Reply to review 1 (Jonathan Harbor) 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 1 (Jonathan Harbor) for his thorough and supportive review. Overall, he is very positive about the manuscript, and most of his comments suggest improvements to its structure and clarity rather than questioning the scientific content. Here we respond (in blue) to each of those comments (reproduced in *black italics* below) in order and then provide a list of proposed revisions (in **bold**) that address the points raised.

Review Criteria:

Does the paper address relevant scientific questions within the scope of TC? Yes

Does the paper present novel concepts, ideas, tools, or data? Yes, but…. This is largely a technical report aimed at selecting a specific drilling site, which they do very well. For the larger scientific audience, what would make this much more compelling is more insight into what they learned about site suitability assessment, and how this might be done in the future. That is the broader significance of this, in the absence of drilling results.

Are substantial conclusions reached? Yes, but see detailed comments.

Are the scientific methods and assumptions valid and clearly outlined? Yes

Are the results sufficient to support the interpretations and conclusions? Yes

Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? Yes

Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Yes

Does the title clearly reflect the contents of the paper? Largely

Does the abstract provide a concise and complete summary? Yes, but see detail comments

Is the overall presentation well-structured and clear? Largely – see suggestion for a larger methodological overview.

Is the language fluent and precise? Yes, very well written

Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Yes

Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? See detailed comments about potential use of supplementary data.

Are the number and quality of references appropriate? Yes

Is the amount and quality of supplementary material appropriate? See detailed comments about potential use of supplementary data for extensive field data.

We are pleased that the reviewer considers our paper largely meets the review criteria. With regard to his point about providing more insight into lessons learned about assessing site suitability – which was also raised by Reviewers 2 and 3 – we agree that adding a paragraph to the main text and modifying the abstract (the latter to address the detailed comment by this

reviewer, below) to include this information would help inform future drilling efforts. Thus, we propose to add these in the revised version. We have responded to the comments about a large methodological overview and using supplementary data in our replies below.

Detailed Comments:

Abstract is clear, and presents the context, rationale, and key results well. For the larger audience, the particular site that is selected is less important than the method proposed and used to select suitable site(s). The abstract might be modified to say something about what was learned from the process that might inform changes or improvements in future site selection work by the authors or others.

We agree modifying the abstract would be useful and therefore propose to amend it in the revised version.

Introduction provides a clear overview of the importance of subglacial drilling in determining if/when an ice sheet was ever smaller in the recent geological past than at present. It also makes a compelling case for the importance of this knowledge in Antarctic science. Relevant past work in subglacial drilling is well described, as is the broader significance of Antarctic ice sheet change on global sea level. This work is placed in context of other related studies, in particular the work at Mt. Murphy (Balco et al., 2023).

Although I appreciate the effort taken to make clear the significance of the larger scientific work to which this project intends to contribute, and the importance of the study region, the unique contribution of the paper (as indicated by the title and the abstract) is the approach used to assess site suitability. This is not reflected in the balance between describing the larger scientific context of the work and describing the background and past work around site selection. It's almost as if the authors start writing the paper about the results that should have come from the analysis of the core samples, but then focus on the site selection (given that they were unfortunately unsuccessful in obtaining core samples due to due to factors unrelated to the choice of drill site). While I appreciate the importance of the detailed background for the intended results of the drilling, I suggest that the authors make this a shorter more focused discussion of what was learned from the site selection process. To guide future work by them or others in site selection.

We agree that the unique contribution of our paper is the approach used to assess suitability of sites for subglacial bedrock drilling. This comment could be addressed by moving parts of the drilling description section to a Supplement. However, Reviewer 2 comments positively on these sections and we feel that some of this information is needed to set the scene about why we were attempting subglacial bedrock recovery in this area in the first place. Nevertheless, we appreciate that the balance of the paper would be improved by shortening sections 1 and 2. We therefore propose to combine those sections to create a single shorter, more focused introductory section that places less emphasis on why the Hudson Mountains are an important place to drill.

Section 3 provides a clear explanation of the key criteria used in selecting a drilling site, based on prior work and a nice example (Figure 2) from Mt. Murphy, Balco et al., 2023. The discussion around acceptable lithologies for in situ 14C measurements is particularly helpful for those less familiar with recent advances for non-quartz- bearing bedrock lithologies. The discussion of technical factors is helpful, in particular that "the presence of permeable firn near

the base of a shallow borehole or crevassing extending near the bed could potentially allow fluid loss and preclude coring". However, I don't recall seeing later in the paper how your approach evaluated the possible presence of permeable firn near the base of a shallow borehole.

Our sentence alluding to the presence of permeable firn is, on further consideration, slightly misleading here. The presence or absence of firn is purely related to the depth below the ice sheet surface: the fundamental process of glacier ice formation is that accumulating firn is gradually compressed to form ice, so there is some depth at which the firn-ice transition occurs and firn is not present below that depth. The firn-ice transition depth varies with local climate: for example, at the relatively warm Mt Murphy site ~350 km WSW of the Hudson Mountains (see Fig. 1a and Balco et al., 2023: https://doi.org/10.5194/tc-17-1787-2023), the transition was at about 15 m depth, but it will be deeper at colder, higher-elevation sites (reaching 100-140 m in places; Ligtenberg et al., 2011: https://doi.org/10.5194/tc-5-809-2011). Thus, whether firn is present at the bed is just dependent on what the local firn-ice transition depth is; firn is possible near the bed when drilling at depths near or less than the transition depth, and unlikely to impossible when drilling at greater depths. At the low-elevation sites described in the present paper, we expect that the transition depth is similar to that observed at Mt Murphy, in the 15-20 m range.

In terms of evaluating the possible presence of firn near the base of a borehole, this can be revealed by measuring the density profile of the ice column: a gradual increase in density would be expected down core in a dry snowpack or, if water is percolating and refreezing, there might be high density layers where water has refrozen, as well as a shallow firn-ice transition depth. Density can be calculated using GPR measurements using a technique known as Wide Angle Reflection and Refraction (WARR), where the antennae are moved progressively further apart from a central point. However, to work well, WARR requires horizontal layered structure, significant density changes at strong subsurface reflectors, no off-line reflectors (for example, from bedrock) and no distortion of layers by crevassing or flow over a ridge. Sites with potential for shallow drilling (such as those in the Hudson Mountains) are, therefore, not ideal for WARR surveys and accordingly we did not undertake such surveys as part of our study. However, applying WARR could be done in future, and the results might indicate if there is firn present at or near the subglacial bedrock surface or whether the base of the borehole is in solid glacier ice. If encountered, the presence of permeable firn at the base of a shallow borehole does not necessarily pose a problem for subglacial bedrock drilling since various technological interventions (such as using a separate electro-mechanical access drill and casing the borehole; see Braddock et al., 2024: https://doi.org/10.1017/aog.2024.12) have been developed to deal with it.

For improved clarity in the text, we propose to (i) remove the allusion to firn in the sentence at lines 139-140 (because this sentence is really about unexpected ice fracturing near the bed), and (ii) add a short discussion of the expected firn-ice transition depth. This should address the reviewer's comment and clarify the issue.

4: I think we are missing a short paragraph that provides an overview of the entire process that is used to evaluate the potential sites – that introduces that there is initial work done (4.1) that then drives the information collected during the field survey (4.2), and that this then leads to the final selection (which the authors have in 4.2.4, but could be renumbered 4.3). This will give readers the big picture of what readers will then experience as they progress through the rest of the sections. Perhaps even a flow chart.

This is a great idea, thank you. In the revised version, we can add text or a flow chart at the beginning of section 4 to provide an overview for readers of the whole process. As suggested, we will also include the relevant section numbers to guide readers through the rest of the paper.

Section 4.1.1 makes clear the importance of prior work on Holocene exposure history, and section 4.1.2 is particularly important in showing how ice sheet modelling is key in helping us consider how potential drilling sites would have responded to past change by looking at potential future change. While the use of potential future change has several advantages, there should be some discussion of why the authors did not model the past periods of change that they are seeking to reconstruct and understand. I appreciate the candor in discussing the current spatial resolution limitations of modeling at the scale of a set of nunataks. What stood out as particularly important was the ability to look at whether the grounding line retreat directions changed relative to the ridge orientation, and the way in which sites might become "islands" rather than exposed.

We did not model periods of past change but instead chose to use a model simulating future change because, to our knowledge, no palaeo-models have yet simulated grounding line retreat inland of present in this region under any reasonable conditions (see Johnson et al., 2021: https://doi.org/10.1029/2020JF005827). Thus, there were no palaeo model outputs available to us to guide our choice of sites for drilling. New modelling is beyond the scope of our study but would definitely be of value for similar studies if undertaken in future. To address this comment, we propose to add sentences in section 4.1.2 to explain these points.

4.1.3 might more logically come after 4.1.1. Rather than judging all sites against all criteria, it is clear from table 1 that once a site failed the ridge test, the rest of the criteria became N/A. So perhaps this can be described this as an initial filter. Then the rest of the discussion can focus on the sites that passed this initial filter (and exclude the others from table 1).

Reviewer 2 also mentioned the position of section 4.1.3 but suggested an alternative location for it immediately before the modelling (section 4.1.2). In the light of these comments, we propose to move section 4.1.3 to before section 4.1.2 because it then sets up the idea of the exposure of ridges being sensitive to grounding line retreat, which we then test against model outputs. We can make this change in the revised version of the manuscript.

Regarding Table 1, we like the reviewer's suggestion to describe an initial filter and remove the sites that don't pass this filter from the table and subsequent discussion. For the revised version, we can explain this approach in section 4.1.5 and amend Table 1 accordingly.

4.1.5 It is very helpful to have this summary to provide the logic for the focus of the fieldwork.

This is good to hear!

4.2. The authors provide a very complete description of the field data and results of the analyses on samples collected for the three candidate drill sites. This is important information that should be available to the wider scientific community. But I struggled with the level of detail - if the purpose of the paper is to advance understanding of a process for drilling site suitability assessment, then it is too much (a subset should be in the main paper, and if we want a comprehensive report out, put it in supplementary data). If the purpose is defined as

also to provide a repository of all the data collected (which is valuable) then the current structure works fine.

The purpose of our paper is to provide both a process to follow for assessing drill site suitability *and* to present all the observations and data we collected because they will likely be useful in future (particularly given that we have not yet been able to successfully recover bedrock from close to Pine Island Glacier and the Hudson Mountains thus remain a desired target for future drilling). We therefore think this level of detail is appropriate and should remain in the main text. In order to make the overall paper more focused, we propose to make the text more concise across all of section 4.2, in addition to combining sections 1 and 2 to make a single shorter introductory section as described above.

4.2.4 This is more than a summary of the results; it is also then a selection of the best site – so I'd label this 4.3 and have the title also indicate "site selection".

Thanks for this suggestion – we agree. To address this and your earlier comment, we propose to renumber section 4.2.4 of our revised manuscript as section 4.3 and give it the new title of "Final selection of drill site" or similar.

5. Conclusion. Although to the project team the selection of the best site for drilling is a key deliverable, for the larger audience reading this paper what is more important is what the team also learned about assessing site suitability. What lessons were learned, what would the authors suggest that others do the same or differently in future drilling site suitability assessments for this type of scientific work, what are areas for improvement (eg in the modeling)?

Thank you for making this point. We do want our paper to be of broad interest as you suggest. We can include a summary of lessons learned/suggestions for improvement in section 5 in the revised manuscript.

The postscript provides helpful context as the reader is likely to be keenly interested in knowing how the coring went and about any results. I can only imagine the frustration with this outcome – field work in challenging environments is tough, and we often don't report the many cases where things did not work out.

Thank you for appreciating the challenges we encountered with drilling in this region, and for valuing the importance of reporting on endeavours even when things have not gone to plan.

<u>We propose the following revisions to address Reviewer 1's comments:</u>

1. **Amend the title to more accurately reflect what is in the paper [also suggested by Reviewer 2]**
2. **Combine, and overall shorten, sections 1 and 2 (Introduction and Aims of drilling at PIG).**
3. **Remove the allusion to firn in the sentence at lines 139-140 and add a short discussion of the expected firn-ice transition depth.**
4. **Add text or a flow chart at the beginning of section 4 to describe the whole process of choosing a drill site and include section numbers to help readers find the description of each section in the later text.**

5. **Move section 4.1.3 (Nunatak topography) to immediately before the modelling (section 4.1.2) [this also addresses comment by Reviewer 2]**
6. **Add text to section 4.1.2 to explain that new modelling was beyond the scope of our study and that no published modelling has yet simulated a Holocene retreat inland of present in this region, so we had to use future simulations as analogy for a smaller than present Holocene ice sheet configuration.**
7. **Section 4.1.5: describe our initial filter for site suitability and remove sites that do not pass that filter from Table 1. [this also addresses a comment from Reviewer 2]**
8. **Shorten section 4.2.**
9. **Change title and numbering of section 4.2.4 to "4.3 Final selection of drill site" (or similar).**
10. **Add a paragraph (in section 5) summarising what we have learned about site selection to help with future project planning [this also address Reviews 2 and 3]. Modify the abstract accordingly.**