

Reply to review 2 (Anonymous) of ‘Detecting Holocene retreat and readvance in the Amundsen Sea sector of Antarctica: assessing the suitability of sites near Pine Island Glacier for subglacial bedrock drilling’ by Johnson et al.

Thank you very much to Reviewer 2 for their helpful comments, which pose some interesting additional questions to Reviewer 1. Here we respond (in **blue**) to each comment (reproduced in *black italics* below) in order and then provide a list of proposed revisions (in **bold**) that address the points raised.

This is a valuable contribution for anyone planning to access subglacial bedrock through shallow ice ($\leq 100\text{m}$); that is anyone who is interested in quantifying if an ice margin had previously been retreated beyond than the present extent. The manuscript presents a structured account of the steps taken to select a suitable drilling site in the Amundsen Sea sector of West Antarctica. As such this manuscript is more of a technical report than a scientific contribution with associated results that address the underlying aim of “unambiguous identification of past episodes of ice sheet thinning”. What is missing is what the authors learned from their process and how they would improve it for future site selection. The manuscript is well written, and the figures are excellent.

Thank you for these positive comments. We appreciate the desire for a description of what we learned about site selection by undertaking this study; this point was also made by both other reviewers. We therefore propose to amend the Conclusions (section 5) to incorporate what we have learned during the process.

Detailed comments:

The two-part title “Detecting Holocene retreat and advance in the Amundsen Sea sector of Antarctica: assessing the suitability of sites near Pine Island Glacier for subglacial bedrock drilling” suggests that the manuscript will provide new data on detecting Holocene retreat and advance in the Amundsen Sea sector of Antarctica, but the manuscript is focused on the second part, “Assessing the suitability of sites near Pine Island Glacier for subglacial bedrock drilling”. This would be a more correct and informative title.

We agree that the two-part title does not completely reflect what is in the paper and will amend the title of the revised manuscript accordingly.

The introduction gives a thorough account of the need for subglacial bedrock drilling, contextualising this into the wider issue of improving our understanding of West Antarctic ice sheet change from the mid-Holocene to the onset of satellite observations.

Section 2 starts by reiterating the limited number of projects that have successfully retrieved subglacial bedrock and measured cosmogenic nuclides in those, before introducing the Hudson mountains field area.

Section 3 describes the approach and criteria used for choosing a suitable drill site. One paragraph is informatively spent on explaining the need for certain lithologies, and that new mineral systems are being explored by several research groups to increase the utility of measuring in-situ ^{14}C and ^{10}Be . The section on drilling and the potential problem of fluid leakage is interesting but I would have valued a section on how to avoid leaky permeable firn. Can it be identified in GPR data?

As we discussed above in the response to review 1, our mention of permeable firn here is slightly misleading and gives the impression that there could be an undetected permeable firn layer at the bottom of an ice borehole. In fact, this is not the case, and whether or not there is permeable firn at the base of the borehole just depends on whether the drilling depth is greater or less than the local firn-ice transition depth. As mentioned in our response to Review 1, we will add some text to clarify this.

Section 4 begins by highlighting the importance of understanding the Holocene exposure history based on fieldwork and surface exposure ages. This could be followed by Section 4.1.3 which focuses on identifying potential suitable ridges using remote sensing. Table 1 (Section 4.5.1) shows that sites without ridges can be excluded as suitable sites. With respect to the manuscript, further mention of the non-suitable sites is unnecessary.

We agree it would improve the flow of the paper to have the section on nunatak topography (section 4.1.3 of the original manuscript) before the section on modelling (section 4.1.2 of the original manuscript). We can change this in the revised version. We agree that Table 1 contains information that is unnecessary at this stage of the manuscript, and potentially confusing (see also Review 1); to address this issue, we propose to follow the suggestion of Reviewer 1 to describe our initial filter and then focus the subsequent discussion only on the sites that pass that filter, as well as removing the non-suitable sites from Table 1.

Section 4.2 provides a nice example of the value of using ice sheet modelling to estimate grounding line retreat and how this changes the orientation of bedrock ridges relative to ice flow, or that some nunataks become islands and are therefore unlikely to retain a Holocene record.

Thank you for this positive feedback.

Section 4.2 is a detailed account of implementing the procedures developed throughout the manuscript at Winkie, Webber, and Maish nunataks. Each nunatak is treated in informative detail. The thin sections were revealing because they demonstrate how difficult it will be to retrieve sufficient material suitable for analyses.

We are pleased that the reviewer found sections 4.1.2 (modelling) and 4.2 (description of observations from Winkie, Webber and Maish Nunataks) informative. In particular, it is good to know that the thin section photographs and associated descriptions are useful and that the level of detail we provided on those is appropriate.

The manuscript is all about site selection. It would have been valuable for the authors to reflect on their selection process and what they have learned that might improve future site selection.

This point was made also by both other reviewers. We agree it would be a valuable addition to the manuscript to include a section on what we learned and propose to include this in the revised manuscript.

We propose the following revisions to address Reviewer 2's comments:

- 1. Amend manuscript title [also suggested by Reviewer 1]**
- 2. Move section 4.1.3 (Nunatak topography) to immediately before the modelling (section 4.1.2).**

- 3. Section 4.1.5: describe our initial filter for site suitability and amend Table 1 accordingly. [this also addresses a comment from Reviewer 1]**
- 4. Add a paragraph (in section 5) summarising what we have learned about site selection to help future project planning [this also address Reviews 1 and 3].**