Stoker et al. Ice flow dynamics of the northwestern Laurentide Ice Sheet

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 Isabelle McMartin and others

Dear authors

Reconstruction of ice flowsets in the northwestern sector of the Laurentide Ice Sheet is important to increase our understanding of ice sheet dynamics in this understudied region of Arctic Canada. You have done a lot of work integrating results from high-resolution remote geomorphic mapping (Dulfer et al., 2023) with the new ice marginal chronology of Dalton et al. (2023). Previous regional reconstructions of ice flow dynamics and ice streams were considered but we are concerned about the lack of consideration for some field-based observations available from regional surficial geology maps, mainly published by the Geological Survey of Canada. Field-based evidence such as striations, till fabrics, and erratic distributions offer direct indicators of past ice movements. Their integration could serve to validate the ice flowsets identified or introduce nuances to the interpretations drawn from geomorphic mapping alone. Please find details in the attached pdf where we briefly present these concerns (and a few others) that could impact some of your interpretations.

Regards,

Isabelle McMartin, Etienne Brouard, Janet Campbell and Pierre-Marc Godbout

We would like to thank these researchers for taking the time to read and provide feedback on our manuscript and for their overall positive comments. We have copied all comments into this document, and we address individual comments below. We would like to acknowledge that some field-based evidence was missed in error and we are grateful that you have brought these publications to our attention so that they may be properly consulted and referenced. At the same time, we would like to highlight that we have made efforts to consult multiple lines of field-based evidence while building and verifying our reconstruction. This includes the recent surficial mapping work from the GSC around Great Slave Lake and the till fabrics described therein (e.g. Hagedorn et al., 2022; Paulen and Smith, 2022) or recently published reconstructions (e.g. Evans et al., 2021). We do not directly include these data in our reconstruction but instead seek to use them to validate our reconstruction and outline our reasons for this approach in detail below. To allow the reader to more effectively compare and validate our reconstruction against the pre-existing data we will create a compilation of selected geomorphology (ice flow indicators and moraines) from previously published works. This compilation figure will form a secondary panel of Figure 1. This will also highlight the progress in surficial mapping that has occurred since Fulton (1995) that is currently displayed in Figure 1.

Field record of ice-flow indicators

The “striation” symbol appearing on regional surficial geology maps includes glacial striae and any other small-scale erosional forms on bedrock (crescentic fractures and gouges, rat tails, nail head striae, stoss and-lee topography, etc., e.g. McMartin and Paulen, 2009). These features provide information on direction, sense, and relative age of ice flows, and on the record of older glaciations, commonly poorly preserved in the geomorphic record. The general orientation of roches moutonnées is also represented by a different symbol on the maps and provides an additional record of the ice flow direction.

It is mentioned in Section 3.4 that “Glacial striations also provide an opportunity for reconstructing former ice flow patterns and may preserve older flow traces on bedrock outcrops where abrasion was limited during deglaciation (Kleman er al., 1990)”. However, we are concerned about the lack of consideration and integration between some of the field-based record and the remotely mapped geomorphology to inform and resolve the flowset directions and age relationships, particularly in areas with complex flow patterns during deglaciation. As mentioned in Section 2, the “understanding of ice flow dynamics … suffers from the disconnected nature of studies at varying scales”. Although the ice-flow indicator record can be very detailed locally, striations indicated on surficial geology maps are often distributed regionally and can inform on former and deglacial ice flow patterns where no landforms are preserved. Field-based ice-flow indicators can easily be extracted from surficial geology maps or Open File reports, all available in digital format (using Advanced search in https://ostrnrcandostrncan.canada.ca/home), and compiled at the ice sheet sector scale. Such large-scale compilations have been completed recently directly east of the studied area in the west-central Keewatin sector of the LIS (Brouard et al., 2022; Fig. 5 below) and further east along Hudson Bay (Behnia et al., 2020; McMartin et al., 2021; DataS2c below) to help constrain the regional glacial history.

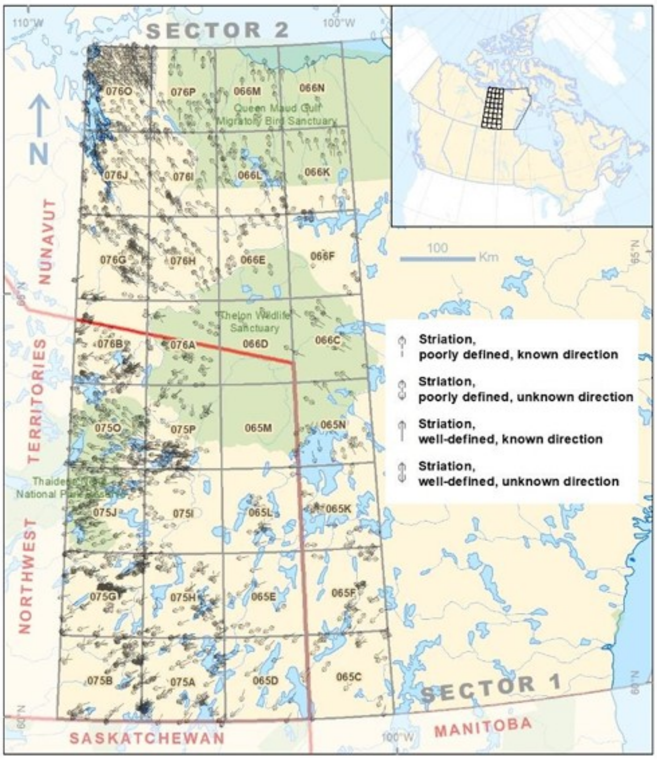
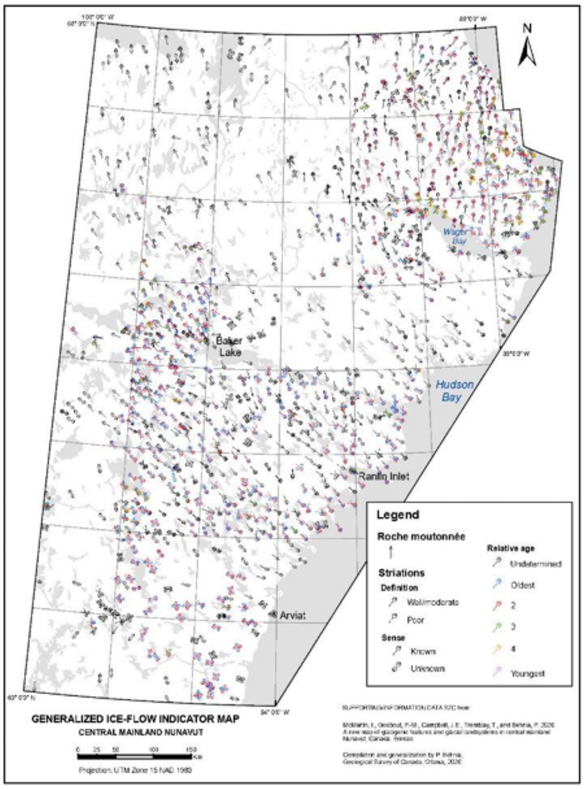


Figure 5. Location of ice-flow measurements in west-central Keewatin and their associated ice-flow orientation (From Brouard et al., 2022).



DataS2c. Generalized ice-flow indicator map, Central Mainland Nunavut (From McMartin et al., 2021).

We have chosen not to incorporate ice flow indicators from mapped glacial striation directly in our reconstruction because maps of striations are not available consistently across our study area. The surficial geological maps where the data is recorded are produced at a wide range of scales (1:50,000 to 1:250,000), some are produced solely as a ‘reconnaissance’ map from remote sensing data (Kerr, 2014 and further references in the section below discussing map resolution), and some regions are currently unmapped. As such, the integration of this data would not be compatible with our aim to produce a reconstruction that is consistent across the whole study area. A high-quality, broad-scale compilation (such as the recently completed Brouard et al., 2022) would be very welcome for our study region and would allow for the integration of striation data into our reconstruction. But, unfortunately the map area of Brouard et al. (2022) falls outside of our study area and the creation of a compilation like this is beyond the scope of our study. However, during the production of the glacial geomorphological map in Dulfer et al. (2023) we did consult and verify our geomorphological map against the existing surficial geological maps for the region and in the publication we described the accuracy and completeness of our map compared to these data sources.

Interpreted ice-flow indicators and glacial histories from some regional field-based studies were considered (e.g. Smoking Hills), but other available regional maps and Open File reports should also be verified in light of the proposed reconstruction. In this regard, an ice-flow data compilation in the Great Bear Magmatic Zone (Normandeau and McMartin, 2013; Fig. 4 and Appendix II) should be taken into consideration, as well as a regional deglacial history reconstruction in the northeastern Horton Plain region, particularly for the interpretation of a NE(?) flowset (Fs-230) and other relative ages of flowsets 224, 225, 231, 323 (St-Onge and McMartin, 1995 and references herein).

Thank you for drawing our attention to these reconstructions (Normandeau and McMartin, 2013; St-Onge and McMartin, 1995), they were omitted in error and a comparison to these reconstructions will be made in the revised manuscript.

- A note on our work process and the justification of it:

Throughout this research, from the creation of the geomorphological map presented in Dulfer et al. (2023) to the ice flow reconstruction, we made efforts to verify our work against the pre-existing work. The final step in the creation of the geomorphological map involved the comparison of our mapped landforms to the available surficial geological maps produced by the GSC to ensure a reliable end product, but we did not seek to directly include any features from the surficial maps that we could not independently verify. This approach allowed us to verify and validate our map to ensure that any inaccuracies or limitations in our map were identified while maintaining a consistent map product across the entire area.

During the ice flow reconstruction, we followed a similar approach of verifying our reconstruction against the available regional ice flow reconstructions. The intention of this was to highlight any potential shortcomings of our work or to identify where our reconstruction has allowed us to add further detail to the existing knowledge. Unfortunately, due to the scale of the region some key references were missed and we regret the omission of these from the manuscript. Thank you for highlighting these studies and we will make sure to properly cite them in the updated manuscript.

However, the guiding principle throughout our process was to produce a (relatively) high-resolution reconstruction of the changing ice flow dynamics during the deglaciation at a consistent scale and level of detail across the whole study region. As such, we did not seek to directly include any pre-existing reconstructions within ours, as this would lead to spatial inconsistencies in the reconstruction. This meant that even data from co-authors on this paper (see Evans et al., 2021) was not directly integrated into this reconstruction, but instead referenced and highlighted as a more detailed regional study. This allowed us to produce a consistent reconstruction across the ice sheet sector and then to make comparisons between our reconstruction and the pre-existing data to validate our efforts or highlight issues. This includes published papers (e.g. Evans et al., 2021) and also GSC maps and reports where possible (e.g. Hagedorn et al., 2022; Hagedorn 2022 and other examples).

We also suggest a comparative analysis with Dyke and Prest (1987), the only other reconstruction offering a continental overview that shows ice-flow drainage and incorporates the extensive fieldwork data accomplished in Canada since the end of the 19th century. This comparison could provide a richer context for this study, especially in contrast to works primarily based on remote mapping. While the cost-effective approach outlined in this study is certainly practical, a more thorough integration of fieldwork data might offer a comprehensive perspective, reducing the reliance on subjective interpretations, particularly regarding the provision of relative chronology through striations.

Thank you for your comment. During the preparation of the geomorphological map produced in Dulfer et al. (2023) and in the reconstruction produced here comparisons were made to Dyke and Prest (1987). The relatively broad-scale nature of Dyke and Prest (1987) ultimately meant that it did not contribute much to our reconstruction or provide any new details and we opted not to discuss this further within the text.

We are unsure what is meant by ‘subjective interpretations’. Beginning from the mapping procedure, two mappers independently mapped and verified each others mapping to ensure the reproducibility of the map product. The relative chronology developed here is based on the same geologic principles that underpin reconstructions based on striations and was carried out multiple times, independently by two separate researchers (Helen Dulfer and Ben Stoker). Additionally, all cross-cutting relationships, the details of the relative chronology and the justification of choices is provided in both the manuscript and in the supplementary table.

Throughout the whole process we prioritised transparency and reproducibility, hence the provision of all shapefiles, locations of identified cross-cuts, etc. in the supplementary material. We believe our methodology has produced a robust reconstruction and our transparency will allow future researchers to independently verify our mapping and interpretation using desk-based, low-cost methods or easily integrate future detailed field studies to improve upon our reconstruction. The integration of striation data is not within the aim of our reconstruction due to spatial inconsistencies in the scale of mapping and the absence of striation mapping from entire portions of our study area.

In conclusion, we follow an established flowset mapping approach and we have sought to be as thorough and transparent in our approach as possible. To achieve this we explain in detail throughout the text our choices, include supplementary files with extensive information on our flowsets, including the location of cross-cuts that inform our relative chronology.

Regional stagnation to the east of the studied area

In section 6.2.3, it says “Therefore, we suggest that widespread stagnation had not begun for the northwestern LIS margin to the west of 210°W but may have occurred further to the east, as documented by Sharpe et al. (2021).” First, it should probably be 110°W instead of 210°W, and in the Conclusions on page 47 again: “If widespread stagnation of the margin occurred, we suggest that it must have occurred after the LIS retreated to the east of 110°W.”

Thank you for the note, it is correct that it should read 110°W.

More importantly, no basis for the assumption that widespread stagnation may have occurred further east of the studied area is provided, other than what is presented in Sharpe et al. (2021). Recent and current high-resolution geomorphic mapping using ArcticDEM and extensive field-based studies from east of 108°W to the Hudson Bay coast does not support or indicate widespread stagnation (Campbell and Eagles, 2014; Campbell et al., 2016, 2018, 2019, 2020; Lauzon and Campbell, 2018; Livingstone et al., 2020; McMartin et al., 2021; Brouard et al., 2022, 2023; Dyke and Campbell, 2022; Vérité et al., 2024). In contrast, numerous evidence for a sequential, time-transgressive retreat is presented in these publications. If the authors of this study have not mapped east of 110°W, they should not assume or propose that a widespread stagnation characterized ice retreat east of their studied area unless a proper discussion with alternative views is presented.

We agree with this comment and will amend the text to better reflect the differing views on ice margin retreat processes across the Canadian Shield. The formulation of the sentence was not intended to imply that widespread stagnation does occur to the east of 110°W. Rather, it was our intention to describe the ice marginal retreat processes we reconstruct to the west of 110°W and then we contrast these processes to the ice marginal processes documented by Sharpe et al. (2021) to highlight that we have not observed similar evidence. As we had not mapped to the east of 110°W, we wanted to remain ambiguous about the processes which occurred in this region. Unfortunately, the wording is a little clumsy and does not accurately represent this. It will be re-written to include the references you suggest and to highlight the evidence for time-transgressive, dynamic margin retreat, which matches with the processes we observe in our study area.

Ice streams on the Canadian Shield

At Line 961, it says: "While hard-bed conditions are known to exert a stabilising influence on ice flow, large ice streams are observed across the Canadian Shield." Yes, further east in Keewatin over the Shield, large ice stream footprints have been reconstructed from geomorphic mapping (including high-resolution mapping with ArcticDEM), including not only the Dubawnt Lake ice stream (e.g. Stokes and Clark, 2003, 2004) but several other large ice streams (Margold et al., 2015, 2018; McMartin et al., 2021) and not necessarily involving glacial lakes (i.e. marine-terminating ice streams).

We thank the authors for their suggestion. We agree that it is probably an important point to highlight that glacial lakes themselves that act as the direct trigger for ice streaming, but the presence of a calving margin which can occur at lake or marine terminating margins. We reference the Dubawnt Lake ice stream as it represents the first observed reactivation of ice streaming on the Canadian Shield for this specific ice stream sector. In Stokes and Clark (2003, 2004), it is suggested that the development of glacial lakes acted as a trigger for ice stream. As such, we use the Dubawnt Lake Ice Stream purely as an example to highlight that although ice streaming is reduced on the hard-bedded Shield, it is still possible, and to gain an insight into the broader controls on ice streaming. The examples you raise (e.g. McClintock Channel Ice Stream, McMartin et al., 2021) also provide good examples of hard-bedded ice streams and will be integrated into the text to provide further depth to our discussion.

At Line 965, it says that "The development of the ice-marginal Dubawnt Lake to the east of our study is hypothesised to have triggered the Dubawnt Lake Ice Stream… (Stokes and Clark, 2003, 2004)". This is incorrect. Stokes and Clark conclude that "As the ice margin retreats back, early glacial lakes form in the Thelon and Back River drainage basins. These early glacial lakes in the Thelon River basin increased in size and deepened, triggering the Dubawnt Lake ice stream." They further suggest that "This results in widespread thinning of the ice sheet and produces a large glacial lake to the south of the ice stream, dammed by the Dubawnt Lake ice stream lobe ". This glacial lake is west of Dubawnt Lake (Stokes and Clark, 2004; Fig 8).

You are right. We will correct this within the text.

Another point concerns the influence of crystalline bedrock on ice-margin stability. The absence of ice-flow landforms on the Canadian Shield could be attributed to several factors. 1) The scarcity of sediments associated with harder-to-erode crystalline bedrock may hinder landform production. Thus, the absence of ice-flow landforms on crystalline bedrock does not imply the absence of ice streaming (especially on the eastern part of McConnell Lake); it merely indicates that no landforms were preserved. 2) Because crystalline bedrock is more challenging to erode, ice-flow landforms that directly record bedrock are generally smaller and likely too small to be noticed at the scale mapped. Utilizing ArcticDEM at a more detailed scale, such as 1:20,000 or 1:10,000, might have revealed these features.

> We are confident that we have captured all large-scale ice flow (e.g. ice streams) across our map area, including on the Canadian Shield, that is recorded in the glacial landform record for the following reasons:
>
> 1) Our flowset coverage highlights landforms are observed over much of the region, including the eastern part of glacial lake McConnell. While it is true that the geomorphological imprint of ice streams on hard-beds would be different to that of ice streams on soft-beds, we do not imply an absence of ice streams based purely on the absence of landforms (as we have landforms mapped), nor solely by the elongation of these landforms. Instead, the broad-scale, diverging sheet flow that we observe on the Canadian Shield is used to infer the absence of ice streaming. There is evidence of ice flow across much of the Canadian Shield in our study area, but none of this ice flow evidence indicates any large-scale ice streaming activity (e.g. hourglass-shaped, converging and diverging flow patterns).
> 2) While we state in Dulfer et al. (2023) that mapping was performed at 1:50,000 to 1:100,000, this was a conservative estimate used to guarantee the minimum level of detail provided by our map. In areas where landforms were absent, a more detailed mapping scale was adopted to verify that the absence of landforms wasn't an artefact of the mapping scale. In addition to this, all mapping was compared to available surficial geological maps to ensure that there were no large areas of ice flow indicators absent from the map. Indeed, this comparison did not identify any large omissions in our map.

ArcticDEM and high-resolution mapping

While discussing the benefits of high-resolution data for understanding glacial history, the authors did not actually map at the highest spatial resolution provided by ArcticDEM (2 m). Instead, they mapped at scales ranging from 1:50,000 to 1:100,000 (Dulfer et al.,

2023) to cover as much area as possible. This mapping scale is similar to, or even lower in resolution than that in studies that used aerial photographs (1:15,000 to 1:60,000). The advantage of ArcticDEM lies in its complementary data, which enable the production of hillshades at a higher resolution than previously available (about 20-30 m resolution). While we understand the necessity of cost-effective methods, it appears there was an aim to produce a regional scale product with supposedly high-resolution data, yet the execution was only partially completed. The authors should be cautious with their claims of using high-resolution data, as their practice does not match their assertion.

As previously mentioned, the map scale provided (1:50,000 to 1:100,000) is a conservative estimate to best highlight the minimum map product provided, mapping of course occurred at much higher scales in many locations.

You highlight our mapping scale is lower than the resolution of aerial photographs but the resolution of surficial map products produced by the GSC is not always produced at the true resolution of aerial photographs. In total there are 82 NTS map tiles covered by our reconstruction. As of the publication of Dulfer et al. (2023), 8 of these NTS tiles are unmapped by the GSC, 1 tile is mapped at 1:50,000, 45 tiles are mapped at 1:125,000, 20 are mapped at 1:250,000, 2 full tiles and ¾ of an NTS tile are mapped at 1:100,000. Finally, 7 tiles are only covered by district-scale surficial geological maps at 1:500,000 scale (Craig, 1960; Rampton, 1988). In comparison, our mapping is presented at a much higher resolution for the vast majority of the study region. Although, some individual surficial geological map tiles do exceed the resolution of our map, especially the recently completed surficial geological mapping around the Great Slave Lake region (Hagedorn et al., 2022; Paulen and Smith, 2022). As such, we believe it is a fair comment to say that our reconstruction is at a high-resolution.

In addition to the range of mapping scales, the mapping procedure employed is not consistent across the entire region. Some NTS tiles mapped by the GSC are only covered by reconnaissance mapping efforts, relying on remote sensing data and limited field evidence (i.e. similar procedures to this study, see Table 1 in this document, below this comment). These reconnaissance maps are vital to providing total coverage of the area. Some of the striation data is taken from unpublished datasets, preventing these data from being scrutinised (e.g. Kerr, 2018).

Throughout the production of the Dulfer et al. (2023) map, the surficial geological maps from the region were compiled and extensively compared to our map product to assess the effectiveness and reliability of our mapping procedure.

Table 1: A brief summary of some of the variations in mapping procedure across existing surficial maps

Reference	Map scale	Fieldwork?	Data source
Kerr (2014, 2018)	1:60,000	'Limited fieldwork', some striation data is based on previously unpublished work	Airphoto interpretation
Kerr (2022)	1:60,000	None	Airphoto interpretation
Kerr and O'Neill (2017)	1:125,000	Striation information taken from older publications	Airphoto interpretation
Kerr and O'Neill (2018a and b, 2019a and b)	1:125,000	Striation data from recent publications	Airphoto interpretation
Kerr and O'Neill (2019c)	1:70,000	Striations from older publications (Craig, 1960)	Airphoto interpretation
Kerr and O'Neill (2020 and 2021)	1:60,000	No fieldwork	Airphoto interpretation
Kerr et al (2014)	1:60,000	Limited fieldwork	Airphoto interpretation
Kerr et al (2016)	1:70,000	No fieldwork	Airphoto interpretation
Olthof et al (2014)	1:125,000	No fieldwork but takes striation data from Kerr (1990) which only partially covers NTS tile 85-P	Predictive mapping based on LandSat data

Uncertainties

Furthermore, the authors acknowledge a 1000-year uncertainty commonly associated with cosmogenic (10Be) ages, which is foundational to the study's framework for ice-margin retreat. While this initial recognition is valuable, a more thorough discussion on how this uncertainty influences subsequent interpretations, particularly regarding suggested peak ice-stream activity at the onset of the Bølling Allerød interstadial, would greatly enhance the narrative's robustness. Given the acknowledged uncertainties and the inherently subjective nature of both the approach and Dalton's ice-margin reconstruction, a more detailed exploration of these aspects could help strengthen the study's conclusions. While we agree with the authors' assertion that the Bølling-Allerød warming significantly influenced ice sheet melt and dynamic changes, further elucidation on how these uncertainties were navigated in reaching such conclusions would provide clearer insight into the analytical rigor applied throughout the study.

Due to the length of the paper, which is already quite long, we are reluctant to add much greater discussion on the chronology beyond what was mentioned in previous papers (e.g. Stoker et al., 2022; Dalton et al., 2023). However, you raise an important point regarding how the chronology might influence our conclusions and I will provide further information on why we do not think it will impact out conclusions. Within the text we will provide a brief paragraph summarising the influence of age calculation on our reconstruction and why we do not believe the chronology will significantly change in a manner that would impact our conclusions.

- Age calculation approach:

In brief, there are three methods of calculating exposure ages that have been used for the northwestern Laurentide Ice Sheet. I will explain simply the implications of these methods on the calculated age, rather than any reasoning behind the calculation method (covered in Stoker et al., 2022).

The exposure age calculation approach by Dalton et al. (2022) falls in the middle of the age calculations, so provides a middle ground in reference to the other two approaches. The calculation approach employed by Reyes et al. (2022) results in exposure ages that are approximately 500 years older than the reconstruction of Dalton et al. (2023). In contrast, the calculation approach used in Stoker et al. (2022) results in exposure ages that are approximately 500 years younger than the approach of Dalton et al. (2023). In essence, this means that the peak in ice streaming would occur approximately one timestep earlier if the ages were calculated following the Reyes et al. (2022) approach or one timestep later if following the Stoker et al. (2022) approach.

This means that (based on Figure 11 in the manuscript) using the age calculation method of Reyes et al (2022) the peak in ice stream activity would already occur immediately prior to the Bølling–Allerød. In the reconstruction of Dalton et al. (2023), ice stream activity slowly increases immediately prior to the Bølling–Allerød but then peaks at the start of the Bølling–Allerød before slowing down towards the end of this period. Based on the age calculation approach presented in Stoker et al. (2022), the peak in ice stream activity would be situated later in the Bølling–Allerød and the slowdown in ice stream activity would still occur during the Bølling–Allerød.

We opt to use the Dalton et al. (2023) reconstruction as it manages to effectively combine all pre-existing constraints (exposure ages, luminescence ages and radiocarbon ages) and it represents the current best guess reconstruction of the ice sheet chronology. This multi-chronometer method means that we can be fairly certain that the chronology will not change significantly, as all existing age constraints are satisfied and it is not solely dependent on cosmogenic nuclide exposure ages. As outlined in Stoker et al. (2022), we believe that the chronology created by Reyes et al. (2022) is slightly too old to be compatible with the pre-existing age constraints, so we reject this chronological framework. While the chronology presented in Stoker et al. (2022) is also compatible with other chronometers, the change by using this chronology would be to better align the peak in ice streaming with climatic oscillations during the Bølling–Allerød and Younger Dryas and still supports our conclusions.

Therefore, based on the Dalton et al. (2023) and Stoker et al. (2022) chronologies, we can be reasonably confident that this peak in ice streaming occurred during the Bølling–Allerød and the slowdown in ice streaming also occurred (or at least began) prior to the Younger Dryas.

- The spatial relationship between ice sheet thinning and ice stream activity:

Changes in the age calculation approach shift the reconstructed ice stream activity in time and could change the association with different climate events. However, a period of rapid ice sheet thinning is spatially linked to our reconstructed peak in ice streaming. This supports our conclusion that the mechanism driving changes in ice streaming is the steepening of the ice sheet surface slope during surface mass balance/elevation feedbacks (i.e. ice saddle collapse). Regardless of the age calculation approach, two cosmogenic nuclide dipsticks in the Mackenzie Valley region (one at 63N and one at 65N) indicate a period of rapid thinning in this region (Stoker et al., 2022). While the absolute timing of this thinning event can shift through time, the occurrence of rapid thinning at this location does not change. Therefore, we can associate the process of rapid thinning with increased ice stream activity in this region, even if we do not link these events to a climate event. Furthermore, numerical modelling studies have linked changes in the ELA and subsequent changes in the ice sheet surface slope to increased ice streaming during deglaciation (Robel and Tziperman, 2016).

We provide an extensive discussion of the uncertainties of dating in Stoker et al (2022), including an explanation of why we favour a ‘younger’ exposure age calculation that better fits with the pre-existing constraints in the region. In Dalton et al. (2023) there is also a clear explanation and appreciation of the uncertainties, with the exposure age calculation approach of Dalton et al. (2023) trending slightly older than the method used in Stoker et al. (2022). We do not seek to reproduce those discussions here.