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Discussions

Interactive comment on "West Antarctic sites for subglacial drilling to test for past ice-sheet collapse" *by* Perry Spector et al.

Perry Spector et al.

spectorp@gmail.com

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We thank Nathaniel Lifton for his constructive comments that have improved the manuscript. Comments from the reviewer (bold) and our responses follow. We will upload the new version of the manuscript and supplement separately.

GENERAL COMMENTS

This manuscript presents criteria for evaluating sites for subglacial drilling to evaluate significant past ice sheet thinning beyond current conditions, specifically in West Antarctica but which would also applicable to other locations. This is an emerging field as new drilling technologies and capabilities appear

for remote, logistically challenging, and environmentally sensitive applications in Antarctica and other ice-covered regions. Spector et al. do an extremely thorough job in this respect, in my opinion, beginning with description and analysis of ice-sheet modeling results and moving on to detailed site-specific criteria for consideration. In my view this is an impressive, well-written manuscript - it is extremely clear and concise but with considerable detail and similarly clear figures, providing a very useful framework for those in the community considering similar projects. I only had a few minor comments, below, but easily recommend acceptance with minor revision. I'm pleased to say that is one of the best manuscripts I've read in a while.

Nat Lifton

We thank Nathaniel Lifton for his support of the manucript, and we have addressed his specific comments as described below.

SPECIFIC COMMENTS

Pg 12, Ln 15: Concerning the last sentence, if the ridge is oriented subparallel to the wind direction then even the ridge crests might also be affected by wind scoops and snow aprons, so it would be safest to avoid ridges in that orientation.

In this paragraph we have attempted to provide drill-site considerations that are generally applicable to alpine landscapes found in West Antarctica. As mentioned in the second paragraph of Section 5.3, an important point is that wind scoops and snow aprons are only a potential problem for deglaciations in which the ice surface is brought within a few tens of meters of a ridge crest, which is similar to the typical dimensions of wind scoops and snow aprons. These issues will probably be irrelevant for most deglaciations in which the ice surface is more than a few tens of meters above or below a ridgecrest. We maintain that even in the case of a ridge oriented parallel to the prevailing winds, the crest will have greater immunity to these problems than the flanks.

Pg 12, Ln 24: Figure 7 is a clearer demonstration of the asymmetry in my opinion

Updated to "As shown in Figure 7..."

Pg 13, Ln 8: How long is the core?

Updated to: "An 8 m subglacial bedrock core..."

Pg 14, Ln 10: I would argue that that the LSDn scaling model best explains global production rates overall, and should be used instead of Lal (okay to present Lal also, though). Also, the ERA-40 reanalysis gives very similar pressure results to those in Stone (2000). Muon production should be modeled following Balco (2017) Production rate calculations for cosmic-ray-muon-produced 10Be and 26Al benchmarked against geological calibration data. Quaternary Geochronology 39, 150–173. doi:10.1016/j.quageo.2017.02.001

We have switched our production-rate calculations to use LSDn scaling and the method of Balco (2017) for muon production. We are still using the relationship between elevation and Antarctic air pressure of Stone (2000).

Fig 4: A range of prevailing wind directions is shown in 4a, but only a single direction in 4b - seems like it should be a range as well.

We thank the reviewer for this careful observation. The difference between the wind-direction indicators in the two figures is actually purposeful. In this region of Antarctica, the winds generally flow from the southwest. The range shown in Figure 4a reflects the fact that the winds are forced to flow around the Pirrit Hills massif, and so the orientation of the prevailing winds is not uniform in the region, but varies with location around the mountains. At a given location, the winds are relatively constant in direction; hence for Figure 4b we used a single vector that is representative of the wind direction in the domain of the map. We have updated the figure caption to clarify this.

Figure 6: This plot shows the ratios of (N*lambda)/P (or equivalently, N/(P*tau)), not just N/P as stated in the caption (N/P would look similar to the more typical curved two-isotope plot but with the 26/10 axis scaled from 0-1). The text in the caption should be changed to correct this.

We believe that this comment is incorrect. The concentrations shown in this figure are, in fact, normalized to surface production rates (N/P). We do not use N*lambda/P, as indicated by the reviewer. The 26/10 axis is scaled from 0-1 (in the figure it is shown from 0.2-1 to focus on the area of interest). The X-axis is linear, which causes the simple exposure line and the contours of exposure and ice cover to be straight lines. Two-nuclide diagrams in other publications are sometimes presented with the X-axis on a log scale, which causes these lines to be curved. This is likely the root of the confusion.

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