Monday, June 30, 2025

To the editorial team:

We thank you and the reviewers (including the unsolicited comment) for providing the impetus for us to improve the manuscript by doing a major revision, essentially a rewrite of the entire manuscript.

In response to the review suggestions, we have refocused the manuscript. While the data remain the same, our approach has changed significantly, specifically to address weaknesses pointed out by the reviewer comments.

In revision, we have:

1. Refocused the manuscript more on data and less on models of others, both numerical and conceptual. We have done this by adding additional data (a depth profile) and by compiling all extant cosmogenic data for the region in order to do a meta-analysis of inheritance. Following a reviewer's suggestion, we analyzed those compiled data to show the frequency and magnitude of nuclides inherited from exposure prior to the LGM in published bedrock and boulder data sets.

2. Added numerous references and redone the manuscript citation from scratch using a citation manager.

3. Revised all figures and added several new figures both in response to reviewer suggestions and because we have performed different data analyses.

4. Moved some information to the supplement to streamline the manuscript.

5. Addressed all reviewer comments that remained relevant after the manuscript was rewritten.

Thank you for considering our revised manuscript for publication. Please note that with inclusion of the depth profile data, we have expanded the author team.

Note that Peyton Cavnar remains first author but because she has graduated and left the university, please add Paul Bierman as a second corresponding author.

Sincerely,

Paul Bierman on behalf of the author team

REPLY TO REVIEWER 1: BRINER

Review of Cavnar et al., "In situ Cosmogenic ^{10}Be and ^{26}Al in Deglacial Sediment…."

Responses to reviewer comments are in red below.

Cavnar et al produced 10Be and 26Al measurements in sediment deposits sampled from varying distances along paleo-flowlines of the former Quebec-Labrador dome of the Laurentide Ice Sheet. Varying amounts of nuclide inheritance in both modern sediments and early Holocene landforms allow the authors to make inferences about limited glacial erosion in this region during past glaciations. Based on prior work in this and other ice-sheet dome centers, it is expected to find inheritance signals; however, the exact quantities may be relevant for crn tracer studies in offshore sediment cores.

That this paper does not relate to geochronology leads me to be somewhat neutral about the journal choice.

I liked this paper a lot. I enjoyed reading it and found it grammatically clean. The results were interesting to read about and mull over. The figure and table support is solid.

I have several comments that the authors could consider.

We appreciate your review.

1) This is great work, I wonder if the pitch for relevance to offshore work could be amped up. The title could be more informative, for example, by including a conclusion. As I write in my synopsis above, it strikes me that the findings are not highly novel, perhaps even only mildly informative, when couched in terms of inferences on past ice sheet exposure/burial/erosion. I don't think anyone would expect a story from the sediments to be any different than that from surface boulder and bedrock crn data, in addition to other geomorphological datasets, that already exist. That said, it seems to me that the real new information, and more important motivation for this work, is to inform new offshore sediment records like those of LeBlanc et al (2023) and others. To this end, I would suggest being more up front about this motivation (like including this impetus in the abstract and giving it more emphasis throughout the paper; it's there, but I found it not front and center).

Considering all three reviews we have changed the interpretive focus on the paper, focusing on the sediment data in the context not so much of ice sheet history but rather in the context of extant bedrock and boulder data (from which it differs) and, as this reviewer suggests, an example of interglacial modification (or not) of nuclide content in both deglacial and modern river sediment – both of which, when the ice returns, will be moved down ice and eventually into the marine realm. Thus the paper not only provides fundamental data about sediment isotopic

composition but aids in the interpretation of marine sediment records although this is now a minor part of the revised ms and as such warrants only a sub-section of the discussion.

2) I don't see how the Al/Be data in this study can be used to support claims that ice persisted throughout interglacials (and I sort of disagree that the community thinks that such persistence would be unexpected). It seems to be a conclusion from the LeBlanc work, but the data in this study standing alone, I don't see the evidence. In fact, when considering the Al/Be ratios at the proximal deglacial sites (5.2±0.8) against Figure 3 in LeBlanc et al (2023), it seems more plausible for every interglacial to have experienced many thousands of years of exposure. If making this point and further supporting LeBlanc is important to the team, I suggest some exposure/burial history modeling to see if these crn data uniquely support persistent interglacial ice cover. If not, it just seems like rather unsupported bolstering of a previous finding by this research group. Either way, I think the study stands just fine without pushing this conclusion.

Similar to the response above, in rewriting the ms and refocusing the discussion, we have stressed that the sediment data do not allow a unique solution.

Line by line comments

13, don't really need the word 'buried' here

removed

18, 'surface erosion' of bedrock? Or of sediments, too?

Clarified during complete rewriting of the abstract.

22, 'average several thousand years'. The average and standard deviation of other measurements are reported numerically; I suggest doing the same with this one – that is, report the average and standard deviation of inheritance in terms of years.

In revision, we have completely changed our approach to interpreting these data.

22, could remove words from 'after correction..' I find this multiple paragraph abstract long and wordy. I think it could be made much more succinct. Readers can find the details in the manuscript body.

We have rewritten the abstract from scratch, included justification for the study as well as implications and tried to remove extraneous details.

31, could just say 'reported by others.' 'reported here' is a little redundant in this sentence since the first part states that there is inheritance in this study's dataset.

reworded

44, I don't think anyone honestly thinks land-based ice domes like these 'collapsed', suggest other wording. Also, find other wording for the 'northern Canadian ice domes'. What are these, like the Barnes and Agassiz ice caps?

Phrase removed in revision.

48, 'Because' sentence.. suggest rewording. 'far north' but then study Rice study, which is arguably the southernmost of the three LIS domes. Be specific.

Phrase removed in revision.

58, to one of my main points, this paragraph closes to say results could be important for offshore work. I think this paper would be stronger this point was more directly as a major impetus of the work. I guess after reading the entire manuscript, I felt that the work was done as a companion for offshore studies, but I did not get that sense while reading it.

We have worked in revision to broaden the scope of the interpretation and better set the research in the ms into the broader context of our work on ice sheets over the last 20 years, including the LeBlanc paper but not limited to it. The impetus for the research in the ms was in part but not exclusively the LeBlanc paper so we are not comfortable making that paper the "impetus" for our work. Indeed the revised ms focuses on inheritance of nuclides from prior periods of exposure.

80, section 2.1, this is subjective not objective comment, but covering the basics of Al/Be methodology seems potentially below the readership. Critical to include in student writings, but not necessary in disciplinary manuscript.

We have scaled back this discussion while at the same time adding more references to prior work. For readers not familiar with the application of dual isotope ratios, or the use of cosmogenic nuclides as tracers of erosion, we have retained core parts of this section.

115, reword, not sure 'for inception to deglaciation, the LIS was…' makes grammatical sense.

Reworded.

122, 137, the content in these two paragraphs are the same, but are separated by a paragraph on a different topic, suggest combining and streamlining section 2.2

We have reorganized section 2.2 adding additional citations.

126, suggest write 'not as well constrained by comparison'

Reworded.

128, I'm not sure I would characterize this as "it is debated." The Zhou and McManus reference is missing; I didn't look up what they wrote.

All referencing has been redone using reference manager. Wording changed.

131, What is meant by "these"? Prior sentence cites LeBlanc but the data aren't mentioned in order to justify use of "these"

Reworded.

135, 'commonly held assumption' I disagree with. The Holocene technically isn't fully ice free (eg, Barnes ice cap contains Pleistocene ice) anyway. As a rule, I don't think it's entirely responsible to push a controversy to elevate importance of one's work (I wince a little reading this after I wrote it, but I do think it can be said here, sorry if harsh). And as LeBlanc points out, one look at those mild middle Pleistocene interglacials, why would one think that every interglacial was absolutely ice free in the first place? I, for one, think the persistence of small ice caps/domes in the three LIS centers, like today on Baffin but even a larger Barnes, during may Pleistocene interglacials is actually highly likely.

Section revised.

166, who's 'they'?

Reworded.

197, after reading this section, some relevant research could be included. I would think that the abundant literature on bedrock crn inventories from ice sheet settings showing inheritance is worth citing. As are locations within ice sheet boundaries where pre-Holocene sediment has been preserved (Miller et al last interglacial sediments in lakes on Baffin and Greenland, and maybe look into old sediments preserved in the Manicouagan Reservoir, I seem to recall a presentation showing old seds right there in the heart of Labrador).

Rewrote this section and added numerous other references to inheritance of cosmogenic nuclides in LIS sediment, bedrock and boulders as well as the preservation of sediment and regolith from earlier interglacials and preglacial periods. Added details on all other cosmogenic studies in Labrador including their suggestion of inheritance and cold-based ice. Compiled all extant data.

234, love this sentence, to repeat the above, would add emphasis to this earlier in the paper

Noted.

Figure 1, Panel A says "text" in N Atlantic. Box showing area of B is incorrect. Overall could be improved significantly, make larger for starters, no need to be so small – really important figure

here. The dashed ice sheet extent lines are pretty difficult to decipher (why dashed lines?). Panel B is crucial for this work, should be improved, put on shaded DEM perhaps, maybe even color, would be nice to see elevation of the transects and the overall terrain. Dome center does not coincide with 'last ice location' - why not? Wouldn't the retreat center (location of last ice) make more sense if interested in exposure/burial durations?

Figure has been redrafted.

322, would be nice to see in this table - the % correction for Holocene exposure. So many samples are from generally great depths, I would not expect there to be much correction, still, would be nice to show to readers.

We added this.

361, 'with distance from center' note comment above, I question the choice of 'center.' Furthermore, distance from center is a little awkward, it only makes partial sense. Plotting distance along flowline would be better. Or by retreat age would be best. Figure 3c shows two deglacial samples within the 9 cal ka ice limit that have ratios >8. In the south transect, samples in similar positions (just within 9 ka limit) have ratios of 5.5 and 3.5. Seems like data plotted along paleo-flowline distances would make most sense?

We chose to plot samples according to their distance from the estimated center of the ice dome (estimated based on sources such as Couette et al., 2023; Dalton et al., 2020) because of the general radial retreat from the coastal margin to the dome center. We wanted to spatially analyze 26Al/10Be to see if it fit with the overall assumption of the center of the dome being the most persistent ice/last to deglaciate.

Figure 5, could be two side by side plots, one with modern and one with deglacial. Would be cleaner, for example, to symbols don't look different in grayscale (how I printed it out). Also suggest for all plots, sticking with blue/red/purple color scheme in map figures, why not carry that color scheme over to the data plots? Can you add the bedrock sample?

We have revised this figure

403, I find it awkward to take an 'all sample average' for samples that are at different distances and with obviously different exposure/burial histories. It only occurred to me later that you would even bother taking a total average because you could consider this an integrated value for what might make it to the ocean. This is one example of how stronger emphasis on the offshore work (and included as a justification in the abstract, if not the title) might make more of your thinking here in the thick of data reporting make better sense. To me anyway. In the abstract, upon first reading, I was like, 'why in the world would you average this value?'

We have completely reworked this section.

404, neither 6.1±1.2 nor 6.6±0.5 are statistically different than 7.3±0.3.

We have acknowledged this in the rewritten section

405, 'consistent with burial after initial exposure' It could also be that the sediments themselves were emplaced at time zero with a burial signal. Isn't that the nature of sediment work, you don't know what's post deposition vs. pre deposition? Or?

We have rewritten this section.

Figure 6, love seeing the LeBlanc panel here, again emphasizes the importance of this work for the offshore stuff. Panel C I don't see any justification for the splitting of these samples into two groups. Why not three groups, or five groups? What is the threshold that allows distinction of "proximal" v "distal," and again consider distance as was measured versus flowline distance.

We have retained this two fold split for statistical testing but the slope of the line (distance v ratio) shows the same effect.

465, 'cosmogenic dating' seems lazy phrasing

Entire section rewritten,

469, Briner and Swanson 1998 was neither on the LIS or Fennoscandia/Greenland.

Wording changed.

472, There are probably >1000 of crn data published from Baffin, some papers included much data on sediments (boulders, cobbles, pebbles; some of mine, reach out if you have questions), why point to one cobble result from the Davis study? Throughout this paper, there is a bias toward citing authors' own work. To some degree this is difficult to avoid, certainly I'm guilty too at times.

This has been corrected in revision

483, this paragraph and previous paragraph, difficult to summarize all this work in 1-2 paragraphs. That said, the tactic taken, to cite individual studies and individual samples, seems destined to be highly exclusive. Is there a way to discuss this more broadly and lean more on synthesis papers? It's kinda like the main author knew a few papers really well, so leaned toward citing those again and again instead of a more comprehensive view of the literature. Of course this is easy for me to write, and much more difficult to implement, but I'll throw it out there.

We have broadened the scope of citations in revision, adding dozens to the ms.

507, not really ‘remnants’ per se. If ice shrinks to become smaller but in balance with a new climate state, does it make it a ‘remnant’?

Reworded

507, ‘must’ seems strong. Language in the LeBlanc paper uses no such strong language, careful to rewrite history.

Reworded

515, replace ‘taken from outcrops’ with ‘bedrock’ (could add “eg” in front of Marsella ref, again back to comment about citing authors’ own work a little heavily).

Reworded

526, I do not believe your data allow you to make these conclusions. See earlier comment at top of review. Concentrations and ratios of ~5 can be easily explained with burial during glacials, even figure 3 in LeBlanc shows this. If there is anything close to a flaw in this great paper, it is here in my opinion. Unless you perform some modeling that says the data can and are uniquely explained by ice cover persisting throughout during interglacials could you use language used in this paragraph.

Rethought and reworded to focus on observations and the variety of means to achieve lowered ratios.

561, to repeat, I do not see this statement being supported by data, and I fear this work will be cited in the future to support such a notion, but careful reading reveals no such support.

Agree, see comment above.

REPLY TO REVIEWER 2

Review of Cavnar et al., "In situ Cosmogenic ^{10}Be and ^{26}Al in Deglacial Sediment…."

Responses to reviewer comments are in red below.

This manuscript by Cavnar et al. provides an interesting and novel framework for applying cosmogenic nuclide exposure vs. burial dating to understand the behavior of the former Quebec-Labrador Ice Dome of the Laurentide Ice Sheet in eastern Canada. The authors measured in situ cosmogenic 10Be and 26Al concentrations in both modern river sediments and deglacial landforms across a ~1000 km transect and found 26Al/10Be ratios indicative of limited erosion and nuclide inheritance, which led them to suggest that the Quebec-Labrador Ice Dome could have persisted through some interglacials. Additionally, authors conclude that cosmogenic nuclide concentrations present in modern sediments reflect that sediments are sourced primarily from incision of glacial deposits and not from bedrock erosion. Overall, I consider this manuscript to be written clearly, with data supportive of authors' interpretations, with potential implications for future applications of this field and analytical framework in similar settings. I do have some comments for the authors to consider for the publication of this manuscript:

Thank you for taking the time to review our work.

Main comment:

I encourage the authors to adopt a rather speculative approach regarding their main conclusions, especially when indicating the persistence of the Laurentide Ice Sheet during interglacials. The authors could potentially lean more towards explaining how to partition the different agents responsible for the obtained 26Al/10Be ratios: exposure duration, minimal erosion, and nuclide decay, which could be further elaborated in section 6.3. While the different ratios along the transect provide evidence for minimal erosion and burial near the center of the ice dome, offshore data and modeling from LeBlanc et al. (2023) ties the story together. The authors could potentially model different burial scenarios with the new terrestrial samples obtained in this work and the work published by LeBlanc et al. (2023) to weigh in on exposure duration. Otherwise, I suggest revising parts of the abstract and conclusions to be more speculative about the results.

In response to all three reviews we have significantly recast the introduction and discussion of the ms doing exactly what the reviewer has outlined here. Our interpretations are now appropriately speculative.

Line by line minor comments:

L45: Add citation on the collapse of northern Canadian ice domes.

We have removed this in revision.

L49: I wonder if inheritance here refers to inheritance of cosmogenic nuclides, as one would naturally assume, or the word reflects preservation of bedrock features in the landscape. Consider clarifying.

We have removed this in revision.

L67: Consider reorganizing the Background section to improve its organization and clarity. As of right now, the section opens with an introductory motivation paragraph. Then, dives into cosmogenic nuclides application, followed by LIS deglacial history. After those sections, the authors go back to explain the use of cosmogenic nuclides as tracers of sediment sources, followed by a subsection on ice sheet erosivity. I suggest either combining some of these sections to make it more coherent or set them in the way the narrative flow is preserved. For example: start with cosmogenic nuclides and then dive into the deglaciation of the LIS to provide a smooth transition to section 3. Field site.

We have revised the background section to remove repetition and respond to all three reviewers' comments.

L77: The last few words of this paragraph are quite similar to the first sentence of the first paragraph of this section (2. Background) and appears repeated. Consider unifying these sentences for improving the reading flow.

We have revised wording.

L123: Consider adding a citation at the end of this first sentence.

We have added a citation.

L149: Since this last paragraph argues about changes of the LIS at finer time scales, I would suggest including Wickert et al. (2023) GRL citation to the list to further support the sentence about LIS meltwater discharge and subsequent feedbacks.

Citation added.

L197: Consider adding Barth et al. (2019) PP to the references here.

Citation added.

L204: Please consider rewording.

Rewritten.

L209: Don't see these rivers on the maps. Consider adding them to the figures or provide more spatial/geographical context.

All place names in paper now added to figures.

L216: I don't see how this last paragraph is relevant to this study. The authors don't come back to the vegetation or precipitation later in the manuscript. I would suggest that authors consider removing this last paragraph.

We believe that it is important for readers not familiar with the area to understand the geographic context especially in terms of post glacial sediment movement and sourcing and so have not removed this paragraph.

L228: Consider adding an approximate direction of the transect.

Done.

L230: Remove 'as well as contemporary river sediment'. Otherwise, it's redundant with the sentence that comes immediately after.

Paragraph rewritten to remove redundancy.

L234: I would move this last sentence to the end of the first sentence of this section to strengthen the application of this method and integration between terrestrial and offshore sediments published by LeBlanc et al. (2023).

Done.

L253: Figure 1: I would encourage the authors to improve this figure. To begin with, there are several areas of the maps that are cropped, making names of cities, countries, and sites hard to read. Secondly, the font size and colors for some sites are unclear. For instance, lakes are depicted as gray polygons, yet labels are also gray, impeding a good readability. Additionally, there is a 'Text' label next to the label of the Labrador Sea that is erroneous, so please remove it. Lastly, I would suggest adding scale and north arrow to maps.

We have completely redrafted this figure correcting errors pointed out by reviewers.

L255: Figure 1 caption: The Quebec-Labrador Ice Dome is noted in the figure as Q-L Dome, but not explained on figure. Simply, I suggest adding (Q-L) here in the caption.

We will add this in the caption.

Line 260: Figure 2: Would adding the nuclide concentrations and ratios be more informative here in each photo?

We don't believe that this would be meaningful.

Line 260: Figure 2 caption: Include river name corresponding to (MC-03).

We added this.

Line 303: Maybe 'last accessed' instead of 'constants'?

We performed the Holocene exposure correction in January of 2024. However, the constants used by the CRONUS calculator were reported as 2020-08-26. We included the constants date so that our work could be reproducible.

L415: Figure 6 caption: replace ‘stream’ with ‘river’ to be consistent with the same term used across the text. Also, it would be worth explaining what was the rationale behind separating some samples into distal and proximal and how this was done regarding the sampled transect and the likely transient evolution of an ice dome during interglacial(s).

We now use river.

L431: Which subglacial processes were modeled? Also replace ‘modeling’ with ‘modeled’.

This is not modelling that we performed but that of Melanson et al (2013) as we cited in the text.

L436: It is quite challenging to find these sites on the figures provided if you are not necessarily familiar with the field site. I would suggest either providing general location references for these sites or add them to the maps on Figs 1 and 2.

Sites mentioned in the text are now included on maps.

L480: Missing Staiger et al. (2005) in the reference list.

Fixed.

L515: ‘taken from outcrops’ here is confusing. Either delete or consider rewording to improve clarity.

We will change this to ‘bedrock’.

L531: Please consider briefly explaining how you arrived at this conclusion.

Entire section rewritten to improve logic.

L538: Please consider providing more details about the mixing model and/or how the results were computed. I found that this section could use some supporting information (figures or table in the supplementary material, for example).

We added more details about the sediment mixing model.

L544: Add citation of average deglacial age for field area.

We have added details about timing of deglaciation throughout.

L561: I would suggest the authors to be more speculative about this statement. See main comments above.

This section of the ms has been rewritten.

Supplementary material: I found it quite helpful to see field photos. I would, however, suggest revising some of the notes/descriptions since some notes are not relevant. Additionally, elevation units need to be changed so they are reported in the same system for consistency purposes.

We have edited elevation units for consistency.

References:

-Missing some references in the text that are listed in the reference list:

Pico et al. (2018), Abe-Ouchi et al. (2013), Dyke (2004), Goehring et al. (2010), ~~Gregoire et al. (2018),~~ ~~Rasmussen et al. (2006),~~ Rice et al. (2019).

We have redone referencing from scratch using reference management software so reference list should now faithfully reflect ms citations.

REPLY TO REVIEWER(S) 3: Rice, Ross, Paulen, & Kelley

Review of Cavnar et al., "In situ Cosmogenic ^{10}Be and ^{26}Al in Deglacial Sediment…."

Responses to reviewer comments are in italics below.

The following is the result of conversations between: Jessey Rice, Martin Ross, Roger Paulen, and Sam Kelley.

Cavnar et al. provide a novel approach to investigate the subglacial conditions throughout glaciation of the Quebec-Labrador sector of the Laurentide Ice Sheet via an investigation of cosmogenic nuclides in deglacial sediments. The manuscript is well-written, interesting to read, uses novel approaches, and provides some useful insights, however, is missing some key references to past subglacial dynamic work and misinterprets some important previous research. Additionally, the conclusions of the manuscript are a bit too broad and slightly overreach given the data provided in the manuscript. The work does highlight some interesting findings that suggest further work is needed in this glaciologically important region.

In revision, we have refocused and rewritten much of the abstract, introduction and discussion. We have cited additional literature, provided additional background and worked to accurately cite prior, relevant literature. We have dialed back the conclusions and made sure they are consistent with the data we present.

We think the manuscript needs some improvements; some specific recommendations are suggested below:

Thank you for talking the time to provide this level of detail.

Line 47-49: In Rice et al. (2019, 2020), we did not find evidence of relict preglacial landscapes preserved due to sustained cold-based conditions, and we also did not find clear evidence that the oldest glacial bed fragments are from an older glaciation. We interpreted the older flowset (i.e., our Flow 1) as having formed during the last glaciation but likely before LGM. In this work, through multiple proxies, we indicated there was no evidence of sustained cold-based conditions, but for this study in particular, from the limited ^{10}Be samples we collected, we had no evidence to suggest high degree of inheritance from long exposures from previous interglaciations (when compared to other regions of ^{10}Be with sustained cold-based conditions (Staiger et al., 2006; Refsnider and Miller, 2010; Corbett et al., 2016; Dubé-Loubert et al., 2021)). If you have any additional questions regarding this work, I would be happy to chat with you about it (jessey.rice@nrcan-rncan.gc.ca).

The abstract has been totally reworked. Our compilation of all data from the region now quantifies inheritance.

Additionally, given the study area's location, it would not be considered far northern Canada.

This phrase was removed in revision

Line 68-78: While we agree there is still little known about the pre-LGM conditions, there is work that suggests that most areas under the LIS in northern Quebec and Labrador were erosive at different times, possibly due to the migration of the Q-L. It is possible that cold-based conditions existed under the Q-L sector for some time, the divides were also moving across the vast region that it covered resulting in only transient cold-based conditions, with warm-based conditions also migrating across the sector as ice sheet dynamics evolved throughout glaciation.

We have removed this from the introduction and decreased mention of models to focus on our data.

Although you cite the compilation works for this area (Kleman et al., 2009; Dalton et al., 2020), there are specific references used in those manuscripts that describe the cross-cutting relationship of landforms and striations that suggests continued warm-based erosive/deformational conditions at the base of the ice sheet (Veillette et al. 1999; Clark et al., 2000; Rice et al., 2019 (this one is listed in the references, but not cited in the text)).

With our refocusing of the paper toward data this has become less relevant. We have removed mention of thermal regime from the introduction and moved it to the discussion where we cite the papers mentioned by the reviewer.

While we also agree that some numerical models show low probabilities of warm-based conditions for this area, it is important to acknowledge their own uncertainty. Tarasov and Peltier (2007) highlighted the importance of using appropriate bed thermal conductivity and thus recognized the limitations of using a single value for an entire ice sheet model. Pickler et al. (2016- Climate of the Past) is one of a series of studies which analyzed deep borehole temperature profiles to reconstruct basal temperatures of the Laurentide Ice Sheet during the last glaciation at different sites. One of their study sites is close to your sample transect (Sept-Iles). In this study, they obtained temperatures near the pressure melting point at LGM, which was the coldest at all the sites analyzed. Interestingly, reconstructed pre-LGM temperatures were warmer for all the study sites including Sept-Iles. All this to say, determining bed conditions under all the LIS throughout the last glaciation is a difficult problem, and considerable uncertainty persists, but there is limited supporting evidence for widespread spatiotemporally stable and sustained cold-based conditions for most of the region of northern Quebec and Labrador. The evidence is patchy and uncertain at best and suggests regional variations and transient conditions. Most previous researchers interpret the glacial landscape of that large Q-L region as having formed during the last glaciation, suggesting broad warm-based conditions at times, which should be cited to present a more balanced representation of existing literature and interpretations. Similar evidence of these dynamic conditions has also been documented for the Keewatin sector in northern Manitoba and mainland Nunavut.

With recasting the paper toward data and away from speculation, this comment is no longer relevant.

Line 75-77: The relative timing of cross-cutting striations can be ascertained, but the absolute age of each flow cannot (i.e., Clark et al. 2000), although we think it can be constrained. When erosional proxies are placed in the context of relative ice-flow chronologies, the erosive extent of ice flows can be evaluated (see Rice et al., 2019 and 2020). From this work, we indicated that

Flow 1 was erosive and able to transport sediment significant distances (>100 km) and correlated this to Veillette et al. (1999) and Klassen and Thompson (1993)'s oldest flow and sourced from the St. Lawrence Highlands. This flow phase must have occurred pre-LGM as the flow was to the NE, and not radiating from an ice center, which would be expected at or near LGM.

This material has been removed from the ms.

Line 125-127: Arguably, the retreat of the LIS in northeastern Quebec is one of the most poorly constrained across the entirety of the LIS. It may be well constrained in southeastern Quebec/southwestern Labrador, but northeastern Quebec/western Labrador is poorly constrained north of 54° (basically Smallwood Reservoir over to Lake Melville). This should be rephrased.

We have rewritten this section.

Line 142-143: We would argue that most of the evidence now suggests Hudson Bay was not ice-free during MIS 3. Hodder et al. (2023) provided evidence that there was not an ice-free Hudson Bay during MIS 3. See also Hodder et al. (2024) and Gauthier et al. (2024).

Thank you for recommending these sources. We have incorporated some into this paragraph to more accurately reflect what recent evidence suggests about MIS 3.

Line 175-176: Rice et al. (2020) did find this as well, but further north and closer to a major ice divide of the Q-L sector, although found it was more a result of ice divide formation and migration than proximity to the centre of the dome. We found that inheritance across the study area was low, with only a few areas (that experienced ice-divide migration across them) having evidence of sluggish ice conditions and inheritance. A vast majority of the study area indicated little to no inheritance, suggesting significant erosion.

Our focus in revision on extant data has made this specific comment irrelevant and contradicts the commenter's assertion of little to no inheritance.

Line 200: and study site information: Given that the article focuses on glacial erosion, we think at least a brief summary of the glacial history of the study area is relevant and required. The soil thickness has little to do with the study's focus, the till and deglacial sediments are critical and their formation should be discussed.

We have rewritten and expanded the background section adding numerous additional references.

Line 233-234: Why was only a single bedrock sample collected over this 1000 km sector? The reasoning for the single bedrock sample or the insights from this sample are not clear throughout the entire manuscript, it would help the reader understand the methodology better if this was explained.

Our goal was to collect many more bedrock samples. However, bedrock samples were collected according to proximity to our route on the Trans-Labrador Highway, and we found only two within short distance from the road. One of them produced insufficient quartz and could not be used for ^{10}Be and ^{26}Al analysis. We also intended for this project to have two field seasons, but

the second season (intended for summer of 2023) was canceled due to the Canadian wildfires and extensive road closures, leaving us with n=1 bedrock sample.

Line 239- 240: We don't think this is an accurate representation of what Ullman et al. (2016) indicated. We are assuming this was pulled from their figure showing the extent of ice at ~ 7.6 ka? Do you mean it was the centre of the Q-L sector at that time?

Thank you for bringing attention to this. Yes, we pulled this from Ullman et al.'s figure.

253: FIGURE 1 The centre of the Q-L dome does not align with any of the portrayed isochrons, the timing of Ullman (2016)'s centre should at least be represented, or an explanation of how the centre was determined should be given. Also, a DEM would really help this figure and there should be latitude and longitude on the figures and a scale.

Have fully revised this figure.

339: FIGURE 3 There should be errors included with the measurements, it might help to have different shapes to convey the sample type (modern vs deglacial) with different colour fonts to make it easier to separate the two types of samples. As it stands, it is a bit difficult to ascertain which values correspond to what sample at the current figure's resolution. This would help the reader to evaluate the data more easily.

We have redrafted this figure. Errors are found in the table. Including them on the figure makes it too crowded.

405: How are you determining initial exposure? Could the sediment have been deposited under a thick till sheet and then eroded and incorporated into a deglacial sediments during a subsequent glacial event? This is very difficult to determine, but it should be discussed as a potential.

We do not address this because there are not data on initial exposure. We are unsure how to respond to this comment.

409-411: Could the source of those sediments not have been from anywhere along the eastern coast of Labrador? Why would it need to come from Hudson Bay? There is lots of evidence of sustained cold-based conditions in the Torngat Mountains (Staiger et al., 2006) and as far inland as the George River, QC (Dubé-Loubert, 2021).

This is not germane to the paper as revised.

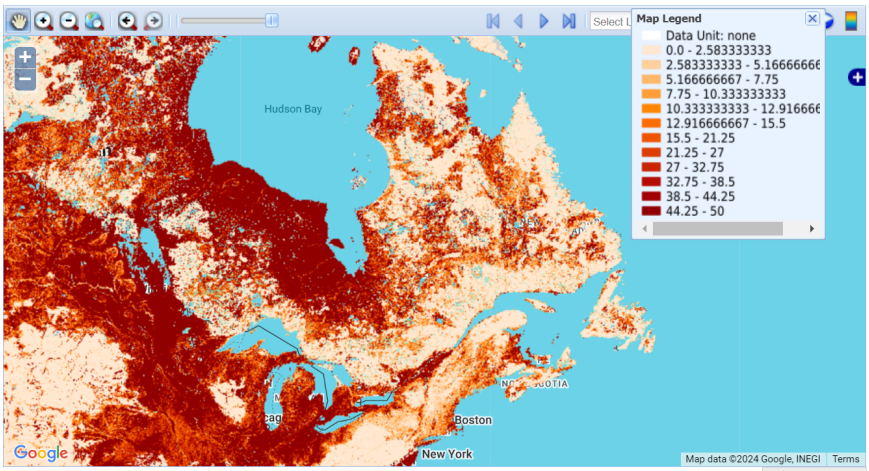
FIGURE 6. There is no y-axis unit, difficult to tell the degree of probability for each plot, could you please add?

The plots show 'relative probability', so numbers on the y-axis would not be informative.421-422: This is a very bold far-sweeping statement for the data provided. We would suggest saying that there was inheritance from a period of prior exposure, but extrapolating beyond that seems a bit too much for the data presented.

Agree, we have rewritten ms to reflect this.

441-442: If you look at the surficial map of Canada (the best resolution for this statement) most of the area is covered in glacial sediments. Therefore, stating that sediment coverage was patchy and thin is not entirely accurate for the study area.

Below shows a screenshot of the Pelletier et al. (2016) map we cite from http://dx.doi.org/10.3334/ORNLDAAC/1304. We think "patchy and thin" is a reasonable way to describe the Q-L region in comparison to much of the rest of the areas formerly covered by the LIS. We now describe the soils as "commonly thin (<2.5 m)".



456-457: Given all of the variables and the difficulty estimating them (i.e., interglacial exposure duration) can you be sure the concentrations purely reflect erosion rates?

No they do not, they represent an impossible to decipher mix of exposure, erosion and decay and that is the basis of our revised discussion.

460- 463: Minnesota is quite far from the Keewatin ice dome and is better described as the southern LIS margin.

We will clarify this, it is in fact both.

475-482: This review of individual papers allows for different methods of identifying an outlier. Why not use the same approach as was used in your determination of Holocene exposure history? Recalculate all the cosmogenic exposure ages from the region (including those from a wider range of papers and use a set of isochrons to determine which ages contain inheritance).

We have done this.

511-516: There are certainly many places the quartz could have been sourced from, but there is significant ice streaming off the coast of Labrador inputting significant sediment into the North Atlantic, why couldn't these quartz grains have come from somewhere along the Labrador coast?

This is correct but no longer relevant after revision.

544-545: This is minor, but is decadal resolution realistic for estimating deglaciation across such a vast area, or even a given area? (9.32 ka). It is understandable that this was derived from published isochron data, but removing the last digit is probably a better representation of the published data.

Rounding changed to 100s of years.

561-562: We do not think this conclusion is valid based on the evidence presented, evidence of polythermal conditions, yes, but using this as evidence of continued glaciation over such a broad region is overreaching.

We have completley reworked the discussion so that this comment is no longer relevant.