoo morphometry (Mäkinen et al., 2017; Ojala et al., 2021), their sedimentological architecture (Peterson Becher and Johnson, 2021; Hovikoski et al., 2023; Mäkinen et al., 2023), and close spatial association with eskers, ribbed tracts, and putative subglacial lakes (Ojala et al., 2021; Ahokangas et al., 2021; Vérité et al., 2022; Mäkinen et al., 2023) is suggestive of rapid murtoo formation within broad and low conduits, at effective pressures close to zero, characterised by short sediment transport distances, and subject to repeated short pulses of meltwater, such as might be found at the spatial onset of channelisation in a 'semi-distributed' transitional drainage system (Hovikoski et al., 2023).

Murtoos are therefore unique glaciofluvial landforms, and their short formation time, small size, and apparent location at the spatial onset of channelisation make murtoos potentially important components of the subglacial system."

We go on to more fully establish these ideas in the next section.

Comment 2.42: 147: you have a study area, not one specific site that you focus on – revise this header to Study Area (and murtoo significance...)

Reply: We have revised as suggested.

Comment 2.43: 150: suggest insert "in south-central Finland" after "moraines"

Reply: We have revised this as suggested.

Comment 2.44: 155: "the lateral margins"

Reply: We have revised this as suggested.

Comment 2.45: 158: study area

Reply: In response to the previous comment (Comment 2.42:) we have revised this throughout.

Comment 2.46: 160: you refer to the other margins (next sentence) as lateral, so for clarity I would here write "bound at its terminal margin" (or distal)

Reply: We have revised this as suggested.

Comment 2.47: 167: typo drumlins

Reply: We have revised this.

Comment 2.48: 173: "complex landform assemblage" is vague, and what does its complexity have to do with surface melting?

Reply: We have clarified that the landform assemblage are glaciofluvial which now reads:

"...together with the complex assemblage of glaciofluvial landforms..."

Comment 2.49: 174: revise to "accompanied by calving into the Baltic Sea" – Greenwood et al. 2017 (also 2023, if you find this relevant) find plenty of iceberg scours indicative of an actively calving margin

Reply: We have revised this as suggested.

Comment 2.50: 176: I think it is relevant to note in this section that the ice margin was (shallow) sub-aquatic and not land-terminating.

Reply: We have added a line to this effect in our study area section (Section 2, Line 149–150) which reads:

"Shoreline data indicates that the second Salpausselkä terminated in a shallow water body ranging in depth from <5 m to ∼50 m (Lunkka and Erikkilä, 2012)"

Comment 2.51: 177-188: this passage is hard to work through – it's very difficult to visualise spatially what's being described. Some information isn't necessary, several sentences give qualifying information before we even learn what's being qualified, and language such as "association between", "borders", variably described sectors/bands/routes is vague. E.g. in the upstream trunk murtoos occur with rm + hummocks in two (?) longitudinal bands, each bounded by a band of lineations. In the northeastern band, murtoos and eskers... (describe arrangement). Downstream, where the FLDIL splays out, murtoo distribution is fragmented and terrain is more dominated by hummocky moraine. Murtoos are sparse within 40km of the Salpausselkä.

Reply: We have reworked much of this text in our attempt to trim down the 'fact-dump' in response to a previous comment. The edits, following the suggested structure, can be found in Section 2.

Comment 2.52: 195: somewhere in the methods, note that GLaDS is used with only one-way coupling, i.e. there is no feedback of hydrology on the ice sheet.

Reply: We have added a line to this effect at the end of Section 3.1 which reads:

"Finally, in the iteration used here, GlaDS is not coupled two-ways to a model of ice dynamics, and instead we prescribe an ice velocity and geometry that is not variable in response to hydrological forcing."

Comment 2.53: 203, Table 1 + other initial instances: since the manuscript deals with both an ice sheet and a water sheet, I suggest using the phrase "water sheet" where this is intended and the term first introduced, to avoid confusion

Reply: We have not adopted this suggested wording. Sheet is an accepted term within the hydrology modelling community and beyond, and we are dealing with a fixed 'ice sheet' geometry here and do not discuss the ice sheet itself in any substantial detai. Where this [ice] sheet is discussed it is always prefixed by 'ice'. Where the '[water] sheet' is discussed, it is always in reference to the distributed system However, we have added a note to the table caption (now Table 2) and to the first use of the term in the methods in order to address the potential for any confusion.

Comment 2.54: 206: "cross-sectional area of which" is a bit confusing – what does "of which" refer back to? Grammatically, it refers to edges, but do you mean channels? Are these the same thing, conceptually? (In which case, how does an edge have a cross-section area?)

Reply: In GlaDS, every edge is effectively a channel and always has a non-zero water discharge (which is why it is necessary to set a minimum threshold for classification as a 'meaningful' channel). Though each edge is fixed spatially, they do have a parameter which describes their effective cross-sectional area in the same way that a 2D ice sheet model can have an ice thickness value despite being dimensionless in the z-direction. We have modified the text to address this confusion, so that it now reads:

"Sheet elements exchange water with channels and the cross sectional area of these channels S, evolves through time due to the dissipation of potential energy" to fix the ambiguous "which"

Above, this we have also (in response to a separate comment from reviewer 1) modified the description of the mesh to clarify that the sheet is represented by elements, and channels are represented by edges. This now reads:

"The GlaDS model operates on an unstructured mesh and includes a model of distributed flow through linked cavities represented by a continuous sheet of variable thickness at mesh elements, and channelised flow—describing uniform, semi-circular Röthlisberger channels (R-channels) that are allowed to change diameter—along element edges"

Comment 2.55: 210: should be ρ_ii not p$_i$ (?) – also in Table 1.

Reply: We have corrected this to ρ throughout

Comment 2.56: 213-14: is this threshold just a matter of classification or does a different equation apply (Eqn 2) above the threshold? Please clarify.

Reply: We have clarified this line to now read:

"In GlaDS, water discharge is non-zero along all edges and so following Werder er al (2013), we set a threshold discharge of $Q_c = 1\,\mathrm{m}^3\,\mathrm{s}^{-1}$ above which an element edge is classified as a channel for our subsequent analysis"

Comment 2.57: 220: I stumbled over "surface elevation" on first reading, since you've just been referring to ice parameters. Suggest "We anticipate that the modern topography (bed elevation) is not representative... and we therefore subtract..."

Reply: We have modified this line to now read:

"We anticipate that the modern topography is not representative of bed elevation ∼12 cal. ka. Therefore, as the baseline boundary condition, z_b, we account for changes, particularly..."

Comment 2.58: 229: can you finish off this comment on base level change with a suggestion of what effect omitting GIA is anticipated to have?

Reply: We have added that this would increase the mean annual air temperature by ∼0.75 C in Section 3.1.1. We go on to imagine what effect this may have on our model in the limitations and future work section.

Comment 2.59: 233: typo except

Reply: We have rectified this.

Comment 2.60: 247: an adaptive

Reply: We have rectified this.

Comment 2.61: 274: delete "around"

Reply: We have rectified this.

Comment 2.62: 282: "nodes were pressured"? Could you add a few words to explain why this is necessary? And what does the velocity of 30 m/yr apply to? Table 1 lists ice velocity as 100-200 m/yr.

Reply: We have added three more equations and a few lines to our description of how the sheet thickness evolves through time in the method. This has allowed us to more clearly state that the basal sliding velocity controls cavity opening rates in the distributed system, and a lower velocity, U_b forces cavity opening rates to remain low, raising the pressure, and best approximating the winter state of the system (a highly pressurised distributed system). The preceding two sentences explain more clearly why we did this (Lines 347–350). They now read:

"For all model runs, to avoid overwhelming an unpressurised initial system with sudden meltwater inputs and to approximate a wintertime hydrology configuration characterised by a high pressure distributed system, we first ran GlaDS to steady state with no surface melt and fixed basal meltwater input. To guarantee the majority of elements were pressurised at the end of our steady state run, we used a low, fixed velocity of $30\,\mathrm{m\,yr^{-1}}$ which limited cavity expansion (see Equation 3)"

Comment 2.63: 291: do you mean the final configuration? "End-member" implies one alternative, at the end of a spectrum of possibilities towards another, opposing, alternative.

Reply: We have adopted the suggested wording.

Comment 2.64: 298 (and Table 1): what is the basal bump height? If the basal topography is taken from the modified DEM, what is this additional basal elevation variable?

Reply: Basal bump height is a term to account for sub-grid variability in the local elevation of the surface, beyond the resolution of the DEM. It is a term important in allowing small cavities to nucleate and allow the distributed sheet thickness to evolve through time (See the new equation 3).

Comment 2.65: 303: note that a shallow water body better replicates the palaeo setting

Reply: We have modified the text to include this, but we also note that our sensitivity testing indicates accounting for the shallow water body has limited influence on the model outcomes.

Comment 2.66: 311: "masked" is ambiguous – did you select these nodes for analysis or exclude them?

Reply: Masked has been replaced with "isolated"

Comment 2.67: 322: (overburden$_{\%}$

Reply: we have updated this to $overburden_{\%}$ throughout the manuscript.

Comment 2.68: 327: does the median include or exclude the 10 years of adjustment time?

Reply: It does not include the adjustment time, but the adjustment time should be 5 years (as stated in the caption for Figure 2) we have corrected this.

Comment 2.69: 327: suggest deleting "pressure expressed as a percentage of overburden" – you've already defined overburden%

Reply: We have removed the suggested text.

Comment 2.70: 328: should be Q_c for channel discharge

Reply: We have modified this.

Comment 2.71: 329: since observed murtoos are shown on the plots, I would clarify that the black solid lines are modelled channels, not observed/mapped ones.

Reply: We have made the suggested change.

Comment 2.72: 331-3: the comments about in/efficient drainage here veer towards interpretation, rather than straight reporting of results. It would make it easier to read and digest if you remove these comments and just report overburden%. I also suggest starting a new paragraph after the overburden sentences – there is a lot of info in this paragraph and splitting it up would make it easier to digest.

Reply: We have removed the references to the efficiency of the drainage system and split the paragraph after our reporting of $overburden_{\%}$.

Comment 2.73: 333-6: "Towards the ice margin" and "60km from the ice margin" in the same sentence is confusing. I can't follow the description, without looking at the figure. The next two sentences also confused me, since the difference in channels appears to suggest a contrast between summer and winter, but spatially I'm not sure whether you're trying to express a contrast or not. Does the following (paraphrased) work? qs is orders of magnitude higher in the zone within 60km of the ice margin than further upstream, in both summer and winter. In summer, high qs is found in patches within the channelised zone. In winter, qs is much lower, and its peak is shifted to a zone headward of the uppermost channel reach, 40-60km from the margin. (?)

Reply: Addressing the first point of this, we have changed line 415 to now read:

"Throughout the year, q_s sharply decreases 60 km from the margin"

To the second point, the following two sentences now read:

"In summer, areas of high q_s (approaching $10^{-1}\,\mathrm{m}^2\,s^{-1}$) are found between channels 30–40 km from the ice margin which we interpret as arising due to channels draw down water from surrounding areas. In winter, q_c is lower throughout the domain, and the highest sheet discharge ($\sim 1 \times 10^{-3}\,\mathrm{m}^2\,\mathrm{s}^{-1}$ Figure 2B) is found in patchy areas within 60 km of the ice margin"

which we hope addresses the confusion.

Comment 2.74: 337: as earlier, comments about channels confuse the issue, which is about velocity in sheet water. What does "reflects the concentration of drainage" mean, in this context? It is an interpretation of a mechanism that would account for a particular trait of Vw, but would help if it were explicitly presented this way. Focus on presenting the patterns in the main variable, and being clear in expressing spatial relationships – I'm not sure if you are reporting a contrast between summer and winter or not, in either spatial terms or velocity magnitude. I suggest also using the same units/terms as used in the figure, i.e. 15 x 10-4 rather than 1.5 x 10-3.

Reply: As described above, discussing q_s we have added the phrase "...which we interpret as arising due to channels draw down water from surrounding areas" and we have deleted references to channelisation entirely from our discussion of V_W and fixed the units as suggested.

Comment 2.75: 341-2: superscript s-1. Also lines 348, 352, 369 – check elsewhere.

Reply: We have corrected all instances where this error occurs

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Comment 2.76: 342: Can you be more specific about the channels that persist over winter – how many? Or what proportion of the summer total?

Reply: We have added this as suggested to Line 423.

Comment 2.77: 343/4: suggest move the two sentences about Fig 3A to here, and open with it. By closing the reporting of fig 3 with a "nothing to see" case, all the interest and power of the findings about biannual signals is lost – much better to end with these interesting cases.

Reply: We have moved the two suggested sentences, making minor text changes in order to smoothly accommodate them.

Comment 2.78: 351: sharp increase in overburden% - not overburden pressure. There are numerous cases in the next paragraph(s) also where "overburden" or "overburden pressure" ought to be "overburden%", or alternatively "water pressure".

Reply: We have modified this throughout the manuscript in order to be more internally consistent.

Comment 2.79: 351: "trends towards" – can you say if this is an increase or decrease or values hover around one value?

Reply: We have replaced "trends" with "reduces" in order to make our meaning clearer.

Comment 2.80: 353: with only a small increase in overburden% ... and little taken up by sheet flow?

Reply: We have added this as suggested.

Comment 2.81: 356: this is confusing, suggest "with no overwinter channels evident in the central area while lateral margin channels persist over winter".

Reply: We have replaced the confusing text with:

" In any given year, channels will persist through winter in either the central third of the lobe or in the remaining two thirds of the lobe"

Comment 2.82: 357 + throughout: I strongly suggest sticking to expressing pressure either as water pressure or as overburden%. It is enough with two alternative ways of expressing the same thing. There is no particular need to express the same thing in terms of effective pressure – it's confusing at best and erroneous at worst. Replace here with e.g. "a persistent area of high basal water pressure (overburden% approaches and exceeds 100%)..."

Reply: In response to point 2.78 we have changed this throughout.

Comment 2.83: 360: increase in water pressure to 120% of overburden

Reply: The symbol was intended to convey nuance in the pressure change, we have replaced this with "...pressure up to a maximum of approximately 120 %..."

Comment 2.84: 360: "associated with" is vague, it's not clear what you actually mean in either case you refer to. Suggest delete this phrase here and be more specific later, where necessary. E.g. node 3842, located on a channel onset site. E.g. node 16402, located in between channel onset areas.

Reply: To address this point, we have made several changes. The modified now reads:

"demonstrates the seasonal evolution of two nodes in this area, each nearby to channel systems"

The phrasing about node 3,842 (already modified in relation to comment 1.17) now reads:

"At node 3,842, chosen to as representative of surrounding nodes at the onset of a channel"

and finally, the line about node 16402 now reads:

"which is located $\sim$0.7 km from a murtoo field between the onset of channels."

Comment 2.85: 371: "remaining at 80% of overburden" – no need for another way of expressing pressure!

Reply: We have modified this line to read: "Here, the system is effectively inert, with $overburden_{\%}$ remaining $\approx$ 80%"

Comment 2.86: 373: "To explore behaviours potentially associated with murtoo formation..." ??

Reply: We have adopted a slightly modified version of this suggestion. The lines (458–459) in question now read:

"We explored behaviours potentially associated with murtoo formation by focusing on nodes 40–60 km from the ice margin, within the zone thought to be associated with murtoo formation $\sim$12 cal. ka..."

Comment 2.87: 375: "...of a mapped meltwater route that also hosts...". Can you also give n=? in each case with the definition of the group (i, ii, iii)? You should also note again here that these meltwater routes are taken from Ahokangas et al., and note how you treat eskers (part of a meltwater route, or separate?).

Reply: The n = values are reported in the methods already, and on the line after but we have repeated them here. We have also added the suggested clarification so that it now reads:

"As noted in Section 3.2, group ii may also include eskers ('channelised routes' in Ahokangas et al., 2021) as these are often coincident with meltwater routes."

Comment 2.88: 377: does "throughout the year" mean "mean annual"?

Reply: We have added "...and *at every point* throughout the year". We hope this clarifies that throughout the year means at every point during a year. It is true that "mean annual [$overburden_{\%}$]" is also true in our data, however, that is not what Figure 4 (now Figure 5) explicitly shows.

Comment 2.89: 382: is the statistical difference between groups significant for each of the 4 variables, considered separately? Or in combination?

Reply: We have added "for each of the four parameters discussed here" to clarify that it is the former case.

Comment 2.90: 388, 393, 396: could you be more specific where you state a variable is "significantly higher" or lower? In this context, it is easy to misinterpret significantly higher for substantially higher; a difference can be small, but statistically significant. What is meant here? Generally, the blue/orange variables in Fig

4, 5, A32 look pretty similar to each other, so any differences seem like they would be small, but statistically significant – can you express this more accurately and specifically in the written text?

Reply: We have rewritten this paragraph to clarify that the significance is exclusively statistical and is not a comment on the value of the difference. To address that point, which we agree is a useful one to make, we also have added an approximate value of this difference. This updated paragraph can be found between Lines 472 and 488.

Comment 2.91: 400: what do you mean, “to best apply the model to a palaeoglacial setting”?

Reply: We have removed this text as part of our discussion overhaul

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Comment 2.92: 404: “water is efficiently evacuated” (or, water pressure is dissipated??)

Reply: We have changed this line (Line 495) to now read: “no channels longer than one km are formed and water is instead more readily transmitted through the distributed system at relatively low pressures ($overburden_{\%} < 60\%$)”

Comment 2.93: 06 + throughout: as above, express pressure in terms of either water pressure or overburden%

Reply: We have changed this specific case to “high $overburden_{\%}$” and have adopted the same throughout

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Comment 2.94: 408: “At a minimum sheet conductivity of 10-5...” – symbol not needed since you wrote out in words

Reply: We have deleted the errant “of” but left the mathematical symbol in the brackets for consistency with how we report all sensitivity tests.

Comment 2.95: 411: KC should be kc (small letters)

Reply: We have made this change.

Comment 2.96: 411 + throughout Section 4.2: Table 1 gives units for ks as m7/4 kg-1/2 and for kc as m3/2 kg-1/2. These are muddled and inconsistent (7/4 or 3/2) through the Section.

Reply: We have fixed the two incorrect units for k_c to be $m^{3/2}\,kg^{-1/2}$

Comment 2.97: 412: “low pressure” – which variable? (Also line 429)

Reply: We have addressed each of these by fixing the reference to $overburden_{\%}$ only.

Comment 2.98: 420: “the location of which” – what does “of which” refer back to?

Reply: Line 510 now reads: “...approximately 25 channels extend up to 50 km from the ice margin. The location of *these channels* closely follow the position of high discharge moulins...”.

Comment 2.99: 448-455: superscript yr-1. Note that elsewhere in the text the unit for years is given as a.

Reply: We have fixed this to be superscript, and also fixed the one erroneous year notation we could find.

Comment 2.100: 452: what do you mean by "more tightly constrains the observed summer water pressure..." ? I don't follow.

Reply: We have modified this (Line 543) to now read: "In addition, the transient velocity results in a spatial distribution of $overburden_{\%} \approx 100\%$ in stronger agreement with the contours of 40–60 km from the ice margin compared to the baseline scenario"

We hope this addresses the uncertainty.

Comment 2.101: Section 4.2 overall: see comment above (problematic text) – this sections reports the end member behaviour and selected middle option of the range of sensitivity tests, but doesn't anywhere evaluate these outputs. How stable/robust are the results presented for the baseline case? What do we learn from these sensitivity tests?

Reply: As described above we have now added description into Section 4.2 demonstrating which outputs are less realistic and therefore pointing towards the baseline model being the most appropriate choice. Sensitivity tests are an important parts of exploring the responses of the model and therefore we argue these outputs are a useful addition

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Comment 2.102: 5.1 header: seasonal drainage is implicit in some parts of your discussion in which you compare winter and summer conditions, but seasonality isn't a trait you explicitly dwell on and explore. This doesn't seem a particularly appropriate header (or could be made more so by extracting aspects of seasonality for dedicated discussion).

Reply: In rewriting the discussion we now have several new section names, which are:

"5.1 Catchment-scale hydrological configuration"

"5.2 Comparison with glaciofluvial landforms"

"5.3 Comparison between model outputs and mapped murtoo locations"

and

"5.4 Limitations and future work"

Comment 2.103: 460: "...demonstrated by Kirkham et al., who evaluated hypotheses..." – in its present formulation, I moved onto the next phrase + reference as a new item, not connected to Kirkham.

Reply: We have removed this text, which on reflection read more like an introduction. The reference to the work of Kirkham et al., remains in the intro but this specific text no longer features in the discussion.

Comment 2.104: 472: do papers by Hewitt, Schoof etc not include transitions between channelised + distributed systems (and compare to landform-based predictions of geometry)?

Reply: As above, we have removed this section, but in this sentence we were explicitly referring to the inherently channelised area-routing algorithms and their application in the palaeo setting. Hewitt & Creyts (2019) (A model for the formation of eskers; *GRL*) does describe a model esker formation, however it concerns a single channel, and does not include a sheet component to the model. Hewitt et al., 2011 does include a continuum description of the distributed system coupled to a single channel (and indeed is what GlaDS is based on, together with the Schoof 2010 description of channelised drainage). They do make a comparison to esker spacing based on scaling relationships. We have added this to the introduction, to which we have also added references to the work of Boulton, Hewitt, and Beaud.

Comment 2.105: 474: does “as” mean because, where, when, ... in this context?

Reply: We have modified “as” to “where” to clarify we are referring to a spatial transition

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Comment 2.106: 475: what does “define a parameter space for basal hydrology models” mean, specifically, in this context?

Reply: We have significantly rewritten this section and as such this line no longer exists.

Comment 2.107: 479: I’m having trouble visualising “vertically arcuate along a comparable path to the surface” – can you rephrase?

Reply: We have changed this on Line 562 to:

“...are arcuate at a similar curvature to the surface slope...”

Comment 2.108: 483: delete “Instead” – doesn’t seem like there’s a contrast

Reply: We have changed “instead” to “accordingly” and also tweaked the following sentences (Lines 570–571) so that they now more clearly communicate the idea that 40–60 km from the ice margin is the likely area of murtoo formation

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Comment 2.109: 485: “...gradients limit the growth of channels...” (?)

Reply: We have added “hydraulic potential gradients” to clarify the effect low surface gradients have on the basal hydrological system.

Comment 2.110: 486: permit (delete s)

Reply: We have adopted this change

Comment 2.111: 490: “Across the full domain <70km from the margin” sounds like you’ve got two mutually exclusive areas. I think you perhaps mean within the width of the domain and within 70km of the margin ?

Reply: We have removed this specific text as part of the discussion rewrite and have been more precise in our description of processes within this area.

Comment 2.112: 494: suggest deleting “when plotted as a summer average” and change previous line to “summer water pressure”

Reply: We have removed this specific text as part of the discussion rewrite, but anywhere we discuss summer water pressure we have been sure to adopt the suggested wording.

Comment 2.113: 494: Elsewhere in the downstream zone, water pressure ... ?

Reply: We have removed this specific text as part of the discussion rewrite.

Comment 2.114: 496: insert ii) since you started a list with item i) – and ii) the limited... dropping below 75% of overburden

Reply: We have added the missing "ii)" on line 590.

Comment 2.115: 502 and next two paragraphs: these read more like a summary of the results than a discussion. What's new, what key insights have you gained into murtoo formation and/or murtoo vs channel hydrology? What about all the murtoos upstream of the 40-60km band?

Reply: In rewriting the discussion extensively we hope to have addressed the suggestion that this is just a summary of the results, instead we focus on exploring GlaDS ability to reproduce the conditions associated with murtoo formation. We have also added a line (586–588) about the implication for Murtoos beyond the 40–60 km band—we conclude these formed as the ice margin retreated, please see our response to the previous comment on this subject above.

Comment 2.116: 506: if "channel discharge" is only ever close to but not exceeding the threshold for being designated a channel, how does that entity have a "channel discharge" and be measured as Qc? In fact, I should have asked this earlier with the presentation of results and figures e.g. Fig 3, 4, 5 – if a channel isn't a channel until it has a discharge of 1 m3/s, then how can a channel have discharge Qc below that?

Reply: We have addressed this query in response to Comment 2.56 concerning whether or not different equations apply when channel discharge exceeds $1\,\mathrm{m^3\,s^{-1}}$. But to address it again specifically here, every edge of the mesh in GlaDS is allowed to have a non-zero discharge at all times, so is in effect *always* a 'channel' of sorts. However only a few edges will ever reach an appreciable discharge and so it is necessary to set a threshold above which an edge (with non-zero discharge) is said to be a meaningful channel. This threshold is arbitrary, but in setting it we follow the work of every other paper which has used GlaDS to explore drainage dynamics.

Comment 2.117: 515: what is it that suggests that cavity expansion necessary for murtoos is captured in your model? Expand on "which suggest". I think you also need to expand on the argument that murtoo formation requires the existence of small channels, as a pre-requisite. Is that the case, or have I extracted the wrong idea from what I think you're alluding to? (This is sort of stated in the introduction to murtoo sedimentology, and on the previous page you refer to cavity enlargement, but does a cavity or conduit mean – in model terms – a small channel?)

Reply: To the first point, we have added an explicit link to the maximum cavity height of 1 m as suggested by Hovikoski et al., 2023 and Mäkinen et al., 2023. The text in question (Lines 580—581) now includes:

"close to the maximum cavity height of 1 m inferred boulder distributions in the upper slope of murtoos".

We hope to have addressed the second portion relating to the hypothesis of limited cavity expansion throughout the text through our other modifications.

Comment 2.118: 526: "such landforms relating to meltwater drainage have been mapped..."

Reply: This specific wording was removed in the discussion rewrite

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Comment 2.119: 531: there is quite a lengthy history of data-model comparison with regards to eskers, and some of that work is likely relevant here (also in the opening of the Discussion, where you also address data-model comparison) e.g. papers by Boulton 2007-2009, by Hewitt 2011, Hewitt & Creyts 2019; work by Flavien Beaud 2016, 2018 among others

Reply: We have added references to several of these papers in the introduction, and the specific text referenced here has been removed as part of the discussion rewrite. However, we do continue to make the point that mapped glaciofluvial make a useful means of comparison for our model outputs. The new text can be found on Line 630 to which we add references to the work of Boulton et al., 2009

Comment 2.120: 535-6: repetitive – suggest "...grid-based models, but the exact location...". "The spacing of channels, however, remains robust...". Delete the early part of the second sentence.

Reply: We have removed this specific text within the discussion rewrite. We continue to include text about this subject (the mesh dependency of channels) at the end of section 5.2.

Comment 2.121: 539: does this contradict the earlier result that I flagged in my general comments, that channel discharge is higher outside mapped meltwater routes?

Reply: We have corrected the earlier comment in response to Comment 2.6. There is no longer a contradiction.

Comment 2.122: 541: presumably the baseline run was selected in order to match the Greenland geometry, though – it's not really a result that this is the case, is it?

Reply: The baseline run was selected because it provides the most reasonable intermediate hydrology outputs e.g., channels of a length comparable to contemporary ice sheets, water pressures not far below overburden in winter. These parameters are the same as baseline models applied to contemporary ice sheets that have been tested against *in situ* data and therefore appear to provide a reasonable approximation of ice sheet subglacial conditions. The ice sheet geometry itself is not tested in the sensitivity tests, only the hydrology parameters that have been shown in other studies to be most important for drainage development (i.e. sheet and channel conductivity). We have changed this text to address this (Line 605–608):

"However, without extant ice in the FLDIL against which to test our models, we are unable to fully determine the correct parameters for our FLDIL domain. As a result, the baseline model was parameterised following existing work on contemporary ice sheets (see Section 3.1.2). As expected, the baseline model provides a range of seasonal water pressure and channel lengths that are similar to models of contemporary ice sheets validated with geophysical methods (e.g., Dow et al., 2020)"

Comment 2.123: 542: consistent language with earlier: "In the baseline model run, small channels..."

Reply: We have removed this specific text within the discussion rewrite. We have attempted to be consistent with this in the new discussion

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Comment 2.124: 558: "between our model results and the observations beneath Greenland" ? – "those" implies model results beneath Greenland...

Reply: We have made the suggested change and also added a specific reference to observations on Line 612, which now includes Van de Wal et al., 2015.

References: Van de Wal, R.S.W., Smeets, C.J.P.P., Boot, W., Stoffelen, M., Van Kampen, R., Doyle, S.H., Wilhelms, F., van den Broeke, M.R., Reijmer, C.H., Oerlemans, J. and Hubbard, A., 2015. Self-regulation of ice flow varies across the ablation area in south-west Greenland. The Cryosphere, 9(2), pp.603-611.

Comment 2.125: 561: "in these systems" – does "these" refer back to the distributed system, channels, or both? Be specific.

Reply: We have replaced "these systems" with "in the distributed system" on Line 614

Comment 2.126: 571-2: this sentence repeats the opening sentence of the paragraph, but more specifically and concisely – suggest delete the opener or replace it.

Reply: We have deleted the opening sentence as suggested.

Comment 2.127: 572: overburden%

Reply: We have corrected this.

Comment 2.128: 576: as above, if the "channel" has discharge orders of magnitude below the threshold to be a channel, how can these be considered channels?

Reply: The order of magnitude was an error, and we have updated the text to clarify that the channels seen to persist through winter are very close to the arbitrary threshold we apply to channel discharge, and that a slightly higher threshold would have excluded winter channels. Lines 701-702 now read:

"Channels persisting through winter months tend to operate at very low discharges of 1–3 $m^3 s^{-1}$, and would not be categorised as channels with a higher threshold"

Comment 2.129: 583-4, + preceding + following paragraph: you argue biannual behaviour in the model output is a consequence of internal model dynamics (i.e. an artefact) yet then offer an explanation for it in glaciological terms. What is your argument? And what is the relation to a patchy murtoo distribution?

Reply: We have attempted to clarify the relevant paragraphs to say that the biannual forcing arises through a combination of our fixed climate (giving rise to the repetitive biannual signal), but that the spatial distribution of these channels arises because of divergences in the ice flow direction, which is also important in the generation of glaciofluvial features in the FLDIL.

The relation to the patchy distribution of murtoos is as follows: meltwater routes are distributed evenly across the FLDIL, but murtoos are confined to the outer two thirds of the lobe and absent from the central third. Every winter, channels persist either in the central lobe or the outer two lobes, alternating each year. These winter channels affect the water pressure into the following summer. In winters where channels in the central third of the lobe persist, they influence the overburden of meltwater routes upstream of these channels in the following summer, but they have no affect on murtoo routes because there are none present in the centre of the lobe. In the following winter, winter channels affect summer water pressure in both murtoo routes and meltwater routes, giving rise to the significant differences between murtoo routes and meltwater routes considered over the whole of our model run.

We have updated the paragraphs in the discussion (Section 5.3) to address this observation, which is likely routed in both model setup and glaciolgical reality.

Comment 2.130: 623: study area

Reply: We have made this change, now found on Line 781

Comment 2.131: 631: while you have performed some sensitivity runs, neither your Results nor Discussion section explicitly analyses or evaluates those outputs, so it is false to say that "By sensitivity testing... we demonstrate...". You have selected the baseline run as your favoured answer.

Reply: When rewriting the conclusion we removed this specific phrase, but to reiterate the other comments regarding sensitivity testing, our baseline was chosen because it provides the most reasonable/plausible hydrology outputs including winter pressures and channel lengths when compared to contemporary ice sheets which have been tested against *in-situ* data.

Comment 2.132: 635: that murtoos arise because of "consecutive years of elevated meltwater volumes" has not been clearly demonstrated or argued in the Discussion. Your Discussion has presented biannual behaviour linked to winter persistence of some channels, and implied some related cavity/channel growth behaviour (although see above, 583-604), but your Discussion does not explicitly tie this to murtoo formation. "Elevated meltwater volumes" is also an ambiguous phrase: volume is not considered, as a model variable – do you mean pressure, discharge or velocity?

Reply: We have significantly changed this section and we hope to have addressed this comment in doing so. The logic regarding elevated meltwater volumes was incomplete and speculative. We have removed the text in question entirely.

Comment 2.133: Table 1 Channel conductivity: the main text refers to experiments with values 0.001, 0.05, 0.5. Ice velocity: given the inference of ice streaming, a tested velocity range of 100-200 m/yr seems rather limited, and low – why?

Reply: We have changed the table values to the $\times 10^x$ notation in the table throughout, and corrected the testing range. We do not (intentionally) infer ice streaming within the FLDIL. Instead, velocity was chosen to be comparable to melt-season surface velocity in land-terminating sectors of the Greenland Ice Sheet (e.g., Tedstone et al., 2015). We have added a sentence in the methods (Lines 367–368) clarifying the rationale for our chosen range of velocity values. This reads: "For basal velocity, we tested values between 100–200 $m\,yr^{-1}$ chosen to be comparable to GPS measurements of surface velocity across land-terminating sectors of the Greenland Ice Sheet (e.g., Tedstone et al., 2015)."

Reference: Tedstone, A.J., Nienow, P.W., Gourmelen, N., Dehecq, A., Goldberg, D. and Hanna, E., 2015. Decadal slowdown of a land-terminating sector of the Greenland Ice Sheet despite warming. Nature, 526(7575), pp.692-695.

Comment 2.134: Fig 1 I don't think that slope visualisation of terrain models is the most intuitive way to illustrate and distinguish murtoo morphology, especially for those not accustomed to working with terrain models or unfamiliar with these landform types. I would suggest a conventional hillshade, with illumination best suited to each panel. Also consider rotating the triangle symbols in the direction of murtoos? Caption: change to study area. A) "Murtoo fields identified by Ahokangas...". D) If murtoo fields are adjacent to the esker, then murtoo fields have not undergone an "abrupt downstream transition" to an esker – please revise, the description of a transition is misleading. E) "within the centre" – delete "at"

Reply: Figure 1 does not show a slope model. Each panel shows a multi-directional hillshade optimised for the mapping of glaciofluvial landforms in the region as described in Ahokangas et al., 2021. We have clarified this in the caption and adopted the other suggested caption modifications. The end of the caption now reads:

"All panels show a multi-directional oblique weighted hillshade based on 2 m LiDAR data (see Ahokangas et al., 2021, for details)."

We have added rotated triangles and also added ice flow directions as suggested by reviewer 1.

Comment 2.135: Fig 2 give units for qs, Vw and Qc. In the caption, move the sentence about channels as black lines to after (C), and say that this is the case in panels A-C. (Not just A). murtoo triangles aren't visible at this scale (appear as dots), nor is channel scaling for discharge (appear as black lines). I'd remove the murtoo/channel legend, and simply add to the last line of the caption that black dots are murtoo fields, from (which ref?).

Reply: We have made all of the suggested changes to what is now Figure 3.

Comment 2.136: Fig 3 label A, B, D, E next to coloured dots on panel C. consider flipping the inset graph so that distance = 0 to the right, for consistency with the map plot? caption: opening sentence, suggest "at four nodes over model years 15-25 in the baseline model run". And panel C) ... "in model year 19 (arbitrarily selected)." caption final sentence, should be qs

Reply: We have made most of the suggested changes to what is now Figure 4, we have not changed the orientation of the inset graph.

Comment 2.137: Fig 4 give units for qs, Vw and Qc colours: the red and orange are rather close, and the pink too – could you choose more distinct colours? Also, the legend shows solid pink while the graphs show dashed, and the legend describes as dashed purple. Please revise one or an other to be consistent. "Meltwater routes that do not contain murtoo fields (murtoo hosting)"... "routes which do host murtoos (murtoo free)" – both of these phrases in the caption are internally inconsistent. They are also (possibly?) inconsistent with the legend. Please revise.

Reply: We have changed the colours, and updated the caption in what is now figure 5, making sure to fix the colours for all other figures using this scheme. We have also revised the murtoo/meltwater route naming convention. We also have corrected the residual difference component of panel D, which had previously been a duplicate of the residual difference graph from panel B.

Comment 2.138: Fig 5. give units for qs, Vw and Qc same errors for murtoo hosting / murtoo free – the caption is internally inconsistent, and please check consistency with legend. I note that I have not checked legends/captions for all appendix figures, but at least A32 has the same error – please revise, and check other figures thoroughly.

Reply: We have made the suggested changes, and changed Figure A32.

Comment 2.139: Fig 6 caption line 2: delete "against"

Reply: We have deleted this against.