RESPONSE TO REVIEWS, Cavnar et al.

Briner

It is nice to see this paper again, there have been many improvements and this will be a great contribution. While this work joins a huge body of work that has well established the fickle nature of ice sheet erosion and how this plays out in complex spatial patterns of clean CRN ages versus some with inheritance, it is one of relatively fewer studies focused on sediments. I would therefore ask the authors at every opportunity to emphasize the added knowledge to the field that CRN analysis of sediments uniquely provides.

Thank you Jason. In revision we have tried to make more clear the power of amalgamated sediment measurements to understanding nuclide inheritance on a landscape scale.

A few larger comments then some line-by-line stuff

What is the age resolution of using Dalton 2020 isochrons to assign ages to landscapes where 10Be samples were collected? I read the word "interpolated" (line 385), maybe that can be unpacked a bit? What is the estimated uncertainty with this approach? To the nearest 500 years?

Dalton 2020 gives isochrons in our study area for 6.3, 6.8, 7.3, 7.9, 8.1, 8.5, 8.7, 8.8, 9, 9.6, 10.3, 10.9, 11, 11.5, 11.8, 12.1, 12.8, 13.5, 14.2, and 14.9 cal ka. So, the resolution of the isochrons ranges from 100 to 700 years and averages 500 years. For samples located between isochrons, we estimated their deglaciation ages by linearly interpolating between the isochrons. We have added this statement to the ms text for clarity

Thanks in advance for bearing with peanut brain here, was there a production rate used in this paper? I think when just concentrations are reported, no PR choice is needed to be made. But at some point the post-glacial (time since Dalton-deglaciation happens at each sample location) exposure needs to be subtracted – does that require a production rate? If so, what was used? Do the production rates in Table 2 require a SLHL production rate as a starting point? Does a production rate need to be employed to convert inherited concentrations to age? If so, what production rate is used? Maybe it's in there and I missed it. Also, if you assume no inheritance for some samples (can you do this, or maybe not), can you use your dataset to calibrate a new PR? Sorry for the mind dump, if there is any way to make this aspect of the work more clear, please do so.

We estimated the amount of inheritance in years by subtracting the Dalton deglacial age from the CRONUS-calculated 10Be age (using LSDn). This inheritance value in years was then converted to concentration by multiplying by the local production rate obtained from CRONUS. One could invert the samples for which, based wholly on 14C age models, an

effective production rate over time and space for eastern Canada but that is not the purpose of the paper and would by its nature integrate over time and space. We'll leave that as an exercise for others based on our data tables

A key argument that this paper strives to make, similar to what was in the recent LeBlanc paper, is that these data suggest ice domes like the QLD survived some interglacials. I'd like to drill into this a bit. I don't intend to be overly skeptical, but for argument's sake, one could look at the global d18O stack and say, sure, there are some mild interglacials in the middle Pleistocene, so yeah, there was probably more LIS volume surviving through the interglacials than, say, the Holocene. But are there independent data, like CRN values, be it in IRD in sediments on land, that actually necessitate this? Why do the concentrations and ratios demand that versus the alternative that these integrated samples record a burial and exposure combinations that can be likewise satisfied even with 5-10 kyr of interglacial exposure (especially if the sediment samples were previously buried and not gaining full surface re-exposure during brief ice-free periods). I just want to be sure that, as a community, we're not inventing soundbites or storylines that sound good and *could* be supported by the data, from stories that ARE supported by our datasets. I suspect you might have an answer to this, and if so, it would be great to see it logically spelled out in the text.

This is an important observation. As LeBlanc et al, wrote in their 2023 paper, "Till could shield bedrock from cosmic radiation, but till only partially covers glaciated regions in North America today (Pelletier et al., 2016)." Even if till were present, it too would have been irradiated during ice-free interglacials and then exported when ice readvanced." Indeed burial by till or in outwash deposited could have the same effect – but with the paucity of till, it. seems less likely. We have toned down the writing by removing the word persistent from the ms.

There are other data that support lingering LIS ice (see for example, Lines 226-234 in Roger Creel's preprint at https://eartharxiv.org/repository/view/9043/). But the reality is that there's simply not enough cosmogenic 26/10 ratio data collected on land to tightly constrain LIS ice extent over time. Future work should aim to get 26/10 data from some of the hypothesized ice dome areas to further evaluate this hypothesis.

Line by line comments

70, would be helpful if these kinds of place names appeared in the figures to orient the reader faster, and here in particular it would show the reader where you consider the "center" of your transects

We have made these changes

93, "substantial" – large or many? Better word?

Removed word

95, could add “net post-glacial” between words ‘greatest’ and ‘rebound’ and maybe can remove words ‘of rebound’ later in sentence

Done.

103, paragraph starting here lists rivers and place names not on figures, it would be helpful to orient readers and make this nice work more accessible to add these labels

We have removed much of this paragraph because it was not germane to the manuscript.

155, Goehring et al 2010 was a study that took place in east Greenland, yet it is cited here following reference to ‘southern and western Greenland’

Corrected.

172, could consider adding ‘marine-terminating’ ice so it reads “… also suggest marine-terminating ice may have lingered in eastern Canada through many interglacial periods.”

Corrected

180, typo/grammar, fix

Corrected

185, use ‘farther’ not ‘further’ when distances are the subject

Corrected

191, extra comma after et al

Corrected

196, here I was confused about referencing the 2020 paper and not the 2023 paper but it later became clear why – anyway to preemptively signal to readers why cite an older paper?

Yes, we have added that signaling a bit later as there is no call out to either of Dalton’s papers anywhere near line 196- typo perhaps?

200, what about ‘some’ older-than-expected 10Be ages?

Corrected

201, ‘radiocarbon ages lag the 10Be ages’ is less straight forward than writing which ones are younger or older than the others. Use of ‘lag’ makes one need to stop and think what you are trying to convey. I also immediately wondered about production rate or scaling choice as another possibility for this offset

We have added caveats in this part of the ms.

206, I find this section long and a little off topic – well, not off topic, but is going into this level of background necessary? It sort of seems to be making a point that has been made for 20 years, and it attempts to span a huge body of work that will inevitably miss references and studies. This is a stylistic comment, but I'd prefer to read a short paragraph that synthesizes the concept while citing some studies as examples.

Given that much of this material was added in response to request of other reviews we have left much of it and tried to streamline where possible. Cutting to a short paragraph doesn't seem practical and wouldn't acknowledge both the general and regional literature.

217, there is a lot of literature on Baffin island for pre-lgm lake sediments, Bierman et al 2001 is not what comes to mind. Could add Miller et al (https://doi.org/10.1002/jqs.3433)

Added reference as suggested.

222, I would argue maybe that Halsted estimated that those boulders 'could' contain inheritance, not that they necessarily contain inheritance?

Modified text for subtlety.

369 and 385, I was a little confused here in these paragraphs. At 369 there is language describing calculation of CRNs attributable to Holocene exposure, and at 385 it seems to revisit the concept but maybe makes it more clear what was done (get independent ages from Dalton iosochrons). I say "more clear" because at line 369, the method of using Dalton ages does not appear until the end of the paragraph. I'm not positive, but I feel that these series of paragraphs could use some further streamlining.

We have reworked and reorganized these paragraphs for clarity.

387 'convovle'? Could you use 'combine' (convolve is a new word for me, not that my noggin is vocabulary vault)

We have reworded.

406, you write that one set of value is 'higher' than the other but statistically they overlap (presumably at 1 sigma) so is there really any difference? I don't mean to push too hard on this and often struggle myself in these instances, because the means are different. But thanks for considering this.

We have clarified that these values are not statistically separable.

423, I was confused what "to the left of the profile" meant when I read the paper. Only now when typing this review do I realize what I think you mean, which is that the sample came from a different place on the rock face than the other samples in the depth profile? Maybe

you can explain that better, and/or label where each samples if from in that figure photograph

We have modified the text for clarity.

427, figure 4 caption, mention the sample IDs in this caption. In the text, the depth profile samples are referred to by their sample ID, but not in the figure caption or on the figure, so it took me longer than it needed to in order to figure it all out

Good suggestion that we have implemented.

455, the 7.3 ratio is variably referred to as the "arctic" ratio (here) and "high latitude" and "Greenland" at other points in this paper. Might help to characterize the location of where that ratio was derived (or applies to) by using the same set of words.

Sticking with this comment, your samples are from 47°-53° N. Does the 7.3 ratio really apply here? What is thought to be the production ratio at low latitudes these days? Mid latitudes? And, if the ratio is 6.7 (or whatever) versus 7.3, how would your results change? Inquiring minds want to know this.

We have added the mid-latitude ratio. It is not clear from empirical data what the production rate is at the latitudes we sampled but it must be between these two estimates.

462, farther

Corrected

501, can you show figure 8 before figure 7? Upon first mention of your nice compilation it would be good to see where these samples are from – either that or add dots to an earlier map figure

We have swapped figure order.

506, well, the "average boulder inheritance" OF THOSE WITH INHERITANCE is a missing phrase, right?

No, this is the average inheritance of all boulders. We have updated the sentence to say "The average inheritance in all boulders, counting those with <3000 atoms g-1 as zero, is equal to ~1800 years of surface exposure…"

508, "…for sampled boulders…". All sampled boulders or some?

We have clarified this statement. It now reads "In contrast, the median inheritance for all sampled boulders using their raw values (i.e., not treating those with <3000 atoms g-1 as zero) is negative, -4000 atoms g-1…"

509, it occurs to me that this discrepancy could be due to not adjusting for isostatic uplift? Can you add this to the list? I'm not sure what production rate is used when converting concentrations to years, but most production rates are in fact from sites that experienced some rebound, so one could conclude (as I and others have) that it's a wash, since both the calibration samples and a study's samples experienced uplift. But here in this interior field site, there was very thick ice, just little coastline for constraining rebound. But, for example, the Arctic PR sites had 60 m of rebound and what might it be in the core of your ice dome, 100s of meters? Just a thought.

This is an excellent point. We estimated three paragraphs further down that accounting for uplift could increase inherited concentrations by 3400 +/- 2400 atoms g-1, which overlaps with the magnitude of this cosmogenic-radiocarbon age discrepancy. We have added isostatic uplift to our list of possible explanations.

522, this paragraph.. I wonder if it is important to keep in mind sampling bias. It's not like these samples of bedrock and boulders were sampled randomly across this space. Something I often forget when using samples collected to characterize a landscape.

We recognize that there may be a sampling bias in the location of bedrock and boulder samples – at least in part dictated by limited road access. Sediment samples, because they integrate across much larger areas, are likely less subject to such a bias. We have added wording about this to the revised ms.

535, analysis

Corrected

535, can you re-word "… is robust to uncertainty…"

Reworded

537, I find this a little awkward to sketch out these scenarios, it is not clear to me why this "sensitivity analysis" was done, seems kinda random

Given that our estimates of nuclide inheritance are entirely dependent on the Dalton 2020 retreat chronology, we felt it was important to assess their sensitivity to the chronology. Using a +/- 10% shift in the chronology (~1,000 years) is admittedly simple, but it shows our first-order conclusions are likely robust. We have tried to make the rationale more clear and less random at and near this line in the paper.

614, explain further how this dataset requires ice domes to survive full interglacials

We have added text here as described in our response to the reviewers' narrative comments above.

632, farther

We have changed “further” to “farther”.

RICE

In situ Cosmogenic 10Be and 26Al Reveal a Complex Exposure and Erosion History of the Landscape Once Covered by the Quebec-Labrador Ice Dome

Cavnar et al. have provided a compelling study using paired in situ cosmogenic nuclide (10Be and 26Al) measurements to deglacial sediments, modern river sands, and bedrock within the region formerly occupied by the Quebec–Labrador Sector of the LIS. This investigation includes an extensive dataset of previously published nuclide concentrations from 238 bedrock and boulder samples from across the Quebec-Labrador Sector. Their objective is to constrain the erosional efficacy of the LIS in this sector and to evaluate the extent of nuclide inheritance— cosmogenic concentrations acquired during pre-LGM exposure and preserved through subsequent glaciations and its implications for future cosmogenic nuclide age determination studies.

From their results, deglacial sediments (100%), boulders (34%), and bedrock surfaces (57%), all contain inherited nuclides. Additionally, the similarity of nuclide concentrations between modern river sand and deglacial sediments demonstrates that paraglacial fluvial systems recycle glacial deposits with minimal contribution from fresh soil or recently eroded bedrock. A single bedrock depth profile possibly suggests the presence of inherited nuclides.

Major recommendations: Given the challenging nature of this investigation and the vast remote region it encompasses; some assumptions and generalizations were understandably necessary. However, I think at times the findings are overstated given these limitations.

We disagree and believe that we have objectively compiled and reported our own observations and those from the literature. We provide specific details in our responses below.

One of the main generalizations is that the Quebec-Labrador Sector is represented as a single dome with static radial flow throughout the entire glacial cycle and placing its centre near Lab City. I acknowledge that some generalization has to be made for the study, but acknowledging this in the manuscript is required. The Quebec-Labrador sector was a vast ice mass with multiple ice saddles and divides with ice flowing in multiple directions which makes determining the "centre" of it difficult. The best case for the closest centre to this study I think has been made by Dyke and Veillette who place it relatively near the community of Schefferville or further north. The current centre that is used is more a reflection of the centre of ice-margin retreat, which could be argued to control meltwater flow and tie into the glaciofluvial delta samples, but equating that centre with erosional intensity and the ice sheet's ability to remove inheritance is misleading.

We don't believe that this distinction is meaningful. Sediment sourcing and transport must have changed over time as the Laurentide Ice Sheet grew and melted back. The sediment derived cosmogenic data are a spatial average over time of ice sheet erosivity and sediment transport and mixing. We have added such a statement to the revised ms.

This needs to be clearly clarified in the manuscript. Additionally, I believe that some of the samples classified as boulder and bedrock samples should not be used for assessing the Quebec- Labrador sector, as they may have been sourced/eroded from other sectors.

We disagree with removing data from the compilation. By providing detailed data tables of the data we compiled and present in figures, any reader can filter the data as they wish.

With that being said, the overall findings based on the data presented are valid and provide an important caveat that all researchers who collect cosmogenic nuclide deglacial data should be cognisant of when collecting such samples. I am impressed with the degree to which the manuscript has been improved and the effort that the lead author put forth to facilitate these changes.

I have provided specific comments by line below. Please don't hesitate to reach out if you have additional clarification questions.

Jessey Rice

Specific recommendations:

Line 19: I think you should refer to this as the Quebec-Labrador sector. Dome implies a single centre with radial flow, which vastly oversimplifies the actual flow morphology of the ice sheet. It was composed of multiple saddles and divides, from which ice propagated. Setting aside that these saddles and divides migrated through time (which should be discussed briefly in the manuscript.

We have not made this change because Quebec-Labrador ice dome is used repeatedly in the literature including by this reviewer most notably in his 2020 paper (https://doi.org/10.1002/esp.4957) entitled, A GIS-based multi-proxy analysis of the evolution of subglacial dynamics of the **Quebec–Labrador ice dome**, northeastern Quebec, Canada.

Line 20 -21: Stating deglacial eskers is a bit redundant, just eskers would work. Being consistent with the number of samples would also make it easier to read. i.e., we sampled esker and delta sands (n=10), modern streams (n=11), and bedrock (n=1).

We have reworded the sentence in a way that we hopes make this clear and not redundant.

Line 28: I am not sure 65/192 (34%) would be considered common, this should be rephrased to not oversell the findings.

We disagree and have not made this change.

Line 38: Again, stating many is a bit misleading when you group in the boulders with the bedrock, together they are less than 50%.
We disagree and have not made this change.

Line 44: e.g., not e.g,

Corrected

Line 48: e.g., not e.g,

Corrected

Line 59: I think saying the deglacial history has been established is not quite accurate, we have made incredible gains but there are a lot of questions remaining regarding the deglacial pattern and chronology of the Quebec-Labrador sector.

We have modified this wording.

Line 61: A bit of an awkward sentence, possible revisit.

We have modified this wording by making sentence active voice.

Line 83: White circles, not open circles.

We have fixed this.

Line 84: Should cite the Margold paper from which this data was taken.

We have now used this citation.

Line 100: Figure E. from this image it appears that the bedrock profile was taken from a small bedrock scarp. It is difficult to say for certain from the image, but if so, would this not expose the lowest sample in the profile to quite a bit of cosmic bombardment since its exposure? Or is this a drilled exposure along the highway? Please provide a bit more context, the findings from this profile are significant to the overall conclusions, so more discussion on it is warranted in my opinion.

The bedrock profile corresponds to a scarp that was exposed by a 'recent' road cut. While

we couldn't figure out the exact year of the work, aerial photographs and satellite imagery indicate that it was after 1985. Sampling took place in 2019, the small bedrock scarp – and all samples, except for the surface one – have therefore been exposed to direct cosmic bombardment for a maximum of 34 years. Considering a local ^{10}Be production rate of ~4.41 atoms/g/a, 'modern' inheritance would account for 150 atoms/g since the road cut. It therefore does not change significantly the findings from this bedrock profile, and consequently the overall conclusions of the manuscript. We have added these details to the figure caption.

Line 177: I know it is mentioned later in the manuscript, but I think it is better to state here why you did not use the 2023 reconstruction, this sets the foundation better for later discussions.

We have added a reference to 2023 paper here and mentioned that it included cosmogenic data.

Line 180: Missing the ice sheet (The margin of the then retreated).

We have fixed this.

Line 196: I think it's important to reference the Dyke reconstruction as well here. There were few or no changes in many places of the eastern sector during that update.

We don't agree – Dalton is the most modern and inclusive compilation.

Line 203-204: Analytical uncertainty could also be the reason no? They are commonly in the multiple century scale.

No, that would require all or most samples to have analytical imprecision biased high and that' not how counting statistics work.

Line 266: Nelson et al. is missing a year

We have fixed this.

Table 1: I think glaciofluvial delta is more accurate than glacial delta.

We have fixed this.

Line 299: ICP-OES**:** inductively coupled plasma optical emission spectrometry

We have fixed this.

Line 315: 2850x 10^{15} seems like an odd way to report this, given how all of the other 10Be abundances are reported, should be 2.850 x10^{12}

We have changed this; they are of course exactly the same number

Line 352: My understanding of a Tukey test is that it assumes the data is normally distributed. But you state earlier in the sentence that the data is non-normal in its distribution. I am wondering if this is the best test for this dataset, or the authors have transformed the data prior to testing it, which should be stated. This should be addressed either way.

Yes, it is true that a Tukey HSD test is best used for normally distributed data. The data were evaluated for normality using a Shapiro Will test and were slightly non-normal. Because of this we also reported using a Wilcoxon rank-sum test (which is well suited for non-normal data). However, the distribution of our data appears normal when represented graphically. Sometimes statisticians use a loose definition of "normality" when the data are field-based, and n is relatively small. This is why we performed both tests (which ended up having the same results), for those who prefer to category the data as normal or non-normal. Regardless, conclusions are the same and use the same alpha threshold.

Section 4.5: Were any corrections for shielding incorporated? If no, why not? If there was not shielding it should be stated. Although based on the photos there should be some topographic shielding. I am assuming there will be significant snow cover as well based on 158 mm of snow water equivalent stated in section 2.

The annual mean snow depths for the 1981-2010 period were measured to be 17 cm in Baie Comeau, 31 cm in Wabush and 25 cm at Happy Valley-Goose Bay. The snow-cover corrections would increase the surface exposure ages by 2.5%, 5% and 3.9%, respectively (see Couette et al.'s supplementary data for details on the calculation). However, these should be considered as estimates as snow cover probably varied greatly during the Holocene. Additionally, these corrections would likely be maximum values for boulders as they are often wind-swept or too small to prevent thick snow accumulation. Adding snow-cover corrections should therefore be performed with caution as they could potentially add further uncertainties. We have choosen not to make this correction and added a statement to that effect in the revised manuscript (footnote, Table 2)

Line 371: Why were older constants used for the deglacial sediments? Why were the 2024 constants not used?

We used the older constants because only version 2 of the calculator provides specific muon and neutron production rates needed to do the modeling. We could not use version 3 and thus not use the 2024 constants. There is no reason to believe that they differ.

Line 374: I assume you used a density of 2.65 for the bedrock samples?

Yes, stated in footnote d of table 2

Should state to be clear Line 374: Did the deglacial and modern sands have the same density?

We did not measure the density of the sands and assume a typical value of 1.7 g cm-3 as was mentioned in the reviewed ms. The density of the modern sands is not relevant. This calculation accounts for Holocene subsurface nuclide production in the deglacial sands, and does not apply to the modern sands.

Line 404: Missing a space between 21of

We have fixed this.

Line 414: Given that the two labs had different uncertainties from their blanks, it would be easier to assess the data if the labs were indicated in the table.

The table is already large. This information is provided for those who need it in the supplemental data.

Line 446: Again, saying this is common when it is in less than half of the combined samples (bedrock and boulders) is misleading.

We disagree and feel that the reviewer may be implicitly biased by their prior writings suggesting that inheritance is unimportant.

Line 449: This is logical given the erratics are being transported subglacially and are susceptible to erosion from multiple sides where the bedrock is just from the surface.

OK

Line 459: The reference to the centre of the QLID to being near Lab city is a bit misleading as this was the location where ice retreated toward, but was not the “centre of the dome”, we have tried to move away from the use of the term dome and centres as these ice sheets were not as simple as a single source of ice flow radiating away, but in fact composed of multiple ice divides which migrated throughout glaciation. However, for this evaluation of the data I realize it is unrealistic to have multiple migrating points of an ice centre. Therefore, I think the general middle, or confluence of many of these ice divides was somewhere between Schefferville and Kuujjuaq and should be used instead (see: Andrews & Miller (1979) and Dyke & Prest (1987). Additionally, based on dispersal patterns and ice-flow erosional indicators, the closest divide is further north (Klassen and Thompson, 1993). I do not think this will affect the conclusions you make, but it is a more accurate

placement of the centre. And from Veillette et al. (1999)

We understand the reviewer's concern, and we have updated the phrasing to refer to the samples closer to Labrador City as further toward the interior of the Q-L sector but not as

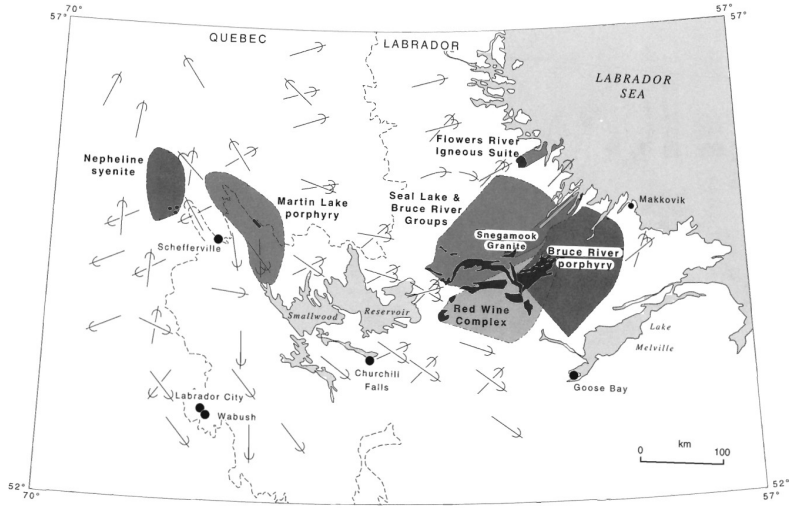

Figure 20. Indicator erratics derived from six distinct bedrock sources define glacial dispersal trains in central Labrador. The trains vary from narrow ribbons of outcrop width near the ice sheet margins, where ice flow has been relatively simple, to broad fans and patches near the locations of former ice divides in western Labrador, where ice flow has been most complex. Data are derived from field mapping of clasts (see Fig. 21 a- for detail of dispersal trains).

the dome center but rather, "the middle" as the reviewer suggested. The QLID, while a complex and dynamic feature of the Laurentide, still exhibited radial ice flow. We can see this based on reconstructions of flow lines taken and with modeling of that region of the ice sheet (Melanson et al., 2013).

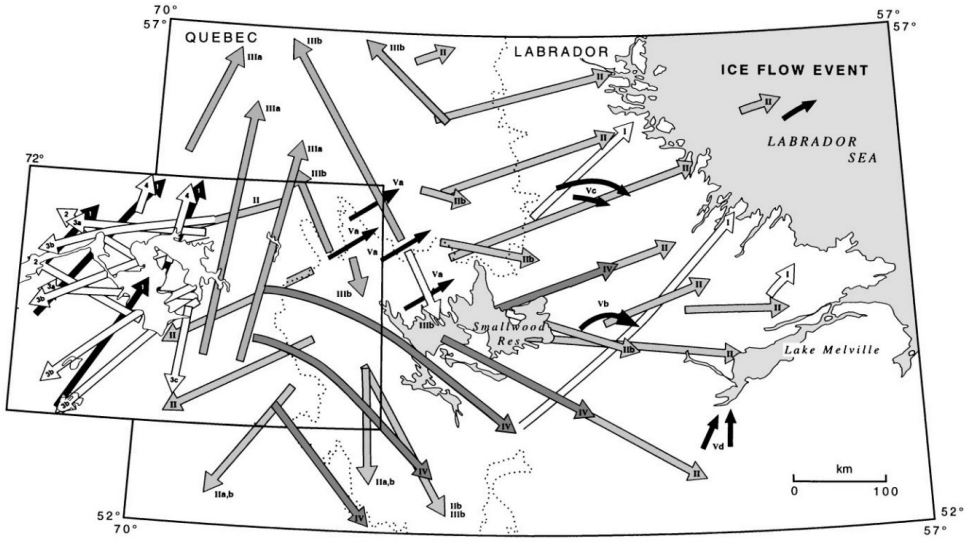

Fig. 13. Ice flow chronology of Caniapiscau area (Fig. 12) correlated with that developed by Klassen and Thompson (1993) for a larger area in Labrador.

Line 479: If the uncertainties are 1σ, why are some not equal? Just provide some clarification in the text on this.

Our manuscript already addressed this point in a footnote to the table that the reviewer cited (it's former line 479). We don't agree that this level of detail needs to be added to the text and are leaving the existing footnote. "Uncertainties account for both analytical and sample depth uncertainties. The depth uncertainties in combination with nonlinear production rate changes with depth yield asymmetrical nuclide uncertainties. Uncertainties are 1σ."

Line 489: I think there should be an addition of clarification that not as many boulders. As it reads, you are suggesting that many of the boulders contain inheritance when in fact it is only 34%.

To us, 34% is many when the assumption with exposure dating is none.

Line 497: The line Bias comso exposure ages high reads a bit awkward. Please

adjust.

Corrected

537: Need a year on the Dalton reference.

Corrected

555: The referenced articles are from the Torngat Mountains- well outside the study area

(over 500 km away), with no sediments or boulders being transported from that area to the study area (all toward the Labrador coast). These studies also specifically targeted areas of cold-based erosion where inheritance would be high.

Yes but removing these samples introduces a bias. We are presenting all extant cosmo data regardless of the authors' intent – to do otherwise would be deceptive. Readers can chose to disregard any data they find superfluous.

560: Need a year on the Melanson et al reference.
Corrected

Figure 8: I have some issues with some of the samples included in this dataset. Please see supp. Data section at the end.

That is one reviewer's perspective on data inclusion, a reviewer who did not design the study. We are presenting data from the literature. Readers are free to filter as they wish. We don't believe as authors that we should filter data based on a single reviewer's preferences when no other reviewers in two rounds of review made this suggestion.

Line 622: grey for consistency.
Corrected but will depend on copy edit journal preference

Line 630: Provenance studies of IRD could support Hudson Strait source and persistence of QLS, although their samples were a bit north:
https://doi.org/10.1016/j.quascirev.2009.08.008

We are not claiming the samples were necessarily sourced from the Hudson Bay but are raising it as a possibility.

643-44: Difficult to average such a large field area down to a single age, would it not be better to do this in groups? At least two different groups given your Figure 1 and two sets of samples spread over 3.5 ka. More groups if feasible.

We don't think such an approach makes sense for several reasons. 1) we do not know the source locations of modern-day stream sediment and two, this is a simple, "back of the envelope" calculation the accuracy and precision of which will not improve with additional splitting into smaller area and age ranges. If the editor insists, we can do such a calculation, but we don't feel it's meaningful and it appears to us, arbitrary.

647: Where was this taken from? Knowing that modern sediment contains on average 33.1 x 103 10Be atoms g-1,
This is the average we calculated for sediment samples collected from modern streams. The data are in Table 3 and in line 406. We added a citation to Table 3 because the reviewer seems to have missed this table.

649: Would a modern river erode enough bedrock to account for a component of modern sediment deposition? I think the bedrock could be removed here.
We disagree. Neither the reviewer nor we know how much bedrock erosion occurred during the Holocene. Our statement is in no way limited to river incision of bedrock it also considers bedrock surfaces both bare rock and those covered by regolith.

Line 656: in a single bedrock profile.

We wrote “a bedrock profile”. Adding single would be redundant so we did not add.

Line 658: Again, the use of commonplace is misleading.

As above, we disagree and view this a reviewer implicit bias

Line 661: Alternatively, the regolith that was removed was not thick enough to absorb all of the cosmic bombardment. There is very little regolith left from interglacial periods and most are in the far north of the QL sector, so stating it was not completely removed, while accurate, is a very minor portion of the landscape.
This is an interesting comment which is in complete disagreement with the thoughts of reviewer Briner who hypothesizes that burial as indicated by 26/10 ratios was caused by sediment not ice. We have left the statement as is in the ms but have added and/or to allow for different interpretations. We simply don’t know how much sediment was where and when and as written, our conclusion allows for that.

667-668: I think this is an important finding and justifies assuming zero postglacial erosion on the bedrock sample, which could be stated earlier than the conclusions.

We don’t see how amalgamated data from rivers can be used with any confidence to argue for no erosion of a single bedrock outcrop and so will not make this change.

Supplementary Data

There are no references for any of the references given in the text which made evaluating the data difficult but some notes:

We have added a separate references cited to the supplement. We decline to remove data that we have compiled from the field area including those of Pico et al. 2021 – these are publicly available online and we have spoken to Pico who authorized their use. If the editor prefers, the citation can be listed as personal communication. We agree that the provenance of samples in uncertain but so is the source area of the sediment we collected. We are not comfortable editing data at the request of one reviewer but have noted in the manuscript that not all geographic areas represented in our compilation are similar – although we suspect most readers are aware of this already.

Pico et al. (2021).

This paper was rejected. As per the journal's guidelines, it should not be used.

18 Oct 2021

Status: this discussion paper is a preprint. It has been under review for the journal Climate of the Past (CP). The manuscript was not accepted for further review after discussion.

Was there a glacial outburst flood in the Torngat Mountains during Marine Isotope Stage 3?

Tamara Pico ✉, Jane Willenbring, April S. Dalton, and Sidney Hemming

Gosse et al. 2006:
This reference is for the island of Newfoundland- it has its separate independent ice sector during the last glaciation which was unique in its own sense. I am not sure this data should be used for this context. It certainly was not part of the Quebec-Labrador sector.

As figure 1 shows, at 18 ka, the ice margin was well off shore of Newfoundland and thus QLID ice was discharging IRD to the ocean and it could have landed in the cored evaluated by LeBlanc.

Godbout et al. 2017:
These samples were collected from a location with a more complex depositional history than just being sourced from the Quebec Labrador sector- could equally likely to have been sourced from the Keewatin sector of the LIS. From their publication:

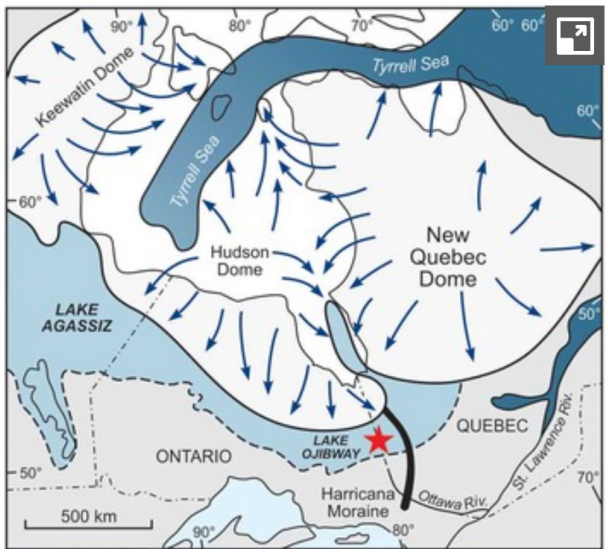

Figure 1 (A) Schematic extent of Lake Agassiz and Lake Ojibway in the context of the Laurentide Ice Sheet at ~8.5 cal yr BP (Dyke et al., 2003; Dyke, 2004); location of the study area (red star). (B) Study area and main physiographic features of the Ojibway basin in Ontario and Quebec. Note that the red box covers the area shown in Fig. 3. Triangles show the location of the outlet system related to the main stages of Lake Ojibway (orange, Angliers; blue, early Kinojévis; white, late Kinojévis; after Vincent and Hardy, 1979). (For interpretations of the references to color in this figure legend, the reader is referred to the web version of this article.)

These are not viable samples for assessing inheritance of the Quebec Labrador sector.

We disagree.

Clark et al. 2003 and Marquette et al. 2004:
These samples were collected from Alpine terrains that, especially during the moraine formations, were likely more topographically constrained to smaller geographical regions and the boulders sampled were unlikely to be sourced from the interior of the Q-L sector. At least equating these high elevation locations to the rest of the Q-L sector is misleading, they should be reported with caution if used at all.

We are reporting them so readers can choose.