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Climate of the Past
Discussions

Interactive comment on "Cosmogenic isotope measurements from recently deglaciated bedrock as a new tool to decipher changes in Greenland Ice Sheet size" *by* Nicolás E. Young et al.

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Thanks for reviewing the manuscript and providing some constructive comments. Here, we provide the main reviewer comments in quotations and our responses follow.

"Separate retreat timing from Kapisigdlit moraines? - One of the main conclusions is that the Kapisigdlit moraine deposition occurred with different timing for KNS (10.24 ± 0.36 ka) and Qamanaarsuup Sermia (9.57 ± 0.38 ka), thus suggesting a new mode of GrIS moraine deposition during the Holocene. However, don't these two mean ages overlap at 1σ? The overlap is small, but an overlap nonetheless. It seems statistically possible that these two ages are equivalent. At the very least, it would be good to show

t-test statistics to help show the level of statistical difference between these ages. If the difference between these ages is not significant, then some of the wording of the paper may need to be modified to indicate the possibility of synchronous moraine deposition."

Yes, this is a good catch by the reviewer. This is certainly something we have thought about but did not explain well in the text. The reviewer is absolutely correct that the two mean ages barely overlap at 1-sigma, no question. What we failed to mention in the text, and what we can address in the revision, is the various differences in moraine setting and the distribution of 10Be ages. From our extensive experience in southwestern Greenland, we have found that the 10Be ages from erratic boulders perched on bedrock immediately inside a moraine serve an extremely close limiting age on the moraine itself. In fact, often 10Be ages from moraine boulders and inboard erratics are statistically identical. This is the case with the older Kapisigdlit moraine that the reviewer mentions. However, at Qamanaarsup Sermia, while the mean age from the moraine boulders themselves barely overlaps the mean age of the Kapisigdlit moraine boulders (what the reviewer is referring to here), the inboard erratic ages at Qamanaarsup Sermia are much younger (~9.3 ka) than the inboard erratic ages for the Kapisigdlit moraine. Considering what we typically see on Greenland, if the moraine at Qamanaarsup Sermia were actually a ~10.3-10.2 ka moraine, we might expect the inboard erratic ages to be ~10.2-10.0 ka, similar to what we see at the Kapisigdlit moraine. However, because the erratics ages at Qamanaarsup Sermia are tightly clustered, we think ~9.3 ka is a robust close minimum constraint on the age of the moraine at Qamanaarsup Sermia. In other words, this moraine was likely deposited just prior to ~9.3 ka.

On the other hand, there is a decent amount of scatter in the Qamanaarsup Sermia moraine boulder dataset and the multi-crest nature of the moraine here points to an oscillating or stagnating ice margin. Therefore, it possible that there may be several distinct episodes of ice-margin advance/stillstand represented on the landscape and by combining all of our 10Be ages, we have inadvertently incorporated a small degree of inheritance into our preferred moraine age. For example, if this moraine complex represents advances of the ice margin at 10.3 ka and 9.3 ka, and boulders from both these episodes are getting re-worked with each other, then one might expect to get a mean moraine age of 9.6 or 9.7 ka if one samples boulders across the entire moraine complex. Regardless, the reviewer makes a good point here that this isn't the easiest comparison to make between different moraine ages, and something we are a bit unaccustomed to in southwestern Greenland. However, we actually think that while the Kapisigdlit moraine was likely deposited at 10.3-10.2 ka, the moraine complex at Qamanaarsup Sermia was deposited either at 9.57 ± 0.38 ka (stated in text), or we inadvertently sampled a composite feature and part of our boulder population actually constrains an ice advance ca. 9.3 ka, which is further supported by the 10Be ages from erratic boulders resting immediately inboard of the moraine that are also 9.3 ka. We can expand on this in the text.

"Ice Sheet Model Methods (section 2.7) – I think it would be good to offer a little more description on the nature of the model setup and experiment design. I realize this model is extensively described in Cuzzone et al (2019) and Briner et al. (2020), but there are some crucial distinctions that could be added here that would help in understanding the results. For example: -What is the nature of the surface mass balance calculations? PDD? -Provide a general description of the flow dynamics. -mention the lack of calving in the methods (it gets brought up later, but it would be good to mention such model limitations in the methods -More description of the 9 model combinations/experiments (line 238-239). How were these 9 permutations selected? How do they differ?"

Yes, we can expand on the model experimental design. Initially, we just cited the relevant references that have been recently published in order to help limit the length of this already long manuscript. Yet, we do recognize that a few more details about the model and climatology set-up in this manuscript might be helpful to readers. Following the first paragraph of Section 2.7, added text will include:

The higher-order approximation (Blatter 1995; Pattyn 2003) is used to solve the momentum balance equations. We use an enthalpy formulation (Ashwanden et al., 2012) to simulate the thermal evolution of the ice, using geothermal heat flux from Shapiro and Ritzwoller et al. (2004). The ice model uses quadratic finite elements (P1 x P2) along the z axis for the vertical interpolation, which allows the ice-sheet model to capture sharp thermal gradients near the bed, while reducing computational costs associated with running a linear vertical interpolation with increased vertical layers (Cuzzone et al., 2018). Subelement grounding-line migration (Serrousi et al., 2013) is included in these simulations, however, due to prohibitive costs associated with running a higher-order ice model over paleoclimate timescales these simulations do not include calving parameterizations nor any submarine melting of floating ice.

Nine ice-sheet simulations are forced with paleoclimate reconstructions from Badgeley et al. (2020) who used paleoclimate data assimilation to merge information from paleoclimate proxies and global climate models. The temperature reconstructions rely on oxygen-isotope records from eight ice cores, the precipitation reconstructions use accumulation records from five ice cores, and all are guided by spatial relationships derived from the transient climate-model simulation TraCE-21ka (Liu et al., 2009; He et al., 2013). The climate reconstructions are shown to be in good agreement with independent paleoclimate proxy data (Badgeley et al., 2020 and references therein). Along with a main temperature and precipitation reconstruction, Badgeley et al. (2020) provide two sensitivity precipitation reconstructions due to uncertainty in the accumulation records and four sensitivity temperature reconstructions due to uncertainty in the relationship between oxygen isotopes and surface air temperature. Briner et al. (2020) pair three of the temperature reconstructions with each of the three precipitation reconstructions to yield nine combinations that are used as transient climate boundary conditions to force the nine ice sheet simulations. Two of the five temperature reconstructions were not used because they yield Younger Dryas ice-sheet margins that are inconsistent with geologic data.

In order to compute the surface mass balance from temperature and precipitation, a positive degree day (PDD) method is used (Tarasov and Peltier, 1999). We use degree-day factors of 4.3 mm ïĆřC-1 day-1 for snow and 8.3 mm ïĆřC-1 day-1 for ice, with allocation for the formation of superimposed ice (Janssens and Huybrechts, 2000). A lapse rate of 6 ïĆřC km-1 is used to adjust the temperature of the climate forcings to the ice surface elevation."

"Discussion of marine Terminating Dynamics (lines 748-764) – How much of the full model domain is influenced by marine dynamics and iceberg calving? Since these regions minimize retreat in the model, is there a way to show or discuss how much of the model domain would be influenced by this model limitation. Are any of the margins still marine-terminating at the minimum Holocene extent?"

We are attaching a figure that shows our model domain, and our initial marine and land terminating regions. The marine areas, where the ice base is below 0 are shown in red. Land terminating regions, where the ice base is above sea-level are shown in blue. Throughout our simulations, RSL changes, and therefore sea-level varies. So marine and land terminating portions of the domain will change through time. But this figure should illustrate that we capture (with our model mesh) marine terminating margins in many of the fjord regions. Our 9 different simulations all have distinct ice extents during the Holocene minimum. Because we capture the KNS fjord geometry reasonably, these margins would be marine terminating, and some portions of the outlet glaciers are floating in our simulations (Although we do not simulate calving, we do simulate grounding line migration). If it is necessary to show, we can try to put together a figure showing elements where there is floating ice during the simulations. But we note that this makes up a very small % of the model domain («1%).

It is hard to determine exactly how our model domain would be impacted by this limitation (no calving) without performing the experiments, however, areas at the ice front and upstream might be affected by calving even in warm climates since the ice is fast flowing and tends to maintain contact with the ocean during those times.

"Technical comments"

Again, thanks for catching these. We will address them in the revision.

"Line 201 – Specifically, what is the production rate uncertainty that was used in quadrature? Is it a constant percentage? Or is it spatially varying? Line 365 – "mean age of 10.20 ± 0.14 ka (10.27 ± 0.23 ka with production-rate uncertainty)." Why are the mean ages (10.20 and 10.27) different? Shouldn't the production rate uncertainty added in quadrature only effect the uncertainty value? Also, double check on these values are appropriately displayed in Figure 2."

We will clarify. 1.8% used in quadrature. Also, thanks for catching the moraine age typos, will double-check.

"Lines 373-377 – Would it be possible to include this early photograph with permission? It would be nice to see this photograph annotated to show the ice extent and trim-line as described in the text."

We can inquire, but make no promises here – although we agree this would be nice. This is an old photograph that currently exists in a GEUS bulletin focused on the KNS region that is somewhat widely available (Weidick, 2012).

"Line 437 – I think the use of the phrase "more proximal" is confusing here. These high elevation sites are less proximal from the historical limit, when considering ice position. I would say that the recently deglaciated sites are "more proximal" than the historical limit. Do you mean that the high elevation sites are closer to the historical maximum limit? Lines 508-509 – "we favor an interpretation that couples less site exposure over significant amounts of subglacial abrasion" – I think the use of the word "couples" is confusing here. What is being coupled to what?"

We can clean up the wording here.

"Figure 1 – The orange diamond of Weidick et al (2012) is very hard to distinguish from other yellow circles. Consider using an alternative symbol. Figure 2 & Figure 3 –

In general it can be difficult to distinguish between italicized outliers and non-outliers. Is there a different way to distinguish outliers other than italics? Figure 17c and 17d – What does this distance axis mean? Is it distance from the coast, or some other arbitrary point? Would be good to clarify in the caption. Figure 17c and 17d – Is there a way to better display the present day location? The yellow dots are hard to see on the first pass Figure 17c-17f – On each of the model result figures, it is hard to distinguish between the green lines and the blue lines. Would it be possible to use a more distinct color gradient for these groupings of simulations?"

These minor figure edits are pretty straightforward. Our model results color scheme was used in order to remain consistent with the color scheme used in Briner et al (2020), but we can address this.

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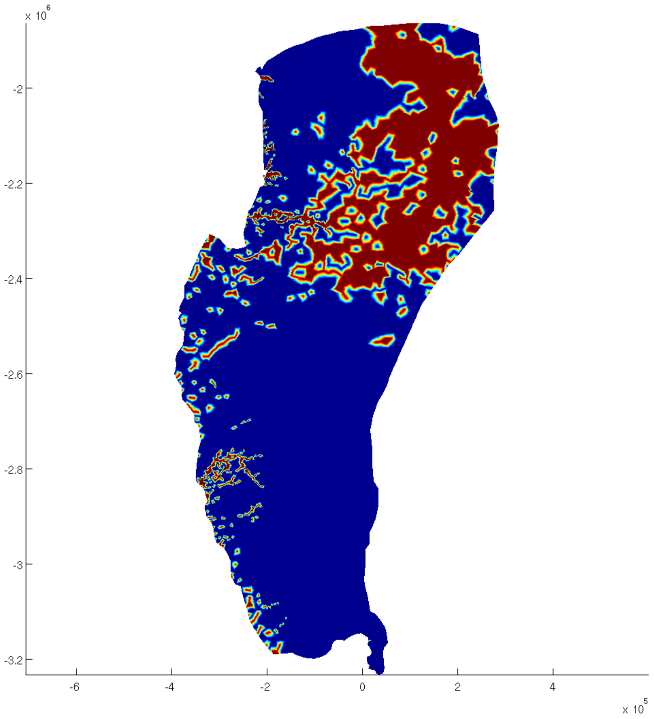

Fig. 1. Marine- vs. Land-terminating portions of the model domain