Reply to review 3 (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 3 for their constructive comments, which complement those of the other two Reviewers and ask for more clarity on a few further minor points. 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 very well written and well structured manuscript, and I found little to criticise beyond the few points already raised in the review by Jon Harbor. I generally agree with the points raised by JH, in particularly with regard to including a focus on more general lessons learned about site suitability assessment from this and previous campaigns, perhaps in a new section 5.

Thank you for this feedback. In response to the comment here, which was also made by the other reviewers, we propose to provide an additional paragraph (in a new section 5 or in the Conclusions) outlining what we have learned about drill site selection through this process.

Minor comments

l. 108-110: For the field site presented in this study, grounding line retreat is the most likely cause of ice-sheet thinning, but perphaps mass-balance changes could drive thinning elsewhere in Antarctica? Since this section describes the problem in general terms, I am wondering if it should be presented more broadly. Also, are these criteria exhaustive? E.g. should criterias on subglacial topography/ice thickness (vs. feasible ice-drilling depths/realistic thinning depths), or surface stability/cover during subaerial exposure, be included? Maybe they are implicit in criteria i) and ii), if so, these criteria could be explained/expanded on as is the case for criteria iii) lithology and Iv) accesibility.

In the revised manuscript, to make this section more broadly applicable as suggested in this comment, we will add in mass-balance changes as a potential driver of thinning at sites elsewhere in Antarctica. However, it is important to realise that exposure dating of subglacial bedrock detects ice sheet thinning, but not the *cause* of that thinning. Without other data and/or modelling, the drivers can only be inferred. In response to the second part of this comment, the four criteria we listed are those which must all be met in order for a site to be suitable for subglacial drilling as a means of detecting Holocene readvance. The other factors the reviewer mentions above are desirable, not essential, which is why we have not listed them in the manuscript. As this section is designed to be a short introduction to our approach rather than an exhaustive exploration of possible scenarios (which might also confuse some readers), we do not propose to add any material here.

L. 224-226: Elaborate what the problem with an island is. I assume that you mean that bedrock on flanks/ridges goes from subglacial to submarine rather than becoming subaerially exposed. But becoming an island isn't in itself a problem as long as parts of the formerly subglacial section of a ridge line becomes subaerially exposed, i.e. if the sea level is lower than the ice level.

We agree this is unclear as written. The reviewer is correct that, in terms of finding a suitable site for detecting Holocene ice sheet readvance, we require a site where any grounding line retreat inland of present would have resulted in (subaerial) exposure of flanks or ridges that

were previously under the ice (subglacial); thus, becoming an island is not necessarily a problem as long as the site fulfils this requirement. We can clarify this in the revised version of the manuscript.

L. 271: In the text, Table 1 is presented as providing information from pre-field survey, but in reality it contains both pre, in-, and post field information. I found this confusing.

Thank you for pointing this out – we agree it is confusing and incorrect. We propose to amend line 271 in the revised manuscript to make it clear what the table contains.

Section 4.2.3. Due to accessibility problems Maish Nunatak was not visited in the field to perform a site suitability assessment. I therefore wonder if this section could be cut from the manuscript, and replaced by a note of the accesibility issue in e.g., the beginning of section 4.2. Information on bedrock lithology and glaciology seems unessential once it becomes clear that the site isn't feasible to access, but details could be moved to a supplement.

There seems to be a misunderstanding here – we did visit Maish Nunatak (as mentioned in line 484 of the original manuscript), but a GPR survey was not undertaken there due to the challenges of surveying where there is extensive surface melting creating large and often water-filled potholes in the ice sheet surface (see Fig. 14). Thus, the lithology, presence of erratics, ridge accessibility and above-ice topography at Maish Nunatak were all assessed, but the subglacial ridge topography or conditions were not determined. To avoid any misunderstanding for readers, we propose to rephrase accordingly lines 526-7 (which we suspect are those that have caused the confusion) in the revised version.

Based on our field observations, in particular the challenges of inputting a field camp and deploying heavy drill equipment where there is extensive surface melting of the ice sheet, we quickly decided during the reconnaissance field season that Maish Nunatak was not going to be a high priority for our drilling campaign because helicopters would definitely not be available to us (helicopters would enable equipment to be deployed directly onto the nunatak, thus negating the need for input to a more distant site via ski-equipped fixed-wing aircraft and subsequent transport to the nunatak via snowmobile/sledge). However, Maish Nunatak could have been a feasible drill site if helicopter-support had been planned, and indeed it could be a suitable site for a future campaign. In the revised version of our manuscript, we therefore propose to keep this section in the main text. We do, however, appreciate that the overall manuscript could be shortened in places. To address this, we propose to slim section 4.2 by being more concise with our use of language (see our reply to Reviewer 1), and by combining sections 1 and 2 into a single shorter section.

Figures

Figs. 9+13. Add approximate scale on photos or in captions

We can add it in the revised version of the manuscript.

Fig. 7+11: Specify that the field of view in panels c (PPL) and d (XPL) is the same (i.e., not just the same rock).

We can add it in the revised version of the manuscript.

We propose the following revisions to address Reviewer 3's comments:

1. **Add "mass balance changes" to point (i) in line 108.**
2. **Clarify the circumstances under which grounding line retreat resulting in an island at Evans Knoll would affect its suitability as a drill site (by amending/adding text around lines 225-226).**
3. **Amend line 271 to clarify what information Table 1 contains (i.e. not just data collected prior to fieldwork).**
4. **Shorten section 4.2 by using more precise language [this also addresses Review 1].**
5. **Rephrase lines 526-7 to more clearly state that Maish Nunatak was visited in 2019-20 and some aspects of the site's suitability assessed, but that it was not possible to carry out a GPR survey.**
6. **Add scale to Figs 9 and 13**
7. **Specify that field of view in Figs 7 and 11 c and d is identical.**
8. **Add a paragraph (in section 5) summarising what we have learned about site selection to help future project planning [this also addresses Reviews 1 and 2].**