Response prepared by Caleb Walcott-George on behalf of, and with input from, coauthors

Please find our responses to reviewer comments in blue. Figure numbers referenced in responses refer to the updated figure number. Line numbers refer to updated line numbers in resubmitted tracked-changes document.

Reviewer 1 (anonymous):

General comments

This manuscript describes a field and GIS campaign conducted in Northwestern Greenland. Using cosmogenic nuclide dating, landfom/geological mapping, and modelling of possible exposure histories the authors describe exposure/ice-cover history at the margin of a relatively passive section of the Greenland Ice Sheet. The authors find a signal consistent with the onset of ice streams and periods of warm based ice amongst an otherwise predominantly cold-based regime. I found the manuscript to be clearly laid-out, well-written, and enjoyable to read.

We thank reviewer 1 for this positive summary and for taking the time to review our manuscript.

Specific comments

Figure 1. Relating to my comment below (Line 100-102), I think adding an annual ice velocity composite (e.g, GrIMP) to the figure would be useful.

Great suggestion – we have added annual ice velocity to Fig. 1.

Figure 1. Could the approximate location of the images in Figure 3 be labelled here?

We added Fig. 3X to the figure to show the locations of the photographs

Figure 6. Does the size of the transparent arrow scale with the approximate magnitude of erosion?

No – this was only meant to represent a larger area of erosion, but to avoid any confusion, we've just swapped these for multiple arrows of the same size

Figure 7. As with figure 3, the location of these (and indeed all) sampling sites might be of interest and could be included as a supplementary figure.

Sample names were included in both Figs. 6 and 7 so readers can find the location of these samples.

Line 31-37: This might be a stronger opening paragraph if the formal definition of ice streams is moved elsewhere, and line 33 transitions straight into line 38. As it is here, it is not immediately clear what the relevance of ice streams are to this work.

Lines 52-71: We moved this text to our paragraph where we discuss paleo ice streams. We agree it's a better fit.

Line 81: Given discussion of transitions between thermal state earlier in the intro, I'd suggest this reference to a key transition is unnecessarily unclear and the sentence could instead be simplified to something along the lines of: "Cosmogenic nuclide and luminescence analysis of sediment and bedrock samples collected *from the glacier bed*..."

We made this edit.

Line 89--94: I would normally expect to see this text in the conclusions. A summary of the work along these lines is already given in the abstract and these may not be necessary.

Text removed.

Line 100-102: As far as I can see, this is the only comment on the current behaviour of ice bordering the study area. I wonder if there is a missed opportunity to further contextualise this study by commenting further on the current variability (or lack thereof) of the ice sheet along this margin. Annual ice velocity, though broadly fairly low (<~50 m yr^{-1}) is variable and Hiawatha glacier in particular is relatively fast compared to surrounding ice.

We agree this is good information to include. We added text at Line 126-148 to address this and bring notice to the ice velocities now included in Fig. 1:

"and ice velocities are low, ranging from 10 m yr-1 to ~120 m yr-1 (Fig. 1; Joughin et al., 2018…"

Line 112: Is the Fig 1. On this line in reference to that of England 1999? Otherwise, the Figure 1 here does not show the LGM ice sheet configuration of the study area and I would suggest the removal of the reference here.

Removed; we realize that we had already referenced Fig. 1 above

Line 117: "maintaining to a smaller position" is the *to* necessary?

That was a grammatical mistake from a previous draft. We removed this.

Section 3.3.2: Could an approximate scale range for “landscape scale” be given here. Even a mapping scale (if fixed) or a minimum mapping scale (if variable) would be useful.

Line 228 - We noted the DEM resolution lower in the section but also included it (25 m) in this sentence for enhanced clarity.

Line 187: Given this is a paper on variable erosion rates within a small area of an ice sheet, “erosive ice sheets” here feels a bit reductionist and could be more precise if it was "areas of high erosion rates beneath ice sheets" for example.

We changed ‘erosive ice sheets’ per the suggestion.

Line 200: What is meant by lakes “dammed by sediments”? Line 274: Could you specify the threshold value here please?

Removed ‘dammed by sediments,’ this was a holdover from a previous draft.

Yes, we specified the threshold value and also call attention to our new figure, Fig. 5 where we show the process used to map lakes, per Reviewer 2’s comment:

Lines 257-259: ‘(Fig. 5; cells with a value less than the threshold of 8000 were water, though we note this value may not be suitable for any location)’

Line 281: Is there precedent for this scaling? If not, could you include a brief comment on why you expect this to not affect your conclusions? It seems there might potentially be some uncertainty introduced if there was any lateral variation in retreat or any changes in the speed of retreat.

We added text at line 360-364 to address this:

“This Holocene-exposure scaling does not account for variability in Holocene lateral retreat rates. Ultimately, however, the goal of this exercise is to determine whether a long-term exposure history can explain the measured cosmogenic nuclide concentrations, which is not drastically affected by slight variations in the Holocene exposure history.”

Line 355: could an example of glacial erosional features be given? I understand they are absent but it might be useful for a reader to know exactly what is absent that might otherwise be expected.

Lines 440-441: We added clarifying text: ‘such as striations or glacial polish’

Line 381: I am missing the link here between the presence of ice-sculpting and the duration of any “wet-based ice”. As a sidenote, could a consistent terminology be used throughout (see also 573-575)? In the introduction it is warm and cold based, but here it is wet-based.

We edited the sentence to remove mention of ice cover duration and added references to the field evidence of glacial erosion:

Line 473-474: “These sites exhibit evidence of glacial erosion including sculpted bedrock and striations, suggesting the presence of warm-based ice.”

We changed text from ‘wet-based’ to ‘warm-based’ throughout.

Line 438: Could a reference to recent advance and retreat patterns be added here?

Added Søndergaard et al. (2020).

Line 443: I am not sure how relevant the Petermann Glacier is to this exact setting. Although relatively close to the study site, the ice dynamic contexts are very different, with Petermann draining a large portion of the ice sheet relative to this peripheral area of the ice sheet.

This is a good point. We’ve revised the text as this – line 552-554

“We speculate that the LGM advance across Inglefield Land was likely similarly swift given our field area represents a short distance with respect to the entire ice flowline out to the Greenland Ice Sheet LGM terminus in northern Baffin Bay.”

Line 476: I am not suggesting this is carried out here, but bathymetric investigation in the Kane Basin may be a fruitful avenue for future work to look for geomorphic investigation of ice stream signatures.

Good idea!

Line 525: suggest adding “while our results suggest that Inglefield Land was covered...”

Added to Line 667

Lines 537-541: multiple uses of “these” make the subject of these sentences unclear.

We reformatted and split this sentence in two for clarity (Line 681-685)

Lines 540-541: It might also be worth adding the role of increased meltwater delivery to the bed in a warming climate

We added: “perhaps in part due to increased meltwater delivery to the bed in warmer climates” Line 684-685

Technical comments

Line 44 (and elsewhere): glaciated is understood to mean “formerly occupied by glaciers”. The use of previously here and subsequent use of formerly are therefore redundant throughout.

Noted. We removed qualifiers such as ‘formerly’ throughout the manuscript.

Line 171: misspelling of terrains?

Yes, we corrected this.

Line 162: these should be EM dashes.

These are EM dashes in Microsoft Word

Line 186: ...generally *topographically* smooth on the landscape-scale...

added

Line 440: is there a missing the? E.g., ...collapse over the Nares Strait

I do not think so? Perhaps ours is a stylistic choice/American English usage – perhaps a copy editor could weigh in?

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Response prepared by Caleb Walcott-George on behalf of, and with input from, coauthors

Please find our responses to reviewer comments in blue. Figure numbers referenced in responses refer to the updated figure number. Line numbers refer to updated line numbers in resubmitted tracked-changes document.

Dear Authors, dear Editors,

Please find below my evaluation concerning the manuscript by Walcott-George and co-authors entitled "Glacial erosion and history of Inglefield Land, northwest Greenland" (manuscript egusphere-2024-2983). First of all, I sincerely apologize for the delay in sending my evaluation.

This manuscript investigates the ice-sheet fluctuation history and subaerial/subglacial erosion over the Quaternary in the Inglefield Land (northwest Greenland). The authors combine mapping of glacial erosion features with in-situ cosmogenic nuclides (26Al/10Be) to investigate the glacial erosion dynamics in this area and propose a quantification of both Quaternary bedrock exposure/ice-sheet covering and subaerial/subglacial erosion rates. Their mapping allows to distinguish different spatial areas with intense (ice stream, coastal zones), minor (inland) and intermediate (ice margin) erosion from landscape features and field observations. Cosmogenic nuclide data confirm these differences and allow to propose subglacial/subaerial erosion rates together with a shared surface history (bedrock exposure/ice-sheet covering) during the Quaternary.

This is an interesting manuscript, overall well-written and nicely illustrated. I think that the approach combining landscape mapping and in-situ cosmogenic nuclide dating is highly valuable and allows to provide both a spatial pattern and a quantification of subglacial erosion. In addition, the combined use of 26Al/10Be nuclides shows contrasted glacial histories for the different sectors of the Inglefield Land. I think that their approach would be interesting to the community and readership of The Cryosphere, but at present the integration of literature evidence to further support or discuss the new findings is sometimes missing, as well as some methodological information (fracture mapping) which could further strengthen the present study and the author's message.

I have outlined below my questions and suggestions in a set of general and specific comments below.

We thank Dr. Valla for taking the time to review our manuscript and appreciate his comments and suggestions.

General comments:

1 – Literature data. I would suggest to present the literature 10Be and in-situ 14C data from Inglefield Land (Sondergaard et al., 2020) at the beginning of the manuscript (section 2), and mapped (Figure 1) since these datasets could be discussed in comparison with the new data). Furthermore, they are important to support the hypothesis of a shared ice-sheet fluctuation history between all studied samples (section 3.5) which are spanning 20-30km distance. At present the information are not missing but they appear at different places in the manuscript and the literature samples cannot be located in the study area.

The original manuscript references the deglaciation history from Søndergaard et al. (2020) at the end of Section 2. Inglefield Land.

We added the location of the boulder samples from Søndergaard et al. to Fig. 1

2 – Mapping and figures. The figures 1 to 6 (excluding figure 3) are sometimes repeating the information and can be synthesized into fewer figures I think. Moreover, the figure 4 (fracture mapping) is somehow misleading since it already shows the areas with bedrock high-density fractures, but there is no figure to show the actual mapping of these areas (same remark for lake cover from LANDSAT images, a methodological zoom would be possibly interesting for readers). Please consider reworking the figures to provide more illustration of the mapped objects.

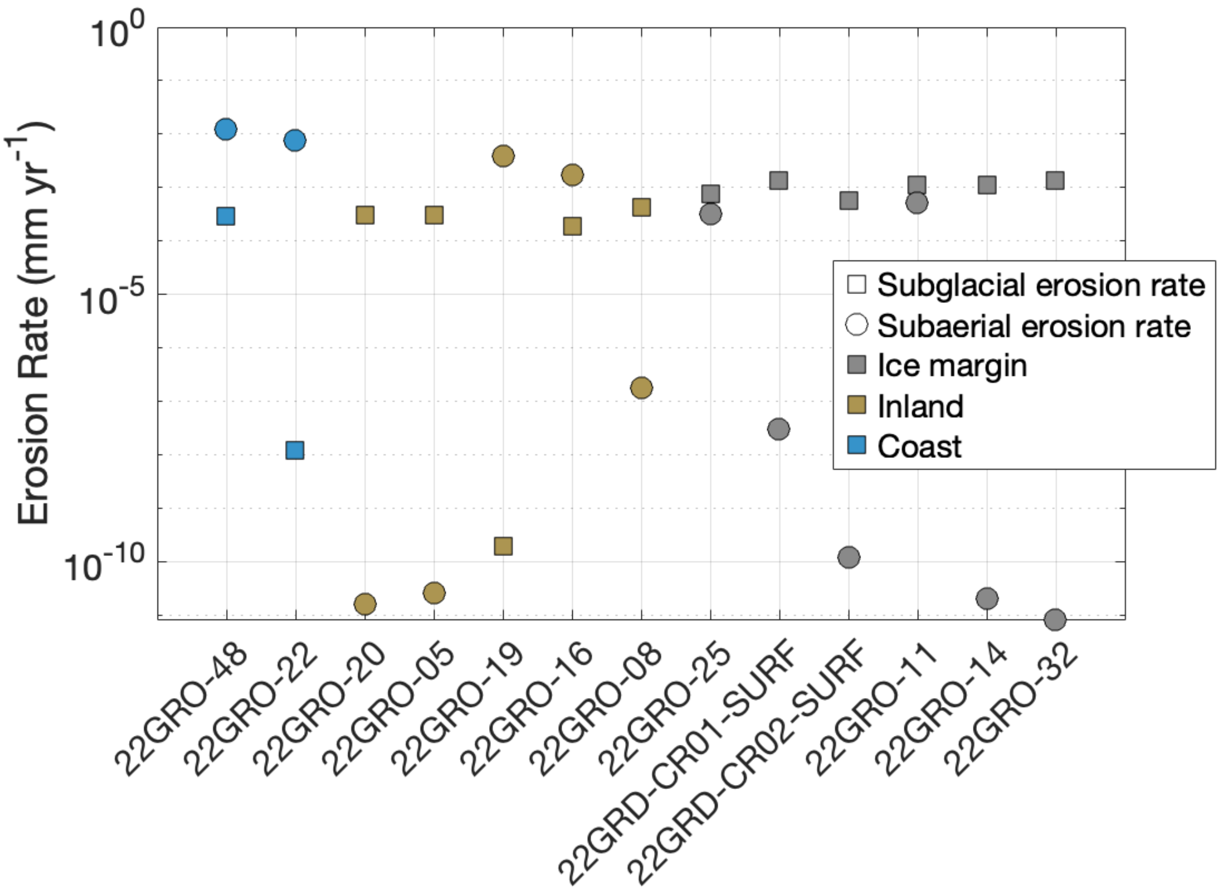
We understand the need to present information as succinctly as possible, but our goal here is to present the content clearly and avoid making overly dense figures where the intended message is lost in too much detail. We believe the individual maps also illustrate our iterative process.

Those are good suggestions for helping illustrate our methodology for future researchers. We added an inset panel to Fig. 4 showing a zoom-in of one of our mapped fracture zones. Additionally, we created a new figure, Fig. 5, that shows our lake mapping procedure.

3 – Discussion of output erosion values. I enjoyed the model outcomes for subaerial and subglacial erosion rates (Figure 11), and their discussions in comparison to literature data. However, there is only little discussion about the spatial variability of these erosion rates between the different sectors (coast/inland/ice margin) and in comparison with the mapping outcomes (Figure 6). Are the output subglacial/suaerial erosion rates really correlated as proposed in the methods section? Can we hypothesize that subaerial erosion rates could be uniform over the study area, and if yes what would be the implications for subglacial erosion rates? Finally, are output subglacial erosion rates really variable between the different sectors to explain the differences in in-situ cosmogenic nuclides? I think the readers will benefit from more discussion around these points given the interesting model outcomes.

There are several great points listed here, which we expand on below and also further down in the response section, where there were specific comments that addressed some of these.

- Yes, the subaerial and subglacial erosion rates do trade off; higher subglacial erosion rates can be balanced by lower subaerial erosion rates and vice-versa. For example, this figure shows best-fit sub-glacial and sub-aerial erosion rates for the exposure-burial scenario corresponding to a $\delta^{18}O$ threshold of 3.74 ‰. However, we want to emphasize that though these best fits are just shown to exemplify this tradeoff, and the full range plausible erosion rates in Fig. 12 is more appropriate for highlighting these possibilities.



We also discuss this further in the next bullet point.

- That is a good point surrounding the paucity of discussion on the spatial variability of subglacial erosion rates – we add the following text to lines 533 – 536:

  "We do not observe clear trends in the modeled subaerial erosion rates, though it is worth noting that subglacial erosion rates are modeled to be greater than about 1 x 10-4 mm yr-1 for ice margin areas, while these exhibit greater variability for inland and coastal samples.”

And lines 649-664:

“Subglacial erosion rates that yield good model-data fits are between 0 and ~2 x 10-3 mm yr-1 across our sites, and all samples have possible subglacial erosion rates >~1 x 10-4 mm yr-1. Samples at the ice margin do not have modeled erosion rates below this value, and their maximum modeled erosion rates (~1 x 10-3 mm yr-1) are higher modeled maximum erosion rates at the inland sites (~5 x 10-4 mm yr-1). This indicates that the bedrock surfaces at the modern ice margin are consistently eroded during ice burial and may experience higher subglacial erosion rates. Some inland sites have subglacial erosion rates of zero mm yr-1, thus highlighting the potential for spatial variability of subglacial erosion rates under a cold-based ice sheet. Furthermore, subglacial erosion rate estimates from our sites with the lowest levels of cosmogenic nuclide inheritance (at the coast) exhibit the largest spread in modeled subglacial erosion rates. These coastal samples also have wide ranges of subaerial erosion rates, and while lower nuclide concentrations indicates that at either subglacial or subaerial erosion must be high to limit nuclide accumulation, we cannot determine which kind of erosion is more significant.”

- As we discuss further below, we do not think that subaerial erosion rates are uniform across the study area. Evidence of intense subaerial weathering – gruss, weathering pits, etc. – paired with varying lithologies across Inglefield Land with different crystal size and mineralogies (which lead to different erosion rates (Margreth et al., 2016), indicates that subaerial erosion rates indeed vary across sampling sites and outcrops. However, it’s worth noting that subaerial rates ~1x10^{-3} mm/yr are compatible for most samples.
- It’s possible that subglacial erosion rates may have varied across different sectors based on the model results (Fig. 12), though perhaps all samples were subject to ~1 x 10-4 mm yr-1 during ice burial. We try to highlight this possibility in the sections added above.

Specific comments, by line number:

- Line 21. “... requires minimum surface histories of ~150 to 2000 kyr”. Maybe precise what is meant by “surface histories” there for clarity.

Line 21: We clarified as ‘minimum combined surface burial and exposure histories’

- Lines 26-27. “between 0 and 2 x 10^-2 mm yr^-1”. Please consider rephrasing in “maximum 2 x 10^-2 mm yr^-1”. Same remark for line 27.

We believe it is important to emphasize that *no* erosion (i.e., 0 mm yr^{-1}) is one of our model outputs and think that just stating “maximum 2 x 10^-2 mm yr^-1” could yield the false assumption that there are no scenarios with zero erosion.

- Line 31. “the largest single contributor”. Why “single” there?

Mountain glaciers as a whole contribute more to sea level rise, but the Greenland Ice Sheet outweighs any one mountain glacier. That being said, we can see how this is misleading, so we’ve changed the text to (Line 32):

“The Greenland Ice Sheet currently contributes more to sea level rise than any other single ice mass…?

- Line 45. “...and basal processes”. Maybe specify which ones.

Line 43: Added ‘such as glacial erosion’

- Line 46. “The distribution of glacial erosional features”. Which ones, and at what spatial scale?

Line 44-45: Changed to ‘The distribution of glacial erosional features including striations, lakes, lateral meltwater channels, and sculpted bedrock across glaciated landscapes at ice-sheet scale’

- Line 49. “ancient landscape”. How ancient, Quaternary or even pre-glacial?

Both – we added clarification here: ‘(Quaternary/pre-Quaternary) – Line 48

- Line 54. “mapping of erosion imprints on a landscape”. I am wondering how these glacial surface features may be preserved or altered during post-glacial exposure (i.e. weathering and geomorphic processes). Maybe can the authors comment on this?

In high Arctic landscapes, the cited studies have found clear transitions in glaciated landscapes between non-eroded and eroded areas that have not been altered enough by post-glacial exposure to inhibit their delineation.

- Line 62. “from multiple periods of exposure”. Maybe specify “during interglacials”.

We hesitate to make this change here, because it’s possible this exposure occurred outside of the known interglacials, e.g., MIS 5e, 11

- Line 69. “shielded from the cosmic ray flux by ice...”. Please specify for non-experts the minimum ice thickness for complete shielding.

Line 88-89: Changed to/added: “(e.g., 10Be, 26Al; no significant production beneath 10s m of ice; Miller et al., 2006).”

- Line 75. “and longer-term ice sheet history...”. I think this could be interesting for readers to have a brief summary of the main outcomes of these previous works.

For brevity, we choose not to summarize these here, as we just want to emphasize the wealth of possibilities from bedrock dating. These citations should point curious readers to the correct literature.

- Line 77. “the bed of extant ice sheets”. Unclear, please rephrase.

Extant, as in currently existing. We added “modern” and “i.e., Greenland and Antarctic ice sheets” to make this distinction clearer. Line 107 - 108

- Line 81. “under ice sheets”. Maybe add “modern” for clarity.

We changed this to “glacier bed” per Reviewer 1’s suggestion and added ‘modern’, for “modern glacier bed” at Line 112

- Line 94. “rates to vary for each sample”. I would suggest to recall there, as for the total surface history, the main outcome in terms of subglacial/subaerial erosion rates.

Per Reviewer 1’s suggestions, we removed mentions of our findings from the Introduction

- Line 106. “Dallas Bay, and Marshall Bay”. From Figure 1, Marshall Bay appears more to the west than Dallas Bay, please correct.

Corrected

- Line 115. “and in-situ 14C from erratic boulders”. Where are these samples in the study area? Would it be possible to show them on Figure 1? (see also my general comment).

Added location and *in-situ* 14C ages from Søndergaard et al. (2020) to the map

- Line 121. “cosmogenic nuclide inheritance”. 10Be and/or 26Al data? Please specify.

Specified as ^{10}Be in text – Line 166

- Line 121. “in boulders across Inglefield...”. Can this literature dataset be shown on figure 1 and compared to the new data from this study?

We added these to Fig. 1, see above response. Given that Søndergaard measured cosmogenic nuclides in boulders and cobbles, we cannot directly compare their ^{10}Be data to our results from bedrock samples.

- Line 125. “bedrock at Camp Century and Summit”. Please provide locations of these zones for readers, maybe in Fig.1 inset, for clarity?

Added

- Line 151. “variable erosion across the landscape”. Specify whether this is subglacial, subaerial or both.

Clarified as ‘subglacial and subaerial’ to Line 187

- Line 167. “relative amount of weathering or ice sculpting”. How is this evaluated? What form(s) or evidence? Please provide more details about this approach.

We added examples of common characteristics associated with ice sheet erosion or weathered bedrock:

“whether bedrock outcrops exhibited evidence of ice sculpting (e.g., sculpted bedrock, striations, glacial polish) or heavy weathering (e.g., grussification, weathering pits; Figs. 2, 3).” Lines 217-219

- Line 180. “hillshade image with a three-times vertical exaggeration for our mapping (Fig. 4)”. It would be nice for readers to have an example of such hillshade images, to help evaluating how mapping has been performed to delimitate zones on figure 4.

Per the above suggestion, we added an inset map showing the fracture zone in higher detail.

- Line 182. “to avoid issues with differentiating fractures from other features”. Unclear, please rephrase.

Changed to: “but coarse enough to avoid mistaking smaller fractures for other landscape features.” Line 233-234

- Line 192. “...shows larger lakes”. Are these the lakes shown on Figure 2? If yes, maybe specify it.

Added reference to Fig. 2 – Line 254

- Line 198. “a suitable threshold value”. Maybe indicate the chosen value for readers.

We added this per a suggestion from Reviewer 1.

- Line 208. “along two SSE-NNW transects...”. Please refer to Table S1 for sample locations.

Added reference. Line 279

- Line 240. “and 26Al sample”. Is there a blank correction for 26Al (not provided in Table S2). Moreover 10Be/9Be and 26Al/27Al blank ratios are not provided in main text nor in Tables S1-S2, or maybe I missed them. Please correct or clarify for readers.

We added process blank ratios to our tables.

- Line 259. “in-situ 14C ages”. Are these ages also from boulder samples, or only those with 10Be ages? This reads unclear from the present sentence, please clarify.

Changed to: “Although *in-situ* 14C ages from boulders…” Lines 331-332

- Line 264. “Given the relatively small distance from the modern ice margin to coast and the speed at which Inglefield Land deglaciated following the LGM”. This is a strong hypothesis, and poses question about its validity given that 14C ages are only described as Holocene ages. There is more information given on lines 281-284, but I would suggest to present further the 14C data (also show them on a map) since these are important evidence for the reasoning. See also my general comment on this point.

Given our reworking and greater explanation of Søndergaard et al.’s results earlier in the manuscript, I don’t think we need to describe it here again, but instead, we’ve added the actual numbers for ‘short distance’ of ‘<50 km’ and speed ‘2 kyr’ to remind readers of their results. We also added the citation. -Line 336-337

- Line 316. Would we expect similar ranges for subaerial and subglacial erosion rates? I see from the broad range that this would be possible, but maybe clarify for readers.

We expand on this, explicitly saying that this range is for both subglacial and subaerial erosion rates:

“For each exposure history, we ran the forward model with subaerial and subglacial erosion rates ranging from 0 to 2.5 x 10-1 mm yr-1 on a log scale (coarse spacing from 0 to 1 x 10-5 mm yr-1 and finer spacing from 1 x 10-5 to 2.5 x 10-1 mm yr-1) to capture the potential range in subglacial erosion rates for cold-bedded glaciers and subaerial erosion rates for polar environments (e.g., Cook et al., 2020; Koppes et al., 2015; Portenga and Bierman, 2015).” Lines 393-398

- Line 352. “of various minor rock units...”. Is this needed, since we do not see the minor units on figure 2?

Thank you for catching this editing mistake on our end

- Lines 360-364. This was not entirely clear from the Methods text and Figure 4 that contoured zones are mapped fractured areas, not the investigated ones. Please clarify this, and potentially show a zoom on a fractured zone to show the approach to readers. See also my general comment.

Added ‘as mapped in’. See also response where we added zoom-in of fractures in inset map. Lines 447

- Line 379. “Apparent 10Be ages”. Maybe add “exposure” for clarity.

Done

Can the authors specify the apparent 10Be exposure age for the boulder sample in this paragraph? This is not explicit from the text.

Added: “Our single boulder sample at the western ice margin site has an apparent 10Be exposure age of 50.2 ± 0.9 ka.” Lines 482-483

- Lines 418-419. Please refer to Figure 9 there. Would it be possible to evaluate a range/uncertainty for the threshold value, discarding one sample or another? I have the impression that the range 3.55-3.65‰ could be possible, would it be correct and what implications for the exposure/ice-cover durations? Please consider discussing this point.

We added a reference to Figure 10 (new figure number) here. Line 515

This is, in essence, what Figure 10 shows. We want to stress that 3.74‰ is the only $\delta^{18}O$ threshold that matches each sample, not identified as an outlier previously. Given previous studies in Greenland ice margin areas cited here (Balter-Kennedy et al., 2021 and Knudsen et al., 2015), we consider our search window of 3.60 to 4.00‰ appropriate.

Also, we want to emphasize that the goal of our model was to just see if it were *possible* that all sites have the same exposure history, so we kept it very first order.

We also added some additional notes to Lines 522-524

“However, other scenarios with similar δ18O threshold values of 3.72 and 3.76‰ fit most of the non-outlier bedrock samples though fewer than our best-fit δ18O threshold value of 3.74‰.”

- Line 421. “This best-fitting exposure history...”. Is this outcome in agreement with the Holocene final deglaciation as proposed by in-situ 14C ages? From Figure 10 I have the impression that a late post-LGM ice retreat is compatible with a 3.6‰ value, but this is difficult to see.

1) Holocene deglaciation (and exposure) is decoupled from this δ18O threshold-based exposure duration calculation in our model since we have relatively good constraints on this from Søndergaard et al. (2020).

2) We applied a 30 kyr smoothing to the δ18O curve, which affects the timing of exposure implied by the threshold value.

3) This δ18O threshold is a way to construct an exposure history that is grounded in a climate record but doesn't necessarily map directly to the site at all times. I.e., we don't really expect the crossing of the threshold to correspond to the local deglaciation age. This makes it a useful exercise for long term history, but not so much for short term (i.e., Holocene)

Thus, we added the following text to Section 3.5 Lines 391-393:

"We decouple this δ18O threshold-based exposure timing during Holocene deglaciation (Knudsen et al., 2015), as the timing of Holocene exposure is relatively well-known from the constraints from Søndergaard et al. (2020)."

And to the caption of Fig. 11:

"Note Holocene exposure is decoupled from the δ18O threshold-based exposure duration calculations and is thus not represented on the plot."

Line 424. Please refer to Figure 11.

Done

- Line 428. "likely has a different exposure history than the collocated bedrock.". How about for output erosion rates, are they similar between bedrock surfaces and the boulder?

We added text to reflect this in Lines 517-533:

"Yet only the exposure history constructed with a δ18O threshold value of 3.74‰ yields 10Be and 26Al concentrations with a good fit to the data in all bedrock samples except those with 26Al/10Be ratios above the production ratio (22GRO-01 and 22GRD-CR04-SURF), which is the sample previously identified as an outlier based on its low 26Al/10Be ratio (22GRO-03), *and our boulder sample (22GRO-32).* This best-fitting exposure history corresponds to ~0.9 Myr of total exposure and ~1.8 Myr of total burial over the last 2.7 Myr (Fig. 10). Given this ice-cover scenario, we find model-data agreement with subglacial and subaerial erosion rates ranging between 0 and ~2 x 10-3 mm yr-1 and 0 and ~2 x 10-2 mm yr-1, respectively. *For our best-fit exposure scenario using a δ18O threshold values of 3.74, we were not able to simulate erosion rates for bedrock samples that did not have a good fit with this δ18O threshold value (22GRO-01, 22GRO-03, and 22GRD-CR04-SURF).* While there were some exposure and erosion combinations that were compatible with measurements from our single boulder sample (22GRO-32), we prioritized bedrock samples in assessing whether all sites could have a common Quaternary ice-cover history, as the boulder likely has a different exposure *and erosion* history than the collocated bedrock."

- Lines 438-442. These lines may already be presented in the settings, since they give important information for the rapid ice-sheet fluctuations in the study area and support the hypothesis given in section 3.5. See also my general comment.

Indeed, this is repeated in the Setting Section and Section 3.5 (per your above comment), but we choose to build on this here as they provide important information

Another secondary question: what is the Nares Strait, is it visible on figure 1?

Nares Strait is NE of Inglefield Land and during the LGM, the Greenland and Innuitian ice sheets coalesced there, creating a saddle. But we realized this detail may just lead to confusion and isn't necessary for our story. We remove mention of this; Nare Strait is off the map in Fig. 1

- Line 453. "our other mapping". What is "other"?

Clarified by including 'our mapping of fractures and lakes' Line 563

- Line 510. "0 mm yr^-1 and 2 mm yr^-1". Maybe this can be rephrased as "0-2 mm yr^-1". I am wondering whether the results can be discussed as spatially homogeneous or whether there is a spatial pattern for subaerial/subglacial erosion rates. See also my general comment.

Agreed

- Lines 516-517. The output subaerial erosion rates appear much higher for some samples than for the literature value. What are the implications for subglacial erosion rates is a common subaerial erosion rate is taken (e.g. 2-3 x 10^-3 mm yr^-1)? And would this hypothesis of a regional similar subaerial erosion rate be plausible? Please discuss this point.

Indeed, our model results do suggest that subaerial erosion rates greater than the published literature values are valid with the range of modeled subglacial erosion rates with a $\delta^{18}O$ threshold value of 3.74‰. We also want to emphasize that these range of erosion rates represent the range of *possibilities within the limitations of our model framework.*

There is a trade-off between sub-aerial and sub-glacial erosion rates; i.e., greater sub-aerial erosion rates mean lower sub-glacial erosion rates and vice-versa. Given the lithology differences within the gneiss units across Inglefield Land, and the evidence of variable subaerial erosion across the landscape (i.e., frost heaving of bedrock pieces, abundant gruss as a result of subaerial weathering, and weathering pits), we do not think it is geologically reasonable to apply a similar subaerial erosion rate across all of our sites. Furthermore, this field evidence of variable subaerial erosion could lend credence to the higher modeled subaerial erosion rates.

We added the following text to section 5.3 to explain why a regional subaerial erosion rate is not likely Lines 645-648:

"Additionally, the presence of weathering features including gruss and weathering pits across Inglefield Land indicate that subaerial erosion likely varies not only from outcrop to outcrop as a result of varying lithology, but also across individual bedrock outcrops (e.g., sampled from near an old weathering pit)."

- Line 523. "0 and 2 x 10^-3 mm yr^-1" for subglacial erosion rates, no? Same remark for line 546.

Yes, this was an error. We have corrected to 0 and 2 x 10^-2 mm yr^-1

- Line 538. Also the literature values for subglacial erosion rates are mostly integrated ofer short periods (maximum LGM), no? These then might be a temporal integration effect, as already shown by Spotila et al. (2004) or Koppes & Montgomery (2009). Maybe discuss this point.

This is a great point. We added this text to the end of Section 5.3 reflect this Lines 685-688:

" Additionally, these studies of glacial erosion focus on relatively short timescales (<multi-millennial), while our modeled erosion rates are integrated over the Quaternary, perhaps highlighting the effects of temporal intergration as previously noted by Koppes and Montgomery (2009) and Spotila et al. (2004)"

- Lines 561-565. "if differential erosion is invoked..."; "Spatial variability in erosion is captured...". Are subglacial erosion rates really different between the studied sites (coast/inland/ice margin)? This is not straightforward from Figure 11, and would require I think more discussion about the model outcomes. See also my general comment.

Our intention is not to favor specific erosion rates over another at each sample location, but to reiterate that by allowing erosion rates to vary by location (i.e., not have to have the same subglacial/subaerial erosion rates), the cosmogenic nuclide concentrations can be explained by a shared ice history. We've revised the text in Section 5.3 to reiterate this Lines 712-717:

"In sum, we find that the disparate nuclide concentrations across Inglefield Land can be explained by a common Quaternary exposure/burial history if erosion rates are allowed to vary *sample-by-sample*. Erosion rates consistent with the cosmogenic data are consistent with others found in polar areas covered by cold-based glaciers. Variability in erosion rates across the landscape likely reflect differences in lithology, as well as subglacial conditions. *By allowing our erosion rates to vary at each sampling location*…"

- Line 600. Can this part of the discussion be extended by comparing outcome results to those of previous studies using cosmogenic nuclides (in Greenland or Scandinavia) for estimating surface histories?

We do not wish to mislead readers to think that our cosmogenic nuclide concentrations can definitively say more than we already have written/interpreted, namely that our concentrations suggest that the Greenland Ice Sheet was larger than today during some Pleistocene interglacials.

- Line 675 – Figure 1. Several questions/remarks for this figure:

- Maybe add the literature data (boulders with 10Be and in-situ 14C dating) for clarity.

We added the boulders for 14C dating from Søndergaard et al. (2020) as these pertain directly to the study.

- Is there a Dallas valley in front of the Dallas Bay, as for other locations?

There is an unnamed valley, but because we do not include it in our discussions, we do not label it as such.

- What is the resolution and source of the DEM used for topography?

As noted in Section 3.3.2, this is a 25 m DEM from Korsgaard et al. (2016). We also added text to the legend for clarity

- What are the text “100 m asl” and “255 m asl” for coastal sites?

These are the general elevations/site IDs of the sites as discussed in Section 3.4.1

- Line 703 – Figure 3: Can the authors provide the locations of pictures on Figure 1? And maybe a quick description of the pictures in the caption could help (or in the main text line167).

The photos already have the Sample IDs that can be referenced to our summary map. To avoid cluttering Fig 1 (in consideration of the suggestions for Fig. 1 made by Reviewer 1), we choose not to include the locations in this Figure as their locations are visible on Fig. 7.

To the caption we added “Note the smoothness of the coastal outcrops compared to the rough, weathered textures of the inland and ice-margin sites.”

- Line 719 – Figure 4. This figure is not really explicit and a bit redundant to figures 1-2 and 6. Can this be replaced by a zoom on a fractured zone that was actually mapped?

We chose *The Cryosphere* to have room for figures and clarity in the points we wish to illustrate. We believe displaying our erosional proxies piecewise helps tell our story. We included a zoom in of a fracture zone as an inset.

- Line 752 – Figure 6. The label code “CR” is missing for SURF samples. Please also indicate the boulder sample for clarity. Finally, are the arrows (potential zone of increased erosion from mapping) discussed somewhere in the text with respect to subglacial erosion rates (Fig. 11)?

We added ‘CR’ to the appropriate samples and text to the caption indicating the superscript ‘b’ represents the boulder sample.

Per Reviewer 1’s comment, we have changed the size of the arrows to be equal so that they do not falsely reflect different erosion rates. These are meant to be indicators of where we have ice streams mapped as beginning.