

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 reviewer 2

The paper by Stoker et al. provides a detailed assessment of the geomorphic features eroded and deposited by the NW sector of the LIS. The authors provide detailed flowlines of the LIS from the LGM through the last deglaciation and from these reconstruction attempt to interpret the style of deglaciation of this sector of the ice sheet. Overall, I find what the authors have done to be worthwhile and useful for interpreting how this sector of the LIS transgressed from the LGM to end of YD. However, I find it difficult to ascertain how the authors have arrived at their conclusions about the time-transgressive nature of the LIS demise without the chronologic information provided alongside their reconstructions. To me, this is the major weakness of the paper and without this information it does not allow the reader the ability to properly assess their interpretations, or at least easily assess it without digging through other publications and comparing and contrasting. I think with this information provided in the figures and within some of the text, it will improve the paper substantially and give readers a very nice assessment of the history of this part of the LIS alongside their geomorphic mapping and interpretations. My detailed comments are provided in the accompanying PDF with other suggestions for improving the manuscript.

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

Line by line comments:

Page 1:

Line 14 - Clarify what "it" means in this sentence. This will help with clarity for the reader.

This will be clarified to 'the northwestern Laurentide Ice Sheet'.

Line 18 - Define the period over which you mean for the reader.

This will be added.

Page 2:

Line 41 - I am not sure what the authors mean here.. Are the authors saying it (the LIS) attained a similar area as the modern AIS?

In any case, I don't think the extent really matters for why the ice sheet provides a test case for studying ice streams. I think the opportunity is simply that there are geomorphic features that may indicate paleo-ice streams and that alone makes this a nice place to study ice streams.

In this sentence 'it' referred to the Laurentide Ice Sheet. We will amend the text to clarify.

Line 43 - In the abstract it says the local LGM was at 17.5 ka. Here the authors say the maximum was during the global LGM which is defined in Clark et al. 2009 and does not extend to 17.5ka. I suggest the authors clarify this so the readers are not confused.

In the abstract we refer to the local LGM of the northwestern sector. The 'maximum' in line 43 refers to the maximum extent of the Laurentide Ice Sheet, not an individual sector. We will amend this to clarify.

Line 46 - I think the authors should not attempt to re-classify LGM. Clark et al. 2009 has done a nice job of defining this and I think to not confuse the readers the authors should use local LGM which they do in the abstract. Redefining will confuse people and "local" allows flexibility in the regional definition.

We will adopt this terminology as recommended.

Line 47 and 48, relating to 'numerical modelling' - Provide a citation

We will include a reference here to Gregoire et al. (2016).

Line 48, relating to 'Bolling-Allerod' - Define the time period here.

We will include the time period.

Line 48, relating to 'ablation area' - Of the LIS or all NH ice sheets? Be explicit for the reader.

We will clarify that this refers to the western Laurentide Ice Sheet and the ice saddle with the Cordilleran Ice Sheet.

Line 50 to line 53 - This link the authors are calling on is a little hard to decipher. Perhaps in this sentence less is more and the authors can simply end the sentence at "...13.5 and 11.5 ka."

This is confusing. The BA interval pre-dates this interval, so how is this connected?

Pico et al. (2019) do not place the collapse of the ice saddle within the BA interval and instead suggest it occurs much later, during the Younger Dryas. We will clarify this in the text.

Page 4:

Line 80 - These questions are great to provide for the reader but I think equally important is provide the reader with the hypotheses that are being tested as well in this paper.

In general, we agree that hypothesis-based research provides the best framework to help guide readers through the manuscript. But, in our situation we believe that some of our research questions have clear hypotheses (e.g. question 1) while some do not (e.g. question 2). Developing hypotheses for each question would require us to split some of the research questions into multiple elements and would become overly complicated and confusing for the reader. So we opt to retain only the research questions.

Page 6:

Line 136, relating to 'a short-lived' - I am confused with this part of the sentence. Do you mean it was short lived? Can you just say how long it remained at it's maximum position?

Within this sentence we provide to dates of the maximum extent (~20ka) and the approximate timing of deglaciation (~18ka). We believe this is sufficient for the readers to determine the length of time this ice sheet sector remained at its maximum extent. Especially compared to the extended duration proposed in the previous sentence (~30ka to ~22ka).

Page 7:

Line 195 - It's not clear to me how they were associated. Is this defined elsewhere? Is it done through cross cutting relations or simply if eskers and/or moraines are near flow sets?

The criteria used to define flowsets is outlined in Table 1.

Page 13:

Line 261, relating to 'all-time maximum' - These are redundant.

This statement is not redundant. It is used to clarify that no glaciations prior to the LGM were larger.

Page 17:

Line 353 - It isn't clear what "matches" means. Do the authors mean agreement? Or, is it something else.? If 64% match, does this mean that 36% are mismatched and therefore agreement is not very good? Some clarity here would help the reader.

The term 'match' is a purely subjective measure of whether flowsets from our reconstruction were also identified in other reconstruction. While a 36% omission of flowsets might sound significant, as we state in the text, the majority of the flowsets that we newly identify are small in size and only add detail to our reconstruction.

Page 20:

Line 408 - I think it's also important to cite the original chronologic work that provided the timing of this age. See references within the Dalton et al. paper. This is important for attributing the original people who did this work and not just the compilation paper of Dalton et al.

We will add these references.

Page 21:

Line 451 - I am finding it very difficult to see how the authors are able to parse out the ice sheet changes in 500 year intervals when the chronology in this region doesn't allow for it. I understand the authors' assumptions and can appreciate what they are trying to do, but laying out the structure in this detailed manner (to me) is giving the impression to the reader to much confidence in the chronology and relating to the geomorphic structures. Perhaps other experts will be okay with this but I think there might be too much inferring to make these detailed reconstructions at 500 year time steps. This of course is the central crux of this paper but I would prefer the authors be more conservative with their reconstructions and at least bin them into intervals of 1000s of years and not in these 500 year snapshots which I don't find convincing without more detailed chronology. See my comments on Figure 6 regarding this section.

The reconstruction of ice flow changes itself is not based on the chronological data. Instead, a relative chronology of ice flow changes is based on the observed cross-cutting locations, as detailed in the supplementary table and in the supplementary flowset map figure. This relative chronology is then fit to the chronological framework of Dalton et al. (2022). As is visible in the figures, there are dramatic variations in ice flow direction and dynamics that occur even at shorter timescales than our 500-year windows depict. This meant in some figures it was necessary to highlight two separate flow directions within one figure panel (e.g. Figure 6I). For example, over ~1,000 years at the start of the Bølling–Allerød there is a complete 180° reversal in ice flow direction in the Mackenzie Valley at ~65N. If we were to use 1,000 year timesteps then the complexity of these variations would not be depicted. These changes, recorded by cross-cutting relationships (as shown in detail in Figure 9), are key to identify the

changing dominance of different ice source regions. While new chronological constraints or methodological improvements change the exact timing of our reconstructed ice dynamics, we believe that the sequence of events and how the ice flow dynamics relate to various ice margin positions is robust.

Providing the chronologic constraints on the figure would go a long ways in giving the reader a better sense of how the authors are deriving these reconstructions. It will also provide the readers with all the information. I understand they are providing the published 500 years intervals from Dalton et la. but I still think providing the location and ages that those reconstructions are derived is important for this more detailed study.

There has been extensive discussion of the chronology of the western Laurentide Ice Sheet in recent years, we do not seek to recite these discussions here as the manuscript is already quite long. To avoid adding further detail on to the small panels of Figure 6, we will produce a supplementary figure that recreates figure 6 with each panel at A4 size and we will include the chronological constraints on these panels. There will also be a brief sentence added for each timeslice description in section 5.2 to highlight the geochronological constraints that are available.

Line 454 and 455 - Cite Dalton et al. here who also noted this lack of information from 25-17ka based on lack of chronology as well.

This will be included.

Page 29:

Line 675 - To make this figure more useful to the reader, I think the authors should provide the location and chronologic constraints for their interpretation on the map (ie dates from whence the data came from and location).. The reader shouldn't have to go back and forth between this paper and the Dalton et al. work to do this., and I think the authors would be making a clearer point about their assumptions by providing the ages on this plot.

As detailed in the previous comment, we will include this information on a supplementary figure.

Page 33:

Line 686 - This is beautiful DEM but I am not sure it is necessary for the main body of the manuscript. I would consider moving it to supplemental.

We will move this to the supplementary information.

Page 34:

Line 697 - This figure is interesting but I'm not sure it is necessary for the main body of the paper since the paper is largely about the ice history and not the lakes.

We believe that this figure should remain in the main text. One of our key aims is to understand the controls and drivers of ice stream activity and one of these factors that we investigate is glacial lakes. We believe it is important to show the location of the main glacial lakes in the region for the reader. It was not possible to include this information on one of the other figures.

Page 35:

Line 703 - Relative to what? This should be clarified.

We will re-write this sentence to clarify that there is more rapid thinning in the ice saddle zone compared to the Keewatin Ice Dome.

Line 710 - What is meant by strong? Clarify for the reader.

This will be changed to 'northwards [ice flow](#) signal'.

Line 719 - This may very well be the case but I think it is important to provide the reader with the information about why "envisaging" this is difficult.

We will amend this to clarify that the Amundsen Gulf Ice Stream was likely important due to its large size and likely high calving rates at its marine-terminating margin.

Line 726 to 728 - Again, I think it is very important to show some of these dates with the new reconstructions. It provide the reader with enough information to make their own assessment of what the authors are arguing.

These constraints will be detailed on the previously described supplementary figure.

Page 37:

Line 741 - Without the ages to constrain the geomorphic features, I cannot provide much feedback in this part of the discussion. I understand that the authors have used the reconstructions from Dalton et al. but it would be beneficial to see those ages on the figures in this paper and to evaluate what the authors have interpreted. Otherwise, I am not sure it's possible to provide meaningful feedback in this section.

As we previously mentioned, the chronological constraints will be marked on a supplementary figure. However, we do not believe the chronological data is essential to determine whether the nature of the ice margin retreat processes. Instead, the reconstruction of the process of ice margin retreat builds upon a series of previous publications that rely principally on analysis of the ice marginal geomorphological signature, without any mention of chronological data (e.g. Dyke and Evans, 2003; Evans et al. 2021; Livingstone et al., 2020; McMartin et al., 2021; Sharpe et al., 2021).

Page 40:

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).

Page 43:

Line 923 to 925 - Hard to ascertain without the age information in the figure.

The retreat of the ice margin on to the Canadian Shield is directly constrained by two sites dated with cosmogenic nuclide exposure dating (Reyes et al., 2020). This will be detailed on the supplementary figure.

Page 44:

Line 942 - I'm not sure this section is necessary and seems to just be a review of what drives ice streams.

This section is intended to highlight some of the other factors that may have influenced ice stream activity and were not described in the previous section due to the difficulty to test them within our study. It aims to provide an element of transparency to the reader and highlight factors that future studies could attempt to better test.

Page 45:

Line 969 - This section also seems tangential to the main points of the paper. I appreciate these sections but they seem to distract from the main thrust of the paper.

Agreed. This will be removed and replaced with a sentence in the main portion of the discussion to highlight this as an uncertainty.

Line 977 - 'empirical record is difficult and we are thus'

We will make this correction.

Line 985 and 986 - Again, and sorry to go on about this, but I find statements like this hard to justify without more information on the chronologic basis of the reconstruction of this paper. Without the ages clearly outlined for the reader, I find most of the descriptions in the text about the geomorphology general fine but without any basis. It is hard for me to assess this paper without that information and difficult to provide constructive feedback to the authors.

We will include the age constraints on a supplementary figure that will illustrate how these changes in ice stream activity are tied to both radiocarbon and cosmogenic nuclide exposure ages. We will also include a brief section that will highlight the uncertainties in the chronology and what the implications of changing the chronology would have for our interpretations.

Page 46:

Figure 11 - I don't find this figure adds too much to the paper's main focus. I would consider removing to help streamline the paper and focus it on the main points as outlined in the conclusion.

We disagree. This figure is key to understanding the controls on changing ice stream activity and underpins section 6.4. The key finding that relates the peak in ice stream activity to climate-driven changes in the ice sheet surface slope is dependent on this figure. We will amend the text to better highlight the importance of this figure and interpretation.

Page 48:

Line 1049 to 1050 - Good to acknowledge the original people but better if they were included meaningfully in the paper.

In any case, can the authors acknowledge specific people they worked with during the project (ie tribal members, leaders, or councilors)? Otherwise, I personally find these statements performative as a person with indigenous heritage- e.g. the nations are recognized but were never asked for permission to map and describe their lands. I could say more but I leave this for the authors' to consider and apologize if one of the coauthors is a tribal member and I overlooked their affiliations and/or heritage.

Thank you for this comment and for raising an important issue. We agree that these statements can be performative if they are empty and without any thought or action. The research permitting system for the Northwest Territories is principally focused on field-based studies, with no permitting system to facilitate communication with First Nations communities for remote studies. However, during the course of our research in the region, we have been involved in multiple applications to the Aurora Research Institute for fieldwork permits. These applications are predominantly focused on the field component of our research but do also describe our intentions to undertake remote-sensing, geomorphological mapping work. Following the publication of this research it is our intention to submit a one-page summary report to the Aurora Research Institute and interested local communities (e.g. the Sahtu Renewable Resources Board, who have previously expressed an interest in this work).