## Response to reviewers

We are grateful for receiving three detailed and constructive comments, including two reviews (Marion McKenzie and an anonymous reviewer) and one open comment by a group of colleagues (Isabelle McMartin et al.). The main issue raised has been an inclusion and discussion of chronological constraints on our reconstruction. To address this, we have decided to produce an A4 version of each panel of Figure 6 and add the available chronological constraints to these detailed tiles in a new supplementary document. We provide further discussion of this issue and other comments in the detailed response below.

**Please note that the reviewer comments are posted in black and our responses are posted in blue**

## Response to Marion McKenzie

In this manuscript, B. Stoker and others present a flowset model and cross-comparison with geomorphic data across the deglaciated northernwestern LIS to provide a vast spatial and temporal analysis of ice streaming and retreat over the Bølling–Allerød and Younger Dryas periods. This work presents compelling evidence supporting collapse of the CIS-LIS ice saddle contributing to increased ice streaming and ice output reorganization during the Bølling–Allerød and varied styles of ice retreat following this collapse. I believe this work fills a knowledge gap in incorporating glacial geomorphic data with geochronology and ice modeling outputs for this region. However, I do think this work could be improved through further development of the interpretation and discussion sections with added supplemental materials to maintain focus in the final sections of the paper. There are some pieces in this paper that I think have merit but may not need to be included in this work specifically. Please see my structural and figure notes and thorough line comments to address areas in which this manuscript could be improved.

Thank you for your overall positive review of our manuscript and for the helpful comments you have provided. Below we provide detailed responses to the comments and describe the changes we intend to make to the manuscript.

**Specific structural comments:**

Please provide more information about the ages used to constrain the flowsets and associated glacial features to specific timeframes. Including a table with this information or providing a section in the introduction introducing dates and context for geochronological situated retreat (Stoker et al., 2022) would greatly clarify a lot of questions I had about how some of your interpretations were tied to temporal constraints.

We will create a supplementary figure which will include an A4 version of each time slice panel of Figure 6 with the relevant age constraints for the depicted ice margin location and the type and age of these constraints. This will provide the reader with the necessary information without adding too much extra text to the manuscript that would distract from our main message. We are reluctant to add much more detail to the main text of the manuscript as recent publications have discussed the regional chronology in detail and the principal focus of this manuscript is the geomorphology. However, we will add a further, brief section of text clarifying how our interpretations are tied to certain chronological constraints and the potential implications of any future changes in the chronological framework.

In general, watch the use of ambiguous identifiers. Often the use of "that" "this" and "they" can get lost in a line of logic, so make sure you're being very explicit when making statements.

Thank you. This is an important point that reviewer 2 also raised and we will double-check the text and clarify these statements.

Section 3.2 should be split into two separate sections. One that describes ice streaming and the inferences made to topography and expand on the deglacial dynamics section.

The title for section 3.2 is somewhat confusing, we have renamed this to 'Deglacial ice margin retreat pattern' to better describe that we are simply reconstructing ice margin retreat patterns and not dynamics.

Section 3.3 could be renamed to "Ice margin during retreat" to clarify that you are describing the ice margin boundary you developed and then the one you used for temporal constraints.

We will amend this section to avoid any confusion. The intention of section 3.3 is purely to provide a brief overview of the NADI-1 ice margin chronology and highlight why we opt to use it as the framework for our ice flow reconstruction. While section 3.2 was purely to describe our process of identifying the former ice margin retreat pattern as depicted in Figure 3 and 5.

Section 5.1: for this first section to be an overview of ice flow interpretation, I don't think the evidence from your results is connected enough. You make statements about ice behavior referencing your figures without tying in the significance of cut-through dynamics or the different types of flow sets you used to make these inferences. I see you do this throughout section 5.2 and am not completely convinced the overview in 5.1 adds to the readability and competes with the merit showcased in 5.2. I would suggest either switching the order of sections 5.1 and 5.2 or simplifying/removing 5.1 and combining it with section 6, which essentially restates a lot of the overview but with better supported evidence.

Thank you for the suggestion. We agree that it is probably more appropriate to move this to the end of section 5 and simplifying it to provide a summary of the ice flow interpretation.

I would consider renaming your section 5.2 headers. I understand the interest in continuity across sections, but I think readability and interest may increase if you give headings related to why the time slices are split the way they are. For example: "5.2.2 Local LGM (17.5 to 17.0 ka)" or "5.2.3 Ice stream network reorganization (16.5 to 16.0 ka)".

We will adopt the suggested subheading style.

Section 6.4: I feel like this section does not incorporate many novel results that you have not already discussed in this paper. I am fine with the 6.3 sections on ice retreat variation but feel like a one to two paragraph summary of this work could be incorporate prior to section 6.5 Additionally, in the 6.4.2 section on the Canadian Shield, this ends up being a discussion of local responses and climate responses to the Younger Dryas and no evidence is presented from the Canadian Shield. If you keep this section, I encourage including information about basal conditions on hard bed surfaces that would impact a slow in ice movement. You also state ice stream slowing is not a result of geology changes, but that directly contradicts what you say in the introduction to section 6.4 and is counterintuitive to the 6.4.2 section header. You address this well in the first paragraph of 6.4.3 so I suggest you remove 6.4.1 and 6.4.2 and focus on results from 6.4 intro and 6.4.3.

Thank you for the comment, it is clear that the key message of 6.4 is not as clear as we had hoped. The key aim of section 6.4 is to guide the reader through Figure 11, describing the changes in ice streaming, explaining the hypothesised mechanisms for these changes and speculating on the possible implications for ice sheet mass balance and ice margin retreat rates during these variations in ice stream activity. This section will be rewritten to better fulfill this aim, following your suggestions.

The conclusion could be much more concise—highlight the main findings without naming the specific evidence.

We will amend the conclusion as suggested.

**Figure and table comments:**

Figure 1E: If you're going to create a hypothetical flowline model with example flowsets, I would either make a schematic of factors used to determine flowset styles in parts A-D or develop a hypothetical model that includes the same description for all flowset types (maybe with some extra text or drawn geomorphic features on the figure to explain differences). This may also help clarify the differences between event and ice stream flow sets which I believe needs a bit more explanation.

We will add further annotations to Figure 1E to help define event and ice stream flow sets and further clarifying edits will be made to the main text to highlight the differences between these flowsets.

Table 2: “Ice marginal position” should be renamed to “Deposition process” as you are describing how these features are developed.

Done.

In the terminal moraine section of “Ice marginal position” I recommend you refine the statement “deposition” to “deposition by bulldozing or transit to the margin” because the process of “deposition” is very broad.

Done.

Can you give some examples of what you mean by “a combination of processes” even if it’s just to mention plucking, meltwater redistribution, etc.

Hummocky terrain is a catch-all term that covers a landform composed of multiple distinct features and forming from a range of processes. The description of hummocky terrain will be significantly expanded to highlight the range of features it includes and the associated processes better. For example, raised, flat surfaces interpreted as ice-walled lake plains indicating localised ice stagnation processes, sharp-crested ridges within the controlled moraine which represent readvances of the ice margin.

Figure 3. Can you increase the contrast of the DEM? It is hard to identify the individual features you have mapped.

The visibility of some of the features is limited due to their small size and the necessity of having a large area covered by this figure panel to highlight our approach of defining ice margins. Unfortunately changing the contrast of the DEM does not help in showing any further detail.

Please also clarify the definitions between esker complex and ridges (i.e., individual eskers could not be identified and rather there is a complex network of meltwater features) and your identification system for major vs. minor moraine crests.

We follow definitions outlined in Dulfer et al. (2022) and we will describe these in the text. Esker ridges are mapped where there is a clearly defined single esker ridge feature that can be depicted by a polyline, while esker complex is used to describe networks of anastomosing eskers or single esker ridges with a complex morphology that are better depicted with a polygon. Major moraines are polygons features mapped when the moraine is >200m wide and minor moraines are polyline features mapped when the moraine is <200m wide.

Perhaps you could zoom in on some of these features or provide specific DEM examples in Table 2.

We will provide a further column in Table 2 with DEM examples of these features.

Figure 4: it is difficult to see the difference between colors of deglacial and inferred deglacial flowsets. I understand the draw of using two near-similar colors because of the similarity in flowset formation, but especially for colorblind readers, I can imagine this difference would be too minuscule to be able to visualize.

We will continue using blue to depict the deglacial flowsets but will use a greater contrast between the blue colour used for these flowsets.

Figure 6: I appreciate what I'm sure was a considerable amount of time and effort in developing Figure 6 – that is an incredible amount of data to visually represent across such a vast spatial area. My only suggestion would be to possibly increase contrast between flowset and ice extent colors – sometimes the flowsets are difficult to see over the intense blue of the ice and the dark DEM underneath. I also have trouble reading the flowset numbers at times, I would consider creating a close-up map of some highly congested areas (section I) in a supplemental figure.

As previously mentioned, we will create a supplemental version of each panel presented in Figure 6. Each supplemental figure will be created at A4 size to increase the readability and will include the age constraints which relate to the specific timestep. We will also increase the transparency of the basemap and ice to increase readability.

Figure 7: This is great – I appreciate the visual representation here. I would consider making this figure supplemental, though. It is not entirely central to the argument you make in the section it is discussed.

Agreed. We will move this figure to the supplement.

Figure 8 seems to be non-essential to the arguments made in this work. I would recommend combining Figure 8 and Figure 5 to relate concepts of ice-margin interactions with glacial lakes or move this figure to a supplemental file.

Figure 8 is important in providing the reader with context on the location and timing of glacial lakes across the study region and aids in answering one of our key questions (what were the controls on ice streaming?). Without this figure, our claims relating glacial lake presence/absence to changes in ice streaming is less supported. We attempted to combine the glacial lake distribution with Figure 5 but unfortunately the figure becomes too cluttered with these sets of information combined. Additionally, the disconnect between the newly created ice margins and the glacial lake reconstruction (adopted from Dyke et al. 2003) was confusing. So we will retain Figure 8 as it is.

Figure 9 caption: Mackenzie is spelled incorrectly in (B). Clarify what you mean by "topography".

Thank you, we have corrected the spelling. By topography we simply mean that ice flow follows the orientation of the valley and ice flow indicators are located around a topographic obstacle. We will include this in the text.

Figure 10D: what is a "zig-zag esker"?

'Zig-zag eskers' is used to describe eskers with a zig-zag shaped planform and that may also be referred to as concertina eskers (e.g. Storrar et al., 2015).

**Line comments:**

Line 14: Suggest change from "it" to "this retreat" or something similar.

This will be changed to 'This ice sheet sector…'

Line 32: clarify calving for marine-terminating ice systems, the first part of the statement could refer to terrestrial and marine ice streaming.

This will be changed.

Line 41: suggest change from "it" to "Laurentide Ice Sheet" could read as ambiguously referring to the North American Ice Sheet complex.

This will be changed.

Line 48: clarify the ablation area of the northwestern sector.

This will be clarified to 'ablation area in the region of the Cordilleran-Laurentide ice saddle'.

Line 49: Direct evidence for the statement in the first half of this sentence?

The evidence for this statement is based solely on the numerical modelling simulations referenced at the end of this sentence.

Line 57: Suggest add "geomorphic-based evidence of ice stream activity"

This will be changed.

Line 83: Clarify "the LIS and CIS ice sheets through the saddle"

This will be changed.

Line 115, remove commas around undated

This will be changed.

Line 126: "Range and were dammed […]"

This will be changed.

Line 132: Suggest "has led […]"

This will be changed.

Line 134: add parentheses timing of Younger Dryas Stade for context

This will be changed.

Line 136: here is this argument still incorporating an early 30ka maximum, or are they arguing a total maximum later, at 20ka? Please clarify this point as these two comparative sentences are not exactly congruent.

The more recent reconstructions do not include a maximum extent at 30ka. We will clarify this in the text.

Lines 150 and 152: The ice sheet wide and across the entire ice sheet in the same sentence is redundant.

This will be changed.

Line 155: Reference figure 1 after "Smoking Hills-Horton River area"

We will add this reference.

Line 160: Cordilleran to CIS

Done.

Line 169: Clarify whether the "uniform mapping approach" was a manually conducted mapping effort or if there were automated tools involved or machine learning approaches to landscape analysis.

The mapping approach was done entirely manually by two separate mappers. We used the term 'uniform mapping approach' to highlight that we mapped the same landforms, scale, and using the same datasets across the entire region, meaning there are no spatial biases in our data. We will clarify this in the text.

Line 184: clarify what you mean by "morphology of the flowset". Do you mean here you are identifying the types of streamlined bedforms? Elongation ratios? Orientation and parallel conformity?

We will clarify this in the text. The morphology of the flowset refers to the overall shape of the flowset of grouped lineations, for example, whether they display any diverging or converging patterns, an hourglass shape, etc. We also describe the morphology of and classify the lineations within the flowsets, but do not define elongation ratios specifically.

Lines 194-197: The comparison between these three sentences is a little difficult to follow. I would add a contrasting argument before introducing the inferred deglacial flowset (i.e., "Conversely, *inferred* deglacial flowsets […]" and in the final sentence

describing the fan-shaped lineations, I would suggest saying “The proposed ice-marginal formation of inferred deglacial flowsets is based on [...]”.

We will adopt these suggested changes.

Line 198: There needs to be more clarification on the difference between ice stream flowsets and event flowsets and possible overlap between the two. Both have abrupt lateral margins, both could occur on the interior, both may be overprinted, and the elongated nature of the event landforms is unclear. I would choose several classifying characteristics for each of the flowset types and make sure you identify the characteristics for all the flowsets so that they may all be directly contrasted and compared.

You raise an important point that we will clarify in the text. Our flowset classification follows that of Kleman et al. (2006) whereby flowsets are first divided based on whether the ice flow event that formed them was fast (ice stream flowset) or slow (event and deglacial flowsets). Following this, there was a secondary division of slow flow regime flowsets depending on whether they formed near to the ice sheet margin (deglacial) or towards the interior of the ice sheet (event). As such, an ice stream flowset can form in both an ‘event’ or ‘deglacial’ position. While the term ‘event’ can be somewhat confusing we do not want to redefine terminology where we can avoid it and so we follow the naming conventions of Kleman et al. (2006).

Line 198: I would also like more clarification on the name “event” for these flowsets. Is this suggesting that these flowsets were developed very quickly in a singular streaming event and were then discontinued? This should be clarified in the definitions.

Thank you for the comment. I hope this has been clarified in the response to the above point and we will explain this in the text.

Line 277: Consider looking at ICE-D and AskICE-D for standardized and recalculated cosmogenic nuclide exposure dates – this is a global dataset that has many CIS and LIS ages from published work that can be compared.

Thank you for the suggestion. The ICE-D database highlights some of the issues relating to changing exposure age calculation methods, including how different production rate, scaling methods, etc might impact the calculated age.

Line 285: I commend the authors on making all shapefiles available from the paper.

Thank you.

Line 297: You name deglacial flowsets as the second-most prevalent flowset type yet have not named the first most prevalent type yet. I would make this clarification or reorder the presentation of flowsets otherwise this statement seems out of place.

We will reorder the presentation of flowsets so that 'inferred deglacial' flowsets, the most prevalent flowset type, is described first.

Lines 302-307: This interpretation of topographically influenced streaming seems like it may fit better in discussion where you will have more room to justify this argument. Based on what is currently in these sentences, I am not quite clear on how you are making this interpretation and if the bedforms in the foothills of the Mackenzie Hills are more affected by topography than those on the Northern Interior Plains.

This section is meant as a brief overview of the setting of each flowset type and how it varies across the region. The topographic influence described is purely a product of the greater topographic relief present in the foothills. We will rewrite this to better describe the observed patterns of topographic funnelling down valleys and the deflection of ice flow around bedrock obstacles and to include less interpretation.

Additionally, you distinguish between drumlins and mega-scale glacial lineations but did not explicitly state your classification guidelines in the methods. I would assume you used Clark et al's 2010 identification of a 10:1 elongation ratio difference between the two bedform types, but if that is the case, then I would mention this somewhere in the methods.

You are correct, we will include this definition in the text.

The sentence between lines 305 and 307 is difficult to follow. I would recommend splitting this up into more than one statement.

We will rephrase this to:

In the central Mackenzie Valley, north-oriented flowsets were formed when the ice surface slope was the dominant control on ice flow direction underneath a thick ice sheet during the local LGM. While the adjacent south-oriented flowsets record the increasing importance of the topographic relief to funnel ice flow up the Mackenzie Valley during deglaciation, as the ice sheet thinned.

Lines 318-319: Again, an interpretation of the influence of local topography on diverging flow patterns.

We will describe the pattern of topographic influence (funnelling of ice flow by the topography) rather than the vague interpretation we previously stated.

Line 329: You use the term "topographic influence" broadly several times in this results section without necessarily defining it or naming the different influences you identify (e.g., funneling from valleys, divergence of flow around bumps in the bed, or separation between flowsets by ridges) please provide examples and be specific when discussing your identified flowsets. Please see McKenzie et al., 2022 for resources regarding topographic and lithologic influence and streamlined subglacial bedforms and

McKenzie et al., 2023 for evidence of subglacial bump influence on streamlined subglacial bedform morphologies.

Thank you for the suggestion. We will revisit this section and where we use vague interpretations we will clarify the individual influences we identify based on the suggested references.

Line 340: This relates back to another comment about event flowsets, but what do you mean by "different events" in this context. Please provide examples (i.e. surging events, subglacial lake outbursts providing lubrication to the bed, etc.).

We hope this was clarified in the previous comments.

Line 341: please provide the "n=" value for all datasets, not just the unclassified flowsets.

This will be included.

Line 370: cite the "previous studies" you are referring in this line.

This will be done.

Line 375: I would clarify "Terrestrial ice-contact landforms" here. There are areas where marine-terminating ice contact landforms are visible at the surface from landscape evolution, so I would just be abundantly clear.

This will be done.

Lines 406-407: Claiming that the geomorphological evidence does not support ice stream activity during the same time as the Amundsen Ice Stream feels rather unsupported. This would take geochronological data to support the timing statement. Your argument of deglacial facies overprinting ice streaming is fine, but here is a good opportunity to pull in your findings of the most common flowset being the deglacial flowsets.

We do not intend to suggest that there was no other ice stream activity during the advance phase and we will clarify this. Instead, we wanted to highlight how any advance phase ice streams have been overprinted by the deglacial record. The presence of the Cameron Hills fragment supports the presence of other advance ice streams and it's fragmented nature, with the only remaining evidence being preserved at high elevation, supports that the hypothesis that deglacial flow has removed the majority of evidence for advance phase ice streams. We have included the Cameron Hills fragment description to justify this claim and will better explain this in the text.

Line 414: I think further supporting this sentence would make it stronger (i.e., "[…] with periods of slow retreat as seen from the presence of periodic recessional moraines and some larger moraine crests […]").

This will be changed as suggested.

Line 414: Do you mean meltwater drainage by ice drainage, or do you mean ice drainage through ice stream networks? Make this clearer.

Ice drainage through the ice flow network, we will clarify this.

Line 415: “these changes” be clearer, are you talking about the periods of rapid ice loss or long-term stabilization that could have caused further erosion/transport to the margin?

‘These changes’ is in reference to both the periods of rapid retreat and of stabilisation. We will amend this to clarify and state ‘to the variations in the rate of deglaciation’.

Line 420: Were likely not active at the same time as each other or at the same time as the Amundsen Gulf Stream during early deglaciation? Make the connection between this sentence and the prior more explicit.

Likely not active at the same time as each other. This sentence is distinct from the previous sentence.

Line 425: I understand the connections here, but I think you need to make them a bit more explicit to readers. I understand how you determine which flow came first through crosscutting relationships, but to make it clear this occurred as a result of the Bølling–Allerød, I suggest framing the argument like “Without the source from the ice saddle, the ice streams of this region began to primarily receive input from the Keewatin Ice Dome to the west as seen in the northerly flowsets transitioning to northwesterly and eventually westerly-facing signatures of ice flow.” You do this in later lines but I would move this up to the start of this argument.

We will amend this as suggested.

Line 444: Connect to the flowset data – potential to include “which is seen to be associated with more truncated streamlined subglacial bedforms and inferred decrease in ice flow speed and subglacial sedimentation organization (McKenzie et al., 2022).”

We will amend this as suggested.

Line 445: the last paragraph in this section could be moved to an introduction between header 5.2 and 5.2.1 to make it more fluid for readers.

We will move this section as suggested.

Line 470: multiple younger flowsets?

We will amend this as suggested.

Line 509: I would break the sentence after “deglaciation” – it took me a few times of reading this to make sense of the additions to the first statement.

This will be amended as suggested.

Line 513: Add “between 16.5 to 16.0 ka” to the end of this sentence for clarity of that is the time slice in which you’re referring.

This will be included.

Line 521: Change “this” to “the later flowset”

We will change this as suggested.

Line 523: Change “collapse of the ice saddle during the Bølling–Allerød” to “collapse of the ice saddle, which occurred during the Bølling–Allerød” for clarity.

We will make the suggested change.

Line 529-532: This statement is either not well supported or could be written better. If you are stating that the ice margin shows a slower ice flow regime that transitions to a faster ice flow regime as you *spatially* move upstream into the Mackenzie Valley, then please clarify you’re referring to the spatial variability across the region that was occurring simultaneously. If you instead are stating that the slow ice flow regime becomes a faster flowing ice regime at this single location over time, I think that statement needs more support, specifically in stating how your flowsets capture that variability (e.g., because more and less elongate elongate features co-exist in single ice stream systems (McKenzie et al., 2022)).

The statement will be clarified. We are referring to the first situation you describe where the slower to faster flow regime transition occur spatially upstream.

Line 565: What do you mean by “margin retreat was active”? Please clarify if you’re referring to sedimentary processes deforming the bed near the margin or timesteps of retreat or something else.

Active ice margin retreat is characterised by the stepwise, time-transgressive retreat of the ice sheet margin (often resulting in the formation of terminal moraines) as opposed to the widespread stagnation of the ice sheet margin by the process of surface melting with little active ice motion (as commonly associated with ‘hummocky terrain’). We will clarify this further within the introduction to this section.

Line 622: I suggest this be changed to “overprinted on flowsets derived directly from the Great Slave Ice Stream.”

We will make the suggested change.

Line 629: “topographic complexity led to complex cross-cutting flow patterns” – maybe clarify what you mean by “topographic complexity” to reduce the use of the word complex in this sentence.

We will clarify this to say ‘large variations in topographic relief’.

Line 633: What does “this” refer to? The topographic complexity, the complex cross-cutting relationships, or the esker? Please clarify.

This refers to the separation of the ice lobes and will be clarified in the text.

Line 638: In the beginning of this section, you name the geochronological tool used to determine the timeslice. I recommend you do this somewhere in all other 5.2 sections. I assumed it was all using cosmogenic nuclides, but after it being explicitly stated only here, now I am not sure.

We will include a description of the chronological constraints in each timeslice section.

Line 705: References at the end of this sentence?

References will be added.

Line 720: At the end of this sentence maybe add something to the effect of “but it is unclear whether this mechanism of drawdown is strong enough to weaken the entire ice-saddle” to better tie this observation to the following paragraph and the opposing arguments.

We will include this suggestion.

Line 723: Be clearer with the word “topography” – a topographic high? Of what size?

‘a topographic high of ~700m prominence’ will be added.

Line 752: Provide examples of what regional stagnation would look like in the deglacial record like your examples for active margin retreat.

We do not see any examples of regional stagnation across the study region, instead only seeing localised signatures of ice stagnation which are displayed in Figure 10B and D. So, we cannot include any examples of regional ice stagnation.

Line 771: What about the hummocky terrain? If there are any other possible explanations for lack of ice marginal landforms, these should be presented here as well.

This section was unnecessarily oversimplified and following comments from McMartin et al (see open comment), we will expand upon this section to highlight the wealth of evidence and arguments for active ice margin retreat across the Canadian Shield to the east of our study area.

Line 798: Evidence that these moraines are from the end of the Bølling–Allerød?

These moraines fall between cosmogenic nuclide exposure ages and radiocarbon dates within the Mackenzie Valley and cosmogenic nuclide exposure ages on the Canadian

Shield. The newly proposed supplemental figure will highlight these ages to support this statement.

Line 829: The “Instead” at the beginning of this line makes the argument confusing because you used a “but” previously. Please make these two sentences clearer.

We will remove the reference to ice margin retreat processes beyond our study area to avoid any confusion.

Also, please provide evidence or citations for the flowsets you use to assume lower retreat rates and slower ice velocities (are the flowsets less elongate?).

The lower retreat rates are principally based on the ice margin chronology of Dalton et al. (2022) and the chronological constraints depicted on the new supplemental figure will support that. The slower ice velocities are demonstrated by the transition to deglacial flowsets and lineations across the Canadian Shield which indicate a broad-scale ‘sheet’ flow across the whole ice sheet sector, with the absence of any ice streams.

Line 853: Just say “We reconstruct both extensive and shorter time transgressive [...]”

We will amend this as suggested.

Line 867-869: This mention of crevasse-fill ridge networks could use some clarification. How does the presence of these features indicate surging behavior?

We use crevasse-fill ridge corridors as indicative of the shutdown of a surging ice lobe as suggested by Evans et al. (2016).

Evans, D.J.A., Storrar, R.D. and Rea, B.R., 2016. Crevasse-squeeze ridge corridors: diagnostic features of late-stage palaeo ice stream activity. Geomorphology, 258, pp.40-50

Line 875: I would add a “However’ at the beginning of this sentence because these statements contradict each other. Also add a reference at the end of this sentence.

We will amend this as suggested.

Line 889: Expand on the explanation for this. Maybe include something like “allowing for basal shear stress to increase and stabilize the ice during retreat and slow streaming.”

We will expand on the mechanisms for this change as you suggest.

Line 908: clarify that “they” refers to geological conditions?

We will clarify this as you suggest.

Line 921-922: This is not a complete statement. Please clarify this sentence.

This will be amended from:

Rapid ice drawdown during the peak in ice stream activity during the early Bølling–Allerød meant that the thin ice sheet profile on the Canadian Shield and low driving stresses

To:

During the early Bølling–Allerød, the peak in ice stream activity resulted in the rapid drawdown of ice in the interior of the ice sheet and caused a thin ice sheet profile over the Canadian Shield with low driving stresses.

Line 961-965: These lines contain a lot of statements that contradict each other. I would simplify this to say ice streaming occurs across the Canadian Shield only after xyz circumstances are met. I would also make sure you don't mention earlier that there is not ice streaming on the Canadian Shield because there is some back and forth in this and previous sections.

We will make the changes you suggest.

Line 968: I take issue with naming "glacial lakes" as a more important control on ice stream formation than subglacial geology. What is the *mechanism* that is the stronger control? Because if this were marine-terminating, I argue it would be the same pattern, so it's not the lakes but perhaps the onset of crevasse-driven ice loss, or increased ice breakage from loss of buttressing.

This is correct and we will amend the text to better refer to the development of a calving margin as the mechanism for driving ice streaming relating to glacial lakes. Due to our focus on the onshore record, we do not reconstruct any marine-terminating margins, but it is misleading not to highlight the possible similarities to lake-terminating margins.

Also, how does this tie into the topography? Can you say anything about the role of topography in relation to the geologic control. I think it could also be argued that the lakes are a function of topography because the topo has allowed for lakes to develop, so does your argument inherently agree with Winsborrow et al., 2010's argument that topography has a higher control on ice behavior than geology?

We will highlight the possible influence of topography on ice streaming in the text. The extent to which glacial lake development is a function of topography is difficult to determine for this region. The glacial lake location is a controlled by the GIA response of the topography to deglaciation, the retreat of the ice margin and the incision of spillways. While the topography in this situation may play a role in controlling ice behaviour, we opt to avoid these discussions due to the uncertainties.