Author response to referee comments on "A 14.5 million-year record of East Antarctic Ice Sheet fluctuations from the central Transantarctic Mountains, constrained with cosmogenic ^{3}He, ^{10}Be, ^{21}Ne, and ^{26}Al"

We thank Julia Lindow for her thorough review of the manuscript and her insightful comments.

Below, we address referee comments and describe additional, unsolicited changes that we've made to improve the manuscript. Referee comments are supplied in bold, with our responses in regular text.

Specific comments:

1. Based on the detailed description of field work and sampling, the authors put a great deal of effort into sample selection and documentation, especially to minimize effects of common complications in surface exposure dating, e.g. nuclide inheritance or non-cosmogenic nuclides. So mainly out of curiosity, could some boulders of sufficient size have provided shielded samples to get direct measurements of inherited / non- cosmogenic nuclides in combination with the surface samples?

We did not collect samples shielded by larger boulders or from the undersides of boulders, although recognize that this technique may be of use for quantifying inherited and/or non-cosmogenic nuclides (e.g., Valletta et al., 2017). However, on lines 148–153 of the manuscript, we discuss previous estimates of non-cosmogenic ^{3}He in Ferrar dolerite, noting that these values are within measurement error for our samples.

2. No potential shielding from snow cover is discussed, and I assume it is considered negligible in respect to locality and the known average low snow accumulation. However, the age of the samples allows for some degree of uncertainty on seasonal or prolonged snow cover, and I would be interested to hear the authors thoughts on this.

As this comment suggests, significant persistent snow cover is inconsistent with local climatology, extremely low subaerial erosion rates over the last 15 Ma, and salt accumulation in TAM soils. Further, the observation that Roberts Massif is a long-term ablation area, as evidenced by the surrounding modern blue-ice ablation zones and the abundant moraines (especially younger than 3 Ma) throughout the massif, supports the idea of low snow accumulation at this location. Given these observations, we've made the assumption that a snow cover correction is not necessary over the course of our record, despite the old age of the landforms.

3. Line 269-271: "First described by Mercer (1972), the Sirius Group occurs throughout the upper (> ~2000 m elevation) TAM as erosional remnants of clay-rich diamicton that are correlated with at least one period of past temperate glaciation." I read this as Sirius deposits are exclusively found above 2000 m, which could be misleading because there are Sirius Group outcrops are at lower elevations, e.g. Hambrey et al., 2003, and Mayewski 1975. I suggest changing the statement to > ~1500 m.

We've updated the text accordingly.

4. Line 571: "≤~200m", this is a little odd, I would just write <~200 m.

We've changed the text to read <~200 m.

5. Section 4.1, Uplift at Roberts Massif: I understand the notion to compare potential uplift rates with existing data (here McMurdo Dry Valleys). However, I question the reliability of evaluating uplift rates or isostatic rebound over the extend of almost 1000 km, and thereby neglecting the influence of regional morphology and geologic structures. For me, the argumentation implies the whole TAM behaved as one block, undisturbed from north to south, while trough incision driven by glacial erosion (as discussed to be the main driver of uplift at Roberts Massif) can also (re-

)activate underlying faults and induce block uplift (e.g. Studinger et al. 2006, or as shown for the Shackleton Range: Paxman et al., 2017). This would reflect in localized uplift rates which could be very different from the McMurdo Dry Valleys. I think this section would benefit from additional details on uplift along the TAM (e.g. Paxman et al., 2019).

We acknowledge the importance of the hypothesis that different TAM blocks have different uplift histories over the last 15 Ma. However, our data do not provide evidence for or against that hypothesis, but rather place bounds on the allowable amount of uplift at Roberts Massif over the course of our record. Given this, we don't discuss differential uplift across the TAM in this paper, but provide evidence for uplift rates elsewhere in the TAM for completeness.

Technical corrections:
1. Fig 1 and 3: missing scale bar and Lat/Lon labels (Fig 1), also, if possible, highlight/ mark study area in figure 1.

We've added a scale bar, lat/long labels, and a box highlighting the study area to for Figure 1. For Figure 3, we've included dimensions in the caption.

2. Fig 4, caption: no mention of (d) in the caption and missing reference to (d) under a); see : "…with numbers corresponding to moraine names in (c) and letters A and A' corresponding to positions in (c)."

We've added a description of and reference to panel (d) to the caption and further updated the caption for clarity.

3. Fig 5 (b), caption: It would be interesting to know the length of the pole for better scale or just give an approximate thickness.

The caption now includes the pole length (120 cm).

4. Fig 11: text and axis labels are quite small, and rather hard to read.

All font sizes will be revisited for final production files.

5. Fig 12: Please check numbering for BBY, BGE and WAL, it's different in figure 4. Also in the map (Fig 4) it is not quite clear which one is BGE.

The labels for BGE and WAL were erroneously switched in Figure 12. This has been corrected. Information about the NLO/NLI and POS moraine complexes, and clarification about the BBY and BGE moraines has also been added to the caption.

6. Fig 14, caption: (c) is missing, and as a consequence subsequent description is off by one letter. "Colors on the timescale at the bottom correspond to moraine colors in Figures 4, 7, and 8." They don't, at least not for the reader, e.g. 'Pliocene' is more yellow then the orange of the moraines in the overview figures. Also, the color scheme used for the age data (d) implies a relation to the timescale used, which I find a little confusing. Maybe a different set of colors or symbols could make this figure clearer.

We've corrected the panel references in the caption. To avoid confusion with the timescale color bar, we've removed these colors altogether as well as the reference to Figures 4, 7, and 8. Finally, we added a legend to panel d, rather than listing the colors in the figure caption, to improve clarity.

7. For the figures in general: The marker and information overlaying satellite maps are of mediocre quality/readability, which might be the result of compressing the images for this pre-

print version, if not it would be worth looking into to ensure good quality images in the final version.

We will ensure that the final version includes print-quality images.

Additional changes to the manuscript:

1. Numbers were switched for the WBK and POS moraines in Figure 4. This has been corrected.
2. Corrected erroneous section references on lines 509 and 524.
3. Corrected erroneous figure references on lines 505, 513, 519, 584, and 603.
4. Clarified figure reference location in sentence on lines 332–335.