Point-by-point responses to Reviewer 1

This paper is missing reference to and discussion of relevant long-term terrestrial archives that extend through at least one glacial-interglacial cycle in the TAM. These would be valuable to add to the Introduction and/or Discussion section (e.g., Bader et al., 2017; Bergelin et al., 2022; Bibby et al., 2016; Kaplan et al., 2017). See links to references below. I recommend also stretching the disciplinary boundaries by briefly considering the implications of your work for life in extreme environments (e.g., Dragone et al., 2021 & 2022).

We have modified the Introduction (Lines 88-104) to incorporate these pertinent studies and also have adjusted the Discussion (Lines 392-406) as suggested.

Lines 123-126 now include reference to the potential sterility of these soils as a salient characteristic of high, dry polar environments.

Line 83-84. As written, this implies that the Sirius Group was deposited at the same everywhere, which may not be the case. Refine the text here.

We refined the text on Line 83 to highlight the potentially broad temporal distribution of wet-based tills associated with the Sirius Group.

Lines 86-90. This section, in particular, requires more nuance in the discussion of polar conditions and cold-based ice. I read this as referring to most of East Antarctica or at least the TAM, not just your field site, and in that context, the text is misleading. It isn't true that cold polar conditions at the surface always produces cold-based ice. We know that much of the EAIS has water at its bed (e.g., Siegert et al., 2007; Wright et al., 2017) and at moraines adjacent to your field site, there is geochemical evidence of warm-based basal entrainment (Graly et al., 2018). This doesn't mean I think the deposits at Otway are warm-based, just that the text about this is not clear about local/thin ice vs the ice sheet more broadly. Avoid overgeneralizations.

We have modified the text in the paragraph spanning Lines 88-104 as suggested by the reviewer, with the objective of providing a more nuanced, less generalised description of cold-based regimes in the Transantarctic Mountains.

Lines 94-96. As presented, this sets up a false dichotomy as regions being compared have several major differences including elevation, distance to the coast, uplift history.

The revised text between Lines 98 and 104 to reflect the environmental variability highlighted by the reviewer.

Line 120-121. What is the adjacent ice thickness? Can you provide a bed/ice surface topographic profile and comment on what conditions leads to the outstanding preservation at this location? This would add value in identifying other sites that may provide complementary long-term records.

Lines 127-131 in the revised manuscript give improved geographical context for Otway massif and describe how the ice-landscape configuration potentially fosters moraine preservation in such topographies.

Line 310. "any exposure age on a moraine that is older (younger) than ALL exposure ages on a stratigraphically older (younger) landform" Have you statistically evaluated what the minimum/ideal value of 'All' must be? I presume one isn't enough – is two? Three? Clarify this point.

The revised paragraph between Lines 317 and 331 provides improved detail on our statistical treatment (and justification) of moraine ages and outlier identification as suggested by the reviewer.

Paragraph beginning line 322 and related to paragraph beginning Line 385. Three sandstones were analyzed with paired nuclides and all three showed a complex exposure history and/or erosion. I don't see three double-dated samples on Fig. 4 – only 2 pairs of pink/blue dots are visible. With these data in hand, it seems reasonable that ALL samples analyzed have a complex exposure history and/or erosion. Why would only sandstones suffer this fate? If erosion is the culprit, should the takeaway be that in 'old' glacial deposits, sandstones should not be used for surface exposure dating in these kinds of settings? Are they always unreliable beyond X exposure time? This wasn't the finding in Balter-Kennedy, so why might these two adjacent settings produce difference results? Alternatively, if the culprit is complex burial history, then why would the sandstones be the only rocks affected? This seems to indicate that all the rocks analyzed should be interpreted as having a complex burial history. This requires more explanation/discussion.

The reviewer identified an apparent omission from Figure 4; the blue symbol, being an outlier in this instance, was indicated in a manner that was not clearly visible. The revised figure retains this symbol, but we have made it bolder and also identified it in the figure caption.

We have modified significantly the text between Lines 336 and 355, which reports the three sandstone clasts and introduces the potential complications of complex exposure. We clarify that erosion is always a possibility for non-normal distributions and highlight that whatever is impacting the three sandstones might also have impacted adjacent dolerites, but also provide a justification for not simply assuming that this is the case (statistically and geologically). The revised manuscript attempts to bring these challenges to Antarctic chronology into the open for a fully transparent discussion.

Lines 375-376. Newer erosion/exhumation data from this region (He et al., 2021) shows the maximum incision of Beardmore substantially predates these deposits, peaking in the late Eocene and mostly complete by the end of the Oligocene. Re-evaluate landscape evolution with these data in mind.

We have adjusted the text on Lines 413-415 (and the paragraph generally) to place this selective linear erosion model into better context.

Figure 2. Acknowledge the source of imagery.

Figure 2 now includes the correct source information for the satellite imagery.

Figure 4. See note about missing data for MOG. For MON – using the mean doesn't follow your criteria #1 as there is no 'main' population. Revise this figure and related text to clarify that the 'mean' landform cannot be determined.

Figure 4 has been revised accordingly and no longer includes mean values.

Figure 6. The gray shading in the background doesn't make sense. There are places where error bars overlap, yet there is no gray shading (mostly older than 9 Ma). In the caption specify 'error' bars (so it's not confused with histogram bars).

Both the figure and the caption have been modified as per the reviewers comments. Where older data points do not correspond to vertical grey bars, this is because they are single data points and not moraine age ranges; this is explained fully in the caption.

Point-by-point responses to Reviewer 2

While certainly suitable for publication, the manuscript could be improved in several respects. In particular, a more detailed discussion of the factors contributing to the large range of exposure ages from each moraine ridge is called for.

The revised manuscript Discussion (Lines) explores the potential causes of age distributions in much greater detail. This includes new text in Section 3.3, discussing the possibility of complex exposure amongst all clasts but cautioning against a blanket assumption of such, and a quantification and assessment of the impacts of likely erosion rates (Lines 377-383). We have also adjusted the Discussion section to highlight the age distributions, their likely causes, and consequences.

The authors do not adequately discuss the effects of erosion on cosmogenic nuclide concentrations, other than briefly mentioning variable erosion rates as contributing to the dispersion of the apparent exposure ages. It should be stated that all the exposure ages presented are in this sense minimum ages and discuss how that affects interpretations, in particular the constraints on the timing of the non-depositional hiatus.

The reviewer requested greater discussion about the influence of erosion on our sampled boulders; we have added this discussion to Section 3.3, where we show that maximum allowable erosion rates for the central TAM result in a relatively minor shift in age of Pliocene-age exposure ages (and thus don't impact our overall findings. We

With regard to our exposure dates being minimum ages, we argue that this is unlikely to be the case for samples impacted by nuclide inheritance, which would serve to overstate the apparent exposure age. Our modified manuscript highlights that nuclide inheritance is

a likely contributor to at least some of our moraine age distributions, and thus the reported moraine ages cannot be reported as minima for deposition.

It is notable that the dispersion of the exposure ages on boulders from the oldest (highest) drift is minimal. This may partly reflect that the glacier only reached this elevation once, thus resulting in a simple exposure history.

It is indeed possible that the high internal consistency of this older deposit reflects a single period of deposition, with no subsequent burial by ice. We have added this conceptual model to Lines 420-429, but also explain that there are limitations to its applicability generally.

A reasonable assumption is that these samples are at steady state erosion. Thus, a long-term average erosion rate can be determined. The implied similar erosion rate of all six samples is likely a result of the common lithology and that boulders with higher erosion rates may have completely weathered away. Quantifying the erosion rate would be of interest for landscape evolution and could be compared to other estimates of Dolerite and other lithologies from the Trans Antarctic Mountains. This erosion rate could then be applied to the younger samples providing a better estimate of the integrated exposure and duration of the hiatus. Assuming the erosion rate falls within previous measurements (10-30 cm/Myr) it is worth keeping in mind that 1 m diameter boulders will disappear within 10 Ma. Given that > 1 m boulders are relatively rare on the younger drifts, Surface deflation and exhumation of boulders probably contributes to the dispersion of the exposure ages. Finally, if the highest samples are a steady state they could be considerably older than 9 Ma.

As indicated in a previous response, the revised Section 3.3 now includes a detailed test of the impact of current (recently calculated) erosion rates on our age distributions at Otway Massif. We find that although erosion must be > 0, it does not impact the overall chronology significantly, particularly at the younger end of our record.

I suggest not using averages for interpreting the exposure ages. Given that the relative contribution of prior exposure vs. erosion exhumation and cover to the scatter in apparent exposure ages on each moraine is unknown, there is really no reason to expect that the true age of the moraine is approximated by the average. This is especially clear in the case for samples from the Montana Moraine. Given the exposure ages of boulders range over 2 million years on all but the OLD moraine, it seems unlikely (impossible?) the dispersion is due primarily to erosion and fracturing of boulders. Rather, this is more likely reflecting prior exposure and exhumation.

Throughout the revised manuscript (text and figures) we now focus on the age ranges. Mean ages have been removed from figures and interpretation with the exception of the Upper unit, which (after pruning outliers) comprises only one sample.

Indeed, although the time scale is much longer, Fig 4 shows the same pattern observed for age-elevation transects of LGM and younger drift. As such it is worth considering a similar interpretation; that the youngest samples may indicate the last time the ice margin stood at that elevation.

While we accept the reviewer's argument here, we avoid using this approach because it implies that geomorphic processes resulting in boulder exhumation (e.g., ice core ablation, deflation), and which therefor give exposure ages younger than the true moraine age, to be negligible. Yet, in these environments, long-term settling/modification of moraines is almost certainly responsible for at least part of the age spreads characteristic of central TAM surface-exposure datasets. To avoid this limitation, we have retained the range as the most reasonable age estimate, while also modifying the text (Lines 431-445) to justify this more fully.

Older samples on each moraine may indicate earlier advances to the same elevation or higher. Although the surface weathering supports a trend toward lower ice elevations over time, there is no reason to assume that this process was monotonic; all of these moraines may have been overridden more than once, and not all advances necessarily left significant moraine ridges.

We agree with the reviewer that the apparent surface lowering is unlikely to have been monotonic and accept that some moraines at Otway may well include 'composite' ages from multiple advances of the EAIS. The revised text includes this discussion in Lines 420-429.

This possibility is supported by the three sandstone samples with paired 10Be-21Ne measurements. These samples all fall below the simple exposure field (Fig 5) requiring at least one burial event. Based on this result, it is not unreasonable to assume that most if not all Dolerite samples also have complex exposure histories. Given these observations, greater discussion of the likely complex glacial history and the limitations of single nuclide cosmogenic dating on these old Antarctic surfaces is warranted.

The revised manuscript explores this lithological difference and the likelihood of <u>all</u> samples being affected in Lines 336-355. This is a very important addition to the paper that has increased transparency.

#177: youngest to oldest should be changed to lowest to highest. Chronology is inferred. In this descriptive section, inference should be avoided.

This change has been made.

#279: Ackert 2002 (here and elsewhere) should be Ackert 2000.

We have rectified this error throughout.

#361: Uplands of Antarctic interior. These are all high elevation sites where significant warming could occur before a change in glacier regime.

We have made this change.

#381: I suggest that some landforms change to all landforms, see discussion above.

This change has been made.

#393: change to accuracy of single nuclide age estimates in cold-based glacial regimes remains poor and that in most (all?) instances ranges are the only reliable means for presenting ages.

We have made this change.

#397: Fig 6. Given the argument for not using average ages, I would remove Fig 6c. In any case, it does not seem to provide useful information. You might consider including some long-term ice modelling results. See for example Fig 6 of Mukhopadhyay et al 2012. The timing of modelled ice volume changes is broadly consistent with data presented here indicating resumption of glacial deposition 3-4 Ma. Note also. That modelled ice elevations gradually increase. This makes sense in that climate gradually cooled. Why would the earliest post hiatus glacial advances be the largest?

Figure 6 now displays moraine age distributions as ranges, in line with the reviewers concerns. This is explained in the revised caption.

#452: change to possible episodes of smaller-than-present ice extent cannot be ruled out from the available subaerial geologic record.prefe

Text has been adjusted as suggested.