



EGUsphere, referee comment RC2
<https://doi.org/10.5194/egusphere-2022-264-RC2>, 2022
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Comment on egusphere-2022-264

Greg Balco (Referee)

Referee comment on "Drill-site selection for cosmogenic-nuclide exposure dating of the bed of the Greenland Ice Sheet" by Jason P. Briner et al., EGU sphere,
<https://doi.org/10.5194/egusphere-2022-264-RC2>, 2022

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 criteria for subglacial bedrock recovery drilling.

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.

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. 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.

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.

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.

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.

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.

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.

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.

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.

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