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Reply on RC2

Jason P. Briner et al.

Author comment on "Drill-site selection for cosmogenic-nuclide exposure dating of the bed of the Greenland Ice Sheet" by Jason P. Briner et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-264-AC2, 2022

Hi Greg, thanks for the detailed and insightful review, we really appreciate the time you took. Please note that some of our replies to the other referee's comments relate to some of your comments, so please check out both of our replies. Thanks again!

Review 2 comment: This paper is an extremely clear explanation of the reasoning behind how one would locate drill sites aimed at applying subglacial bedrock exposure dating to learn about Greenland Ice Sheet response to past climate warming. As it is the paper is quite clear and can be published in approximately its present form. It could be improved by adding some more sophisticated discussion in some sections, as described below.

One initial thing is that I would leave 'Greendrill' out of the title. It's a US-specific project name and it doesn't really make any sense to anyone who doesn't know about the project. Of course the project is described within the paper, but it makes the title unnecessarily incomprehensible before reading. Also, the reasoning described in the paper would apply to any subglacial drilling project in Greenland, not just this one. So I'd make the title more general to just indicate that the paper is about site selection criterial for subglacial bedrock recovery drilling.

Our reply: We agree and suggest instead *"Drill site selection for cosmogenic nuclide exposure dating of the bed beneath the Greenland Ice Sheet"*

Review 2 comment: Item 1. The one thing that is really missing in this paper that could be improved is a better discussion of assessing the sensitivity of potential observables at a drill site to ice sheet size and dynamics. The authors allude to this near line 277 with the statement that drill sites "should be robust monitors of past ice sheet margin change." However, this statement doesn't really mean anything by itself. From the rest of the paragraph I take this to mean that there should be a strong correlation between the ice thickness or ice margin position

at the drill site and some parameter of broader interest, for example total ice sheet volume or the position of the ice margin at a location of interest like a major outflow glacier. Drill site selection for this purpose in Antarctica has focused on this idea by using ice sheet models to develop theoretical transfer functions between overall ice sheet volume and ice thickness at a candidate drill site -- a good example of this is in Spector, 2018, Figure 2 (https://tc.copernicus.org/articles/12/2741/2018/tc-12-2741-2018.pdf), and this approach has been used in several other proposals and planned projects. This is valuable because it can both identify sites where the ice thickness at the site is likely to be related to ice volume, and also because it can identify sites where the response to expected variability or specific collapse scenarios is accessible within the design depth range of a particular drill system. This approach is also discussed in a simple way in the Schaefer et al. paper about the GISP2 bedrock, which uses model results to show that the majority of the ice sheet must be gone for the site to be exposed.

Our reply: We appreciate very much the expertise of this referee, thanks much for sharing your experience and years of thinking about these problems. First, we think that in Greenland, ice presence/absence at most drill site location scales with ice volume - and ice margin position. (Exceptions are perhaps in some peripheral mountains where increased precipitation during interglacials may lead to local ice survival during times when the main ice sheet experiences more negative mass balance.) Of course, however, the relationship between ice presence/absence at a particular site and ice volume may have different slopes (sensitivities) at different potential drill sites. As the referee points out, this relationship can be characterized with ice sheet models. We feel this is a bit beyond the scope of this "suitable sites" paper, but additionally, our group is exploring this and is presented in a companion paper that is currently in review elsewhere led by co-author Keisling. In that submitted paper, we explore what referee Balco is getting at, and we generate pixel-by-pixel map of the sensitivity that each potential drill site location is to the volume loss of the ice sheet as a whole. We have added references to the publicly available pre-print of this companion paper where appropriate in the text.

Review 2 comment: Unfortunately, the exact approach to transfer functions between ice thickness at a site and total ice volume that is in the Spector paper is probably not as useful in marginal areas of Greenland because of the ice margin geometry: in Antarctica, ice thickness variations are manifested as changes in the exposed height of steep-sided interior nunataks, but ice-free areas in Greenland are for the most part marginal horizontal plateau surfaces that are progressively exposed or covered by ice margin migration across a flat surface. Of course even in this geometry the extent of exposed land surface has to be related to the inland ice thickness in a general way, but it's much more complicated. It's also possible that the ice margin position on plateaus in certain colder areas might be related to local mass balance, so if mass balance is less positive during colder times, ice margin position at some places might even behave inversely to total ice volume. Figuring out the appropriate transfer function could be a hard problem for most likely Greenland drill sites, and it's really going to be the central challenge of interpreting whatever results are eventually revealed by the drilling project. I don't think developing this in detail is a necessity for publication of this paper, but I think it needs to be discussed in a more quantitative way.

Our reply: We agree that "developing this in detail is a necessity for publication of this paper." Discussing this in a more quantitative way is a next step, and is started in the Keisling et al companion paper that focuses on the ice-sheet modeling side of this.

Review 2 comment: Specifically, I suggest expanding this part of the paper into a separate section entitled 'Relating drilling results to overall ice sheet geometry,' or something of that nature, that contains the points that (i) one wants a drill site where some kind of transfer function can be established between the observable results and the overall condition of the ice sheet, and (ii) some ideas and geometric considerations for how to establish that transfer function.

Our reply: Great idea. Given our pending companion paper and its focus on this topic, we would rather pass on this particular suggestion, as great as it is, and instead leave it to our other paper.

Review 2 comment: The issue of whether or not ice domes could or could not occur on highlands separately from the ice sheet is also important in this context, and could also be discussed in a more quantitative way. For example, there are many highland plateaus in North Greenland that have ice domes on them, and many that do not. What is the difference? This is clearly important in understanding what happens at a drill site when the main ice sheet retreats. Is there a minimum size or elevation necessary to sustain an ice dome? Do they only occur in areas with particular temperature/precip conditions? Of course, this paper isn't going to completely do this analysis, but it could give some pointers for how to approach the problem.

Our reply: Another excellent point. When choosing sites that are inherently around the ice sheet periphery areas for our funded work, we performed some rudimentary analysis of equilibrium-line altitude patterns/gradients. Referee Balco is keen in observing that some peripheral mountains are glaciated, others not. One can use this pattern, along with topographic data, to show that ELAs rise inland. In selecting sites for our funded work, we projected these gradients inland using bedmachine elevation data and targeted ice-sheet bed sites below the local projected equilibrium line atltitudes. There are caveats of course, firstly it is today's ELA, not one of the past, and it ignores changes in precipitation patterns in a scenario with a reduced ice sheet. But, nevertheless, it points us in the right direction.

Toward the end of the section 4.4, we already include this text: *"Finally, the possibility that high-elevation areas remain glaciated by local ice after inland ice recedes should not be ignored. Many of the sub-ice drilling targets are >1000 m asl, near twentieth century snowline elevations."*

To expand a bit in light of the good points made by the referee, we propose tack on the following: *"To reduce the chances of drilling a site that is occupied by local ice once inland*

ice recedes, one could assess snowline elevation gradients using the presence/absence of ice caps in peripheral mountains along this coastline. Preliminary analysis shows that snowline elevations increase inland. Extrapolating these gradients to sub-ice areas suitable for drilling could help to guide drill site selection by identifying sites with elevations lower than projected snowline altitudes."

Review 2 comment: A final point in this area is that the paper could do a better job of articulating that an array of boreholes from one area is a lot better than one borehole. If you just have one borehole, the site was either exposed, or not, in the past. If you have an array of boreholes at different distances from the ice margin, you may be able to say where the ice margin was at a certain time, or at least establish the cumulative frequency distribution of ice margin extent.

Our reply: As the reviewer knows, this is the strategy in our funded work. However, while yes, the concept is absent from this manuscript, we point out that this paper is not a proposal to do the work, nor is it meant to outline the work that our co-author team is funded to do - it simply lays out the considerations for where one can drill using ASIG and Winke drills. We could fold in a recommendation for this strategy – but again that would just be our opinion for how to use our "suitable areas for drilling" findings in one's future scientific mission. Really those details should be left to groups who wish to drill and use the analysis in this paper to guide their work. In any case, we think there is a way to mention the power of a transect without it being necessarily "our recommendation"… We propose combining this in a paragraph where we also address the referee's other comment about the time trade-off with drilling few 700 m holes vs. many 100 m holes. So see our proposed new paragraph that incldues mention of a transect of drill sites farther down in this reply.

Review 2 comment: Item 2. Another aspect of the discussion that could be improved has to do with the importance of subsurface core vs. surface samples. This is briefly alluded to near line 232 and near line 260, but this doesn't clearly make the important point, which is that if you only have a surface sample you can't tell the difference between a short period of exposure at the surface and a long period of exposure beneath some layer of snow, ice, or rock cover. With a depth profile extending more than about a meter below the surface, you can tell the difference between these two things. Again, this is already described at length in lots of places (e.g., Schaefer 2016 again), so many folks should know it already, but this paper should make this point clearly.

Related to this point is that this paper needs to clarify the difference between drilling into bedrock and sediment. Again, this is alluded to in the line-260 region, but the text doesn't clearly state the important point, which is that if bedrock shows evidence of exposure, the exposure must have been at exactly that location. If you have sediment that shows evidence of exposure, the exposure could have taken place at a different location and the sediment transported (as is somewhere between possible and likely for the Camp Century samples). The paper should make this point clearly. Another related point, of course, is that if the sediment hasn't moved since the last exposure, it's equivalent to bedrock. This is likely relevant for some of the potential drill sites on plateaus that are probably always under ice divides with low flow velocity, so loose surface sediment or saprolite at these locations is possibly equivalent to

bedrock for exposure-dating purposes.

Our reply: This is an excellent comment. We agree that it is worthwhile to expand the bedrock vs. sediment discussion, and to clarify how bedrock cores are more advantageous than surface samples. We thank the referee for adding fresh eyes on this - and opening our eyes to this omission. In fact, when looking at our section "3.4 (now 3.5) Cosmogenic nuclides and subglacial geology", we found that it needs to be overhauled a bit to be better streamlined and reduce some redundancy. We propose adding new text to prior text to clarify bedrock vs. sediment:

"Sampling from a bedrock substrate has advantages over samples from sediment deposits, although cosmogenic nuclide measurements from both are informative. Sediments beneath ice sheets are more easily eroded, deformed, entrained, transported and re-deposited than bedrock. Thus, cosmogenic nuclide concentrations from the sediment grains themselves, which have a transport and deposition history, are more complicated to interpret than those in bedrock. Furthermore, cold-based ice that flows atop sediment sections can more easily erode a sediment surface (via entrainment processes) than in bedrock substrates. Thus, not only is a cosmogenic signal in sediments derived from each individual grain's exposure and burial elsewhere (that are later amalgamated into a single deposit), but the ice-bed itself may not represent a prior land "surface." Thus, sediment samples could be from an arbitrary depth below a paleo-surface. The depositional environment of sediment is also important. If ice overlies a fluvial sediment sequence, then the cosmogenic nuclide inventory is highly likely to have a complicated genesis, and thus a more complicated interpretation. On the other hand, if the sediment is saprolite or regolith, and largely formed in-situ, then its cosmogenic nuclide inventory likely would be more straight forward to interpret. In any case, for targeted sub-GrIS cosmogenic nuclide campaigns, the highest priority sites are those where non-erosive ice rests directly on quartz-bearing bedrock."

…and adding new text clarifying the advantage of bedrock depth profiles vs. surface samples only:

"Cosmogenic nuclide analyses made in a depth profile below the ice-bed interface yield important information. Measurements in a rock core spanning a meter or more, for example, can allow one to easily identify whether or not the current ice-bed interface has been eroded and/or covered by snow, ice or sediment for long durations (e.g. Schaefer et al., 2016). On the other hand, one cannot determine with surface-only samples whether a surface has been impacted by minor erosion and/or burial by snow, ice or sediment. Thus, analysis of bedrock cores is most important for elucidating ice sheet histories from cosmogenic-nuclide inventories. Furthermore, cores spanning several meters and including depths dominated by muon production have the added advantage of constraining orbital-scale term exhumation histories (e.g., Balter-Kennedy et al., 2021)."

Review 2 comment: Item 3. The section on 'Considerations for drilling' could benefit from some discussion of the time-depth tradeoff in drilling operations. In a field season you can (maybe) drill a couple of 700-meter holes, or a

significantly greater number of 100-m boreholes. So restricting sites to < 700 m ice thickness establishes feasibility, but not necessarily optimality.

Our reply: This is another good point. Is it the job of this author team and in this paper to advise how one choses to spend their field season? Maybe it is… We have added a new paragraph to the end of section 3.6 (the last section in the "considerations for drilling" part of the manuscript. Note this also includes the strategy of drilling a transect of sites.

"Finally, additional considerations relating to field season planning could lead to meeting scientific goals most efficiently. Multiple drill cores along transects (even including sites beyond the present – ephemeral – position of the ice-sheet margin) could boost confidence in constraining past ice-sheet dimensions through time. For example, a site that is presently covered by 100 m of ice may have been ice-free during the Holocene, whereas a 400-m-thick site farther inland was not; thus, one could better constrain the position of the ice margin during the middle Holocene. Additionally, using the ASIG Drill, there may be enough time in a field season to acquire one or two drill cores from thicker ice sites (e.g., 500-700 m), versus obtaining many drill cores in a single season from ~100m-thick sites using the Winke Drill. The optimal sampling strategy depends on several factors that relate to a particular scientific objective."

Review 2 comment: Item 4. Not related to the scientific merit of the paper, but I found the conclusions disappointing because they essentially restate motivational material that should have been in the introduction (e.g., all of 466-482). The discovery that subglacial exposure dating is an important thing to be doing is not an outcome of this work -- we knew it already, as evidenced by the fact that the GreenDrill proposal was successful. What the reader is hoping for here is more a statement of what was learned from the work done in the paper: e.g., the screening process described in the paper yielded a large number of candidate drill sites, but they are in fairly restricted areas of the ice sheet, and they present some geometric challenges to understanding the relationship between ice sheet size and drill site exposure. Following discussion above, I would probably highlight that the next real challenge is to establish how the exposure, or lack thereof, of the plateaus that are the most likely candidate drill sites is related to the broader condition of the ice sheet.

Our reply: Fair point. We removed the conclusion pre-amble that, as referee Balco points out, is a re-hash of the paper introduction. We propose leaving in, however, some of the other text. What might seem obvious (and maybe boring) to the reviewer (and maybe to our co-author team), it is actually a small subset of scientists who think about these things - we feel that the conclusion like that which currently exists would benefit students and an array of cryosphere scientists who haven't thought as much about this type of work (e.g., paleo, cosmogenic nuclide methods).

On the other hand, we appreciate the good ideas here about folding in the need for future work, largely ice-sheet modeling (some of which is ongoing), that when married to the CRN analysis from sub-glacial drilling, provide a much more complete picture of whole-ice sheet history and can likewise address "local ice problems." We sort of knew this in the background, example being our other companion paper by Keisling and knowing of course

that we have modeling in our project, but yes we omitted this from this paper... We would add the following to the conclusion as a new paragraph:

"Pairing sub-ice cosmogenic-nuclide analysis with ice-sheet modeling is an important step (Spector et al., 2018). Ice-sheet model simulations have the ability to scale information from single drill sites, or transects of sites, to the entire GrIS. Likewise, results from ice-sheet modeling can help identify which potential drill sites are most sensitive to overall ice sheet mass balance (e.g., Keisling et al., 2022), thus help to prioritize sites or to assemble a strategically chosen group of sites. Finally, high resolution ice-sheet models with fine meshes in areas of peripheral mountainous topography could help with 'local ice survival' issues that could complicate cosmogenic-nuclide records from areas where alpine topography is smothered by the GrIS."

Review 2 comment: Other than that, it's great. I enjoyed reading it and the figures are excellent, except that Figure 3 should be bigger. Just because of the nature of the result the reader is trying to look at small areas in a big ice sheet. Use a whole page for this figure. Otherwise, great job with the figures.

Our reply: Agree, bigger figure 3.

Review 2 comment: Additional minor items:

Lines 66-79. This section could be greatly simplified simply by noting that marine records are indirect evidence no matter what. The only direct evidence that an ice sheet was not in a certain place is evidence from under the ice sheet that something else was there.

Our reply: Referee 1 asked for clarity on direct v. indirect evidence of past ice sheet change. We opt not to remove text from this paragraph, but do clarify some language, as stated in response to Ref 1 comments, we would now write: *"Although alternative histories are possible, the results lead to an important conclusion: an almost entirely absent ice sheet in Greenland within the last 1.1 Myr. Furthermore, these types of data directly constrain past ice-sheet configurations, unlike marine sediment records from adjacent seas that provide indirect evidence."*

Review 2 comment: Line 101. 'Likely' is incorrect. The Schaefer et al. data require surface exposure no earlier than 1.1 Ma.

Our reply: Thank you, will make change.