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 second round of reviews, which have helped to further improve our manuscript. Below we address the general comments and then list our response to each of the specific and technical comments in turn.

General comments

Comment 2.1 I think this is a much improved manuscript from the version I saw last (first version). The logic of the paper is much better presented, clear to follow, and much of the heavy language and confusing constructions have been addressed and resolved. I think the work is a good illustration of how the geomorphological record of former glaciation can be used to test ice sheet/hydrology models and give confidence in their wider utility, as well as shed light on patterns observed in the landform record.

I see that you've given some attention to refining how you formulate your overall aim, and I think that it now works ("explore the ability of GLaDS, a process-based subglacial hydrology model, to explain murtoo formation in both space and time"). I read this, and your manuscript, as testing whether GLaDS can generate the right conditions in the right place for murtoo formation, and then learn something about how these conditions compare to other meltwater landforming settings. I think you could be a bit clearer about what your comparisons can provide, and what they can't. The comparison of distributions is somewhat circular because of the widespread and time-integrated distribution of murtoos in Finland: murtoos are hypothesised to from under high pressure in an area near the onset of channelisation, and the model produces a high overburden zone upstream of channelisation. We could conclude the model is good. Yet, if the conceptual model is wrong and they can form under different settings, then we could look to a different zone in the model output, still find murtoos, and therefore also conclude that the model is good. We can't confirm the hypothesis. In this context, the comparison of model seasonal behaviour against sedimentology is really important, because there are multiple predictions in a sequence that the model must achieve. A good match for sedimentology makes this a genuine test of the physical and conceptual models, and I think some words to this effect in section 5.2 and/or in the conclusions would strengthen the paper.

I think Section 5.3 needs a bit more work – there are passages I still find confusing, and passages where I think important findings are lost that should be brought forward more clearly. Some of these findings draw from section 5.2 also. In particular, I'm concerned that a key argument that is made, that there is a significant biannual difference between murtoo routes and meltwater routes, mixes two sets of findings and hasn't been explicitly demonstrated itself. I expand on this further below with some suggestions for how to improve the clarity of the arguments made in these sections. I think an additional figure that illustrates the spatial distribution of contrasting modelled drainage behaviours would really help the narrative (also explained further below).

Reply We thank reviewer 2 for their positive appraisal of our heavily reworked manuscript. We are particularly glad that our overall writing is now clearer.

We appreciate the suggestion for Section 5.2 and the circularity raised by comparing murtoo distribution to model outputs alone which sedimentology addresses. We have added the following text to the opening of Section 5.2 (Line 389–394):

"The widespread and time-integrated distribution of murtoos throughout our model domain complicates model validation as murtoo formation conditions remain uncertain. The ability of GlaDS to reproduce the hypothesised spatial pattern of murtoo formation (i.e., summer $overburden_{\%}$ ≈100% 40-60 km from the ice margin) alone cannot definitively confirm or refute the hypothesized formation process because murtoos are distributed across our model domain. In this context, comparing seasonal model evolution to murtoo

sedimentology (e.g., Hovikoski et al., 2023; Mäkinen et al., 2023) becomes particularly important as there are multiple predictions in sequence that the model must achieve (Table 1)."

We respond to the specific suggested changes in section 5.3 below (Comment 2.45–Comment 2.57), but in short we have adopted a more cautious approach to the overwinter channels and their biannual signal because we are unable to rule them out as a model artefact. We have also added a supplementary figure in order to illustrate the spatial distribution of winter channels.

However, we emphasise that in removing some of the text about spatial overlaps, this does not alter our conclusions. GlaDS continues to reproduce the overall pattern of drainage beneath the FLDIL and also closely matches the murtoo developmental phases from murtoo sedimentology. Instead, Section 5.3 ends by suggesting some future research directions so that GlaDS may better reproduce the spatial pattern of individual murtoo routes.

Specific comments

Comment 2.2 14-17: "Our model outputs match many of the predictions" would be a stronger (and appropriate) statement than the rather vague "represent". I think you can say more here about what you've actually found and discussed, and importantly, include that the model matches what we would expect from murtoo sedimentology (as above). For example: "Our model outputs match the general distribution of channelised drainage landforms such as esker and meltwater routes. Many of the predictions for murtoo formation are produced by the model, including the location... and, importantly, the seasonal sequence of drainage conditions inferred from murtoo sedimentology. These conclusions are largely robust to a range of parameter decisions, and we explore seasonal and inter-annual drainage behaviour associated with murtoo zones and meltwater pathways"

Reply We thank the reviewer for this suggestion to strengthen our writing in the abstract. We have adopted the spirit of this suggested change, changing the suggested text slightly so that Lines 14–18 now read:

"Our model outputs closely match the general spacing, direction and complexity of eskers and mapped assemblages of features related to subglacial drainage in 'meltwater routes'. Many of the predictions for murtoo formation are produced by the model, including the location of water pressure equal to ice overburden, the onset of channelised drainage, the transition in drainage modes, and importantly, the seasonal sequence of drainage conditions inferred from murtoo sedimentology. "

Comment 2.3 32: "analyses... have been applied"

Reply We have fixed this as suggested on Line 33.

Comment 2.4 39-40: to better fit the development of ideas in this paragraph, I would move the phrase beginning "potentially including" to line 45, which would then flow "...ideal targets against which to evaluate subglacial hydrology models, potentially including processes variable at sub-annual scales and across the distributed-channelised transition". (Note, wherever it is place, suggest replace "those" with "processes", for clarity.)

Reply We have adopted this suggested change, including changing those to processes. This change can be found on Lines 39–46.

Comment 2.5 46: landform genesis uncertainty arises from both fundamentally different concepts of how a landform is formed, and also spatial and temporal scales of formation.

Reply We have changed Lines 47–48 as per this suggestion.

Comment 2.6 59: odd punctuation: write out "length and spacing scaling relationships"

Reply This was a character padding quirk breaking length/spacing where it should not have. We have spelled out "length and scaling relationships" on Line 59–60 as suggested to correct this.

Comment 2.7 65-67: suggest deleting "modern" from "modern subglacial hydrology models" – unless there is a particular reason for this word, to compare with others?

Reply Modern was intended to communicate the difference between process based hydrology models and more simple routing algorithms. We have reworked Lines 61–67 to add a parenthetical definition of 'modern' in this context. The text in question now reads:

"In contrast, modern subglacial hydrology models (i.e., those capable of resolving transitions between distributed and channelised drainage in both space and time) are widely applied to contemporary ice sheets (e.g., Flowers, 2018; Indrigo et al., 2021; Dow et al., 2022; Sommers et al., 2022; Ehrenfeucht et al., 2023). However, despite the critical need to evaluate and improve modern subglacial hydrology models using all available sources of data (Dow, 2023), we are not aware of previous work which has evaluated the ability of such models to reproduce the subglacial conditions (e.g., water pressure, channel location) associated with glaciofluvial landform formation."

Comment 2.8 73: I suggest changing the wording to "and likely represent time-transgressive formation over decades-millennia" – the current wording suggests that there is esker building taking place along the full length of a >10km esker over millennia, which I don't think is what is meant.

Reply We have adopted this suggest change, which can now be found on Line 74.

Comment 2.9 74: typo – glaciofluvial

Reply Thank you for identifying this typo, which we have corrected.

Comment 2.10 84: published as Peterson et al. 2017

Reply We have changed the bibliography reference throughout

Comment 2.11 91: "closer than 40-60km to the..."

Reply We have adopted this suggest change, which can be found on Line 91.

Comment 2.12 93-116: in this paragraph, could you add references to the numbered developmental stages in Table 1, at the relevant point in the text? You begin this way (line 97: represents the first stage...), but it would be helpful throughout. E.g. change text in parentheses on line 99 to "developmental stage 2: Table 1", and thereafter at the relevant point just (Stage 3), (Stage 4)...

Reply Thank you for this suggestion, which we have adopted throughout the paragraph starting on Line 94.

Comment 2.13 107: does "disappearance" mean erosion or non-deposition?

Reply We have added a parenthetical explanation so that Line 109 now reads:

"evidenced by a disappearance of sorted sediment (interpreted as non-deposition rather than erosion)"

Comment 2.14 111-113: there are 3 phrases here beginning "final" or "finally" – suggest rephrasing so that only one event is "final".

Reply We have corrected this as suggested

Comment 2.15 132: "however" in this construction is not being used as a parenthetical aside. Suggest change to "...in the centre of the ice lobe where thin sediment cover may have limited..."

Reply We have corrected this as suggested, which may now be found on Line 134.

Comment 2.16 158-162: I'm not sure these sentences accurately describe what you present in the results and discussion. In my view, you: examine catchment-scale hydrology parameters and compare to murtoo formation predictions and distribution of channelised landforms (eskers); you specifically explore seasonal and inter-annual drainage parameters in the zone where murtoos are hypothesised to form; you investigate differences in modelled hydrology between observed murtoo and meltwater routes, and where no glaciofluvial landforms exist; and you test the sensitivity of your results to a range of parameters.

Reply We have adopted a modified version of this, so that Lines 159–166 now read:

"We examined catchment-scale hydrology outputs and compare these to murtoo formation predictions as well as the distribution of channelised landforms (eskers) in the FLDIL. At individual nodes, we compared the evolution of nodes across our domain against the developmental phases recorded within murtoo sediment excavations (see Table 1, Hovikoski et al., 2023). We then explored seasonal and inter-annual model outputs in the area 40–60 km from the ice margin, at the upglacier limit of channelisation, where murtoos are hypothesised to form (Ahokangas et al., 2021; Ojala et al., 2021). Finally, we go on to investigate differences in model outputs between observed murtoo and meltwater routes, and where no glaciofluvial landforms. We sensitivity tested the robustness of all these findings to a range of parameters"

Comment 2.17 173: suggest delete "uniform" – it seems to contradict being allowed to change diameter

Reply We have deleted uniform as suggested

Comment 2.18 221: spell out MAT (mean annual or monthly air temp?)

Reply We have corrected this (on Line 225–226) to now read:

"the same depressed monthly average temperature and precipitation record as with the ice sheet model"

Comment 2.19 242-3: "Monthly melt was kept fixed annually" – sounds a bit confusing, do you mean that it was kept fixed year on year, i.e. no inter-annual variability, each month's temp was the same each year?

Reply Thank you for identifying this potential source of confusion, and we have corrected this as suggested. Line 246–247 now reads:

"We did not prescribe any inter-annual variability in average monthly temperature."

Comment 2.20 245: again, "total monthly melt was converted to yearly melt rates" – this is also confusing, I'm not sure here what you've done or why

Reply We have deleted this turn of phrase. ISSM takes melt input in units of $m.a^{-1}$ and so total monthly melt needs to be converted to a per annum rate value. However, this was also explained on Line 252 already.

Comment 2.21 260-1: while the variables subject to sensitivity testing are clearly listed in the Appendix Table 1, and nicely explained through the following paragraphs, I would have found it helpful to have a clear summary list of those variables in the text. Consider adding a summary sentence here, or modifying line 206-1, along the lines of "Sensitivity testing was performed on: basal melt rate, moulin density and distribution, conductivity of sheet and channelised water, englacial storage, basal ice velocity, land or water-terminating ice, basal bump height, bed topography surface, and mesh geometry."

Reply As suggested, we have added the following text to Line 265–266:

"We sensitivity tested for basal melt rate; moulin density and distribution; sheet and channel conductivity terms; basal bump height; the englacial void ratio; basal ice velocity; terminus boundary conditions; bed topography; and mesh geometry. "

Comment 2.22 306: delete "between"

Reply We have deleted this as suggested

Comment 2.23 324-5: "and each peaks... and remains..." Here, I also wonder why the peak in sheet discharge and water velocity occurs adjacent to a channel?

Reply We have changed peaks to "are highest" on Line 330. These values peak adjacent to channels because water is routed towards channels along hydropotential gradients and because channels are represented along element edges, while water velocity and sheet discharge are represented on element faces. We have not made text changes to clarify this because we feel it is tangential to our work.

Comment 2.24 325-326: typo – V_W not W_V (three instances)

Reply Thank you for identifying these typos, which are corrected throughout.

Comment 2.25 326: delete one "to"

Reply We have deleted the errant to

Comment 2.26 327: you do have constraints (= landforms) on your model output, so I suggest amending to "Without independent constraint against which to compare our model output"

Reply We have adopted this suggestion on Line 333.

Comment 2.27 330: for more emphasis, I would suggest deleting "and", and breaking the sentence. Catchment hydrology "remains consistent across most of the sensitivity tests. Furthermore, sensitivity test results remain consistent with predictions for murtoo genesis."

Reply We have adopted this suggestion on Line 335–336.

Comment 2.28 331-3: I would delete the additional descriptors for each parameter (e.g. variable, modified, differences in...). It's redundant in the context of sensitivity testing, and risks confusing between an experiment in which a parameter varies spatially/temporally, vs one that has a uniform distribution of the parameter but it differs from the baseline experiment.

Reply As suggested we have removed the additional descriptors.

Comment 2.29 334: I got a little confused here and the next paragraph, since you've just stated that almost all parameters showed relative insensitivity but then go on to discuss how several parameters affect channel location. It would help to start a new paragraph on line 334, and open with a clear statement differentiating catchment-scale results with channel-scale results. E.g. "While catchment- scale trends are robust, the exact location of channels, and their length and local $overburden_{\%}$, vary between sensitivity tests."

Reply We have adopted this suggestion, which can now be found on Lines 340–341.

Comment 2.30 336: "differences in channel location"

Reply We have adopted this suggestion on Line 343

Comment 2.31 341: again to help the narrative, suggest open with "Besides channel location, channel length and $overburden_{\%}$ vary considerably". And I would also be specific here about which sensitivity tests, not just "six of". E.g. "...vary considerably in our testing of sensitivity to conductivity".

Reply In the spirit of this suggestion, the opening of this paragraph (Line 349–350) now reads:

"Although consistent across the majority of tests, channel length and $overburden_{\%}$ does vary considerably at the tested limits of k_s and k_c parameters, describing the sheet and channel conductivity respectively"

Comment 2.32 347: again to help the narrative, suggest open with "For the other end member"

Reply We have not adopted this suggestion, as we feel end member is unnecessarily vague when here we are discussing both a minimum tested parameter (sheet conductivity) and a maximum tested parameter (channel conductivity).

Comment 2.33 347: does the "and" in this line mean that both conditions are met (minimum sheet k at the same time as maximum channel k), or that this result arises when either one of those conditions is met? I'm guessing the latter since each has a separate figure reference. I wonder if there's a way to clarify this. (Same for line 351.)

Reply It is the latter, as we only tested one individual parameters at a time. We have changed Line 355 to now read:

"For *both* the minimum sheet conductivity...and the maximum channel conductivity"

We have also made the same change above, on Line 351.

.

Comment 2.34 357-60: I think a clearer way to argue this is that excessively long or short channels are considered 'invalid', on the basis of modern Greenland observations, and that an anomalous overburdendistribution is considered invalid on the basis of the conceptual model for murtoo distribution, and therefore the baseline terms are considered most plausible.

Reply We have adopted the suggested wording, and Line 365–369 now reads:

"Excessively long (>50 km) or short (<10 km) channels are considered to be invalid on the basis of modern Greenland observations (e.g., Chandler et al., 2013; Dow et al., 2015) and an anomalous $overburden_{\%}$ is considered invalid on the basis of the conceptual model for murtoo distribution and genesis (e.g., Ahokangas et al., 2021; Hovikoski et al., 2023). Therefore, our baseline conductivity terms are the considered the most plausible parameters."

Comment 2.35 366: "the pressure conditions... are notably different"

Reply We have corrected this, now found on Line 375.

Comment 2.36 367: suggest breaking the sentence to make it clear which channels (modelled or observed) you refer to in the final clause. "In Greenland, channels exist at lower pressure..., and the resultant hydraulic potential gradient..."

Reply We have adopted the suggested wording, now found on Line 376–379

Comment 2.37 376: typo – FLDIL

Reply We have corrected this, now found on Line 385.

Comment 2.38 Section 5.2: From your description of sites D& E, I struggle to see the difference in landform/hydrological context. You start with E, and describe it as being representative of channel onset nearby, and conclude that the model drainage behaviour is consistent with murtoo development phases (although murtoos themselves aren't evident here). Then you describe D as being located at the head of two channels, and near murtoos, and interpret that 1 of the 2 modes of drainage here is consistent with murtoo formation. I see that the drainage behaviour differs at sites D& E, but both can allow for murtoo formation and both have some relationship with channel onset, so I think this part of section 5.2 (e.g. lines ∼389-406) would be better packaged if you instead: introduce D& E as both being in the zone hypothesised to favour murtoo formation, and that both display drainage behaviour that could accommodate murtoo formation, but they show different interannual behaviour; and note also that one node is close to an observed murtoo while the other is not. I think this would better frame the later discussion of absence of murtoos where they're predicted, and the discussion of the biannual behaviour and its relationship to landform distribution. Here and below: a figure, perhaps plotted in map view, of nodes that display biannual behaviour vs annual behaviour would really help this discussion and that in section 5.3.

Reply Thank you for this paragraph suggestion, which we have adopted. Lines 399–409 now read:

"Figure 3D & E demonstrates the seasonal evolution of two nodes between 40–60 km from the ice margin, each nearby to channel systems. Both nodes fall within the hypothesised zone of murtoo formation and both nodes display drainage behaviour which could accommodate murtoo formation, with a seasonal increase in $overburden_{\%}$ up to a maximum of approximately 120 % and a more gradual decrease thereafter. However, the two nodes show different interannual behaviour, and only one is located close to a murtoo field. At node 3,842, ∼54 km from the ice margin and chosen to be representative of surrounding nodes at the onset of a channel (Figure 3E), the pattern of drainage repeats annually—every year the increase and decrease in $overburden_{\%}$ is accompanied by peaks in q_s, Q_c, and V_W and the nearby development of channels throughout

the meltwater season. At the onset of channelisation the maximum Q_c approaches but never exceeds 1 $1m^3\,s^{-1}$. However, although this evolution through time does appear consistent with each of the murtoo developmental phases (Table 1), node 3,842 is not located near to a murtoo field .

At node 16,402, located..."

However, we have not created an additional figure as suggested, given the practical difficulties associated with identifying which of several thousand nodes do or do not display biannual behaviour.

Comment 2.39 392: wording is a bit awkward – I suggest "representative of nodes surrounding the onset of a channel" ?

Reply This text was removed as part of the suggested rewrite above.

Comment 2.40 402-5: there's a few awkward phrases here. Consider: "before quickly dropping to an $overburden_{\%}$ that is elevated relative to the previous winter." "Years with an elevated $overburden_{\%}$ are associated with lower Qc...". Put commas round "approaching 1 m3 s-1". Replace odd-numbered years with "We consider that the latter case is more consistent with..."

Reply We have adopted the suggested wording, now found throughout the paragraph starting on Line 409.

Comment 2.41 410: double "maximum"

Reply We have corrected this.

Comment 2.42 411: "the channel remains active over winter".

Reply We have amended this text to now end in "over winter". New text now found on Line 423.

Comment 2.43 418-9: suggest you add to the end of the sentence "that have been invoked to explain murtoo sedimentology".

Reply We have added the suggested text on Line 431.

Comment 2.44 422: since GLaDS is pervasively connected and therefore can't produce the rapid changes in flow that you describe, is this limitation more widely a problem for other modelling questions? Of course the model is a necessary compromise on certain aspects in order to be manageable, but does your work flag that this is actually important to implement more fully?

Reply This is a limitation shared by many recent subglacial drainage models (see Rada Giacaman and Schoof, 2023) and we now state this on Line 434, which reads:

"However, as with other subglacial hydrology models, GlaDS is a model in which the subglacial system is assumed to be pervasively hydraulically connected (see Rada Giacaman and Schoof, 2023)"

Further, we do agree that in order to more faithfully reproduce murtoo formation it is likely that hydraulic isolation is something that is important to implement in future studies. We have included a sentence to this effect on Lines 439–441 which reads:

"Including spatially variable system conductivity is likely to be important in future work which seeks to evaluate the ability of process-based subglacial hydrology models to represent landform formation. "

Reference: Rada Giacaman, C. A. and Schoof, C.: Channelized, distributed, and disconnected: spatial structure and temporal evolution of the subglacial drainage under a valley glacier in the Yukon, The Cryosphere, 17, 761–787, https://doi.org/10.5194/tc-17-761-2023, 2023.

Comment 2.45 Section 5.3 Is the heading appropriate? I think something like "Comparing murtoo and meltwater route hydrology" is a closer description of what this Section considers. Paragraph 1 is overcomplicated, and I think it is mis-framed as an evaluation of GLaDS ability to represent meltwater pathways – you do something more specific than that and it raises an interesting discussion about different types of behaviour. It could be much more succinct and clear if you frame the opening to this Section as: You further explore drainage behaviour in the zone hypothesised to be relevant for murtoo formation. You group all the nodes in this zone according to whether they are located among murtoos, meltwater routes, or neither. Murtoos and meltwater route distribution are based on Ahokangas et al, and you include eskers with meltwater routes, though you acknowledge there is no age control to say if they all form simultaneously (and in the relevant time-slice).

Reply We have adopted the suggested subsection heading and have changed the framing sentence so that Line 450–451 now reads:

"We explore drainage behaviour in the area of anticipated murtoo formation by isolating and taking a spatial median of nodes in the baseline model 40–60 km from the ice margin"

Comment 2.46 Line 448: "winter minima"

Reply Amended on Line 468.

Comment 2.47 449: "do not intersect mapped glaciofluvial geomorphology"

Reply Amended on Line 472.

Comment 2.48 450: "...are lower..."

Reply Amended on Line 470.

Comment 2.49 P3 [in section 5.3] could be more impactful, and it's missing a clear discussion of all elements in Fig 6 – in the second half of the paragraph, you overlook Fig 6 and only talk about what's in the Appendix. I suggest, for all parameters, first comment on what is shown in Fig 6, then comment on what differences are apparent during specific months.

I also think there's some interesting behaviour in Fig 6 that you haven't commented on (but here would be the place for it), and I wonder if it would contribute to your discussion of biannual behaviour and/or differences in drainage mode within the same 'murtoo-favouring zone'. For all parameters, the murtoo data has a bimodal distribution, while the channels either have a single peak or are also bimodal – is this something you can comment on?

Reply As suggested, we have commented more on the PDF, and the next text (Line 474–480) now reads:

"The probability density functions of murtoo routes and meltwater routes is also clearly distinct from terrain without glaciofluvial landforms (Figure 6). However, the probability density functions of murtoo routes and meltwater routes are also different from one another, particularly at the lower tail of the distributions (Figure 6). Murtoo routes have a $overburden_{\%}$ distribution with a more tightly constrained lower tail than meltwater routes, with fewer nodes dropping below $overburden_{\%} = 80\%$. There is a bimodal distribution of both q_s and V_W within murtoo routes that is not evident in meltwater routes at the lower tail of the distribution.

Both meltwater routes and murtoo routes have a bimodal Q_c distribution, but the lower murtoo route peak is offset towards higher channel discharges"

Comment 2.50 P4-P6 [in section 5.3] – I still find it hard to work through the arguments relating to the biannual drainage signal. It's an interesting behaviour, but I'm still not really sure what you are saying about it in relation to either space or landforms (or whether it's a model artefact that wouldn't be present in reality). I think there's a missing link in your arguments that relates to the statements on lines 472-3 and 481-2: that there is a spatial component to the biannual signal in murtoo route outputs, and a significant biannual difference between murtoo routes and meltwater routes.

I don't believe you've demonstrated a significant biannual difference between murtoo routes and meltwater routes. Section 5.2/Fig 3 show that two nodes in the hypothesised murtoo forming zone have different signals – one annual, and one biannual. You've concluded that both behaviours are suited to murtoo formation (albeit only one of the two modes in the biannual case). Section 5.3/Fig 6 show that murtoo and melt routes differ in behaviour within a season/year, but you don't demonstrate here that one or the other is characterised by a biannual signal. Therefore, your statements on lines 472-3 and 481-2 combine these two results, without demonstrating that the biannual signal is landform-specific.

I think paragraphs 4-6 (i.e. remainder of Section 5.3, from line 467), ought to be revised to sharpen the discussion about the biannual signal, its spatial distribution, and how it might relate to a specific landform type. As above, I think that a figure that demonstrates the "spatial component to the biannual signal" would really help – whether or not this spatial biannual pattern also has a connection to a specific landform type. This could be a map – nodes with an annual/biannual signal. And/or an equivalent to Fig 6, but plotted for the two lateral or central parts of the lobe instead of by landform type?

Reply In seeking to address this comment, and several of those below, we have restructured the end of Section 5.3 from Line 491 onwards. We have trimmed and simplified much of the text with a view to highlighting:

- That there is a statistically significant difference between meltwater routes and murtoo routes (Lines 475–491).
- That this difference is subtle, and both murtoo routes and meltwater routes are consistent with the murtoo formation phases (Lines 492–493).
- We conclude that this statistical difference has a strong spatial component linked to the repetitive presence of overwinter channels, GlaDS is not necessarily resolving differences between the landforms themselves (Lines 493–503).
- The existence of overwinter channels is not necessarily surprising, but their repetitive signal is likely driven by our fixed model forcing. Their spatial expression meanwhile may be linked to the diverging lobe and ice flow vectors (Lines 504–509).
- Winter channels, are certainly less likely to be repetitive in a more realistic model setup, and may well disappear altogether (Lines 509–512).
- Murtoos distribution appears to be a compelx mix of factors, and in reproducing murtoo forming conditions in the centre of the ice lobe (where murtoos are absent 40–60 km from the ice margin) our model is clearly missing key subglacial process (e.g., ice coupling, sediment dynamics) and future work is needed (Lines 512-519).

Comment 2.51 In P4 (in section 5.3) , there are far too many ideas all stuffed into one paragraph: I would move the opening to conclude P2 and open P3: murtoo & meltwater routes are both very different to where there are no landforms, and in this way GLaDS faithfully represents FLDIL drainage patterns. This is further evident when plotted as a PDF. Here, however, you also see subtle differences between murtoos and meltwater routes. - Start P4 by posing the problem that murtoos are absent where the model (both

conceptual and physical) suggests they ought to form. Offer your geological/data reasons for this. Then... - Alternatively, murtoo distribution could be related to biannual channel discharge behaviour (reported in section 5.2, Fig 3). Biannual signal is interpreted as due to channels persisting through winter; they likely influence the nearby system the following summer, when the initial melt input would be discharged by an already established efficient pathway. There is also a spatial component to the biannual signal in our model output. When channels in the central third of the FLDIL persist over winter, those in the outer two thirds do not – and vice versa. Murtoo distribution – absent in the central third – could be a reflection of this spatial control on winter channel operation.

This sets up the whole of the final part of your discussion in the context of murtoo formation (presence/absence), and there is a clear purpose to the biannual discussion, rather than discussing a quirk that seems to have emerged from your modelling whose connection to your research question isn't really apparent.

Reply As suggested, we have moved the opening of P4 to conclude P2 and state that GlaDS is faithfully representing FLDIL drainage patterns. However, because we cannot rule out that overwinter channels (which drive the statistically significant difference) are not in fact a model artefact, we have stopped short of linking murtoo formation to winter channels. Instead, in rewriting the end of section 5.3, we have tried to make it more relevant to the paper's aims (how well does a process-based model resolve murtoo formation). To do so, we have reframed this section as one in which we state that GlaDS is clearly distinguishing between meltwater/murtoo routes and the wider terrain, but is failing to resolve differences between murtoo and meltwater routes, and is therefore not able to resolve specific murtoo locations. We go on to suggest future work which may further improve GlaDS ability to represent murtoo formation.

Comment 2.52 P5 – I also think this is unnecessarily wordy and buries the point of the argument. Consider re- ordering (and trimming) the ideas: The appearance of winter channels isn't surprising: they are evident on Greenland. Yours operate at very low discharges (below an arbitrary threshold for classifying a "channel") but nonetheless exhibit this behaviour. However, the spatial pattern of winter persistence is unexpected. There is spatial variability in your climate forcing, which translates into spatial variability in meltwater input, though your input has no interannual variability that would explain a biannual signal in channel Q and overburden.

This leads directly into the next paragraph, in which you offer a solution.

Reply We have adopted these ideas in our rewrite of Section 5.3, and have used this structure in our new writing (Lines 504–509)

Comment 2.53 Line 503 – if this difference between murtoo and meltwater routes that you refer to here is the presence of a biannual signal, then as above, I don't think you have demonstrated this. (If you only mean that they differ, as in Fig 6, then this is ok.)

Reply As part of our rewrite in this section to more clearly communicate the significant differences reported on, we have removed this particular text.

Comment 2.54 504: "divergence... appears to..."

Reply We have removed this particular text

Comment 2.55 506: be specific – which portions of the model have channels that resist closure during winter? A figure would really help this spatial discussion. Where/how do these portions relate to flow divergence?

Reply To address this, we include a new supplementary figure (Figure A2) which illustrates where winter channels form each year and how this relates to murtoo distribution

---

Comment 2.56 508-9: but even if the very regular biannual signal isn't evident any more, do certain sectors have more of a tendency for over-winter channel persistence than others? I'm not sure you intended to conclude this discussion with 'it's a model artefact', but that's how it reads.

Reply There is no clear difference between sectors. In our rewrite, we have elected to be more cautious because we ultimately cannot rule out that it is in fact a model artefact, we have removed this specific line as per the above comments.

Comment 2.57 509-514: this is a really interesting idea, that the sub-lobes approximately match the pattern of winter channel persistence. Again, could a figure that illustrates this be combined with one as suggested above, or would the same figure serve both aspects of this discussion? And can you offer a concluding sentence that might explain why large eskers bound zones with different winter persistence of channels? In terms of the writing structure, I would break these lines off into a new, final paragraph.

Reply The original text was intended to highlight the close overlap between overwinter channels, murtoo routes, and sub-lobes, without being overly speculative. However, because we cannot rule out that the overwinter channels are in fact a model artefact, we have removed this particular section of text.

Comment 2.58 517: I think you've actually explored drainage in relation to more than one glaciofluvial landform – suggest pluralise

Reply We have pluralised as suggested (Now on Line 522).

Comment 2.59 520: the absence of topography is an awkward concept. Suggest amend to "including the absence of any relief"

' **Reply** as suggested, we have amended this to "a total absence of relief" (Line 526).

Comment 2.60 523: the wording here is a bit confusing about the direction/trend of change (up/down or forward/backward in time) – suggest something like "Assuming this area has been uplifted by a maximum of ∼100m, the volume of melt... would have been higher during the YD due to higher temperatures at lower altitude."

Reply We have adopted the suggested wording, which can now be found on Line 528–531.

Comment 2.61 537-539: this seems very specific and a poor final sentence – is it necessary or can you move it earlier in the paragraph? It seems more natural to end with the future work sentence.

Reply As suggested, we have moved the sentence. It can now be found on Line 534–536.

Comment 2.62 In this section you focus on all the things that you haven't included. But you have done some fairly extensive sensitivity testing. I think it would be entirely appropriate to note that you haven't included all these factors and this introduces some uncertainty, but that you have tested many parameters and your findings are largely robust.

Reply We have not made any change in response to this, as we do go on to state this in the summary and conclusion section, where we feel it is most appropriate.

Comment 2.63 544: "The alternating sedimentological sequence"

Reply We have adopted the suggested wording, which can now be found on Line 549.

Comment 2.64 557: delete "extending", you've already said the channels extend

Reply We have removed this "extending".

Comment 2.65 Make sure the title of the article matches the final title.

Reply Thank you for noticing this, which we have now fixed.

Comment 2.66 Fig A15. I think the caption ought to read 'comparison of basal melt 7×10^{-3}... against baseline 1×10^{-3} ?

Reply Thank you for noticing this in what is now Fig A16, it actually should read 5×10^{-3} and we have amended both this and Fig A15.