

Response to Reviewer 2

The reviewer comments in black and our response in blue.

Reviewer 2

Thomas et al. present a new shallow firn record from Peter the 1st island in the Bellingshausen Sea, Antarctica. After developing the timescale of this 15-year-long record, they evaluate relationships among accumulation, melt layers, and $d18O$ with reanalysis products and with a short automatic weather station dataset. The paper lays the groundwork for interpreting a potential deep ice core drilled at the site, which is expected to span about 200 years. With some organizational restructuring and some clarifications, the paper is worthy of publication in a journal such as *Climate of the Past*.

Thank you for your time reviewing this manuscript, your comments and suggestions are appreciated.

Major comments:

The development of the age model is foundational to the paper, as the accumulation rate derives from this and is used in the subsequent correlations. I am surprised by the tiny size of the seasonal variations in the major elements, and am curious what $nssCa$ would show. Ice core records from West Antarctica typically show well-resolved annual dust peaks, so it seems like Ca could be really helpful here. If the data were collected, why are they not included? At any rate, most of the annual picks seem reasonable visually; however, the pick between 2005 and 2006 (Fig. 2) aligns with troughs in most of the major ions, rather than peaks as for most years – and there are peaks to either side of this pick that are not used. Why is this?

Annual layer counting is a powerful tool for ice core research; however, it has its limitations. While many proxies display clear seasonality at inland locations, the seasonal deposition at lower elevation, coastal and island sites can be quite different. This is likely a result of the lower elevation of the arriving air-masses, which contain a greater contribution of local (marine) species and compounds, but less contribution from long-range sources (e.g., dust). Sulphate is a good example of this, and has been explored recently in Emanuelsson et al., 2022.

In the case of Ca, the record does display a seasonality. However, it looks almost identical to Na. The two records are highly correlated ($r=0.8$, $n=249$), suggesting that a significant contribution of Ca is from marine (not continental) sources. When comparing with the seawater ratio (based on Cl), $nssCa$ accounts for just ~28% of the Ca.

Second point related to the timescale is that the potential tie-points (Puyehue Cordon Caulle and the two melt years, 2006 and 2013) need to be tied in earlier in the text and not saved for the discussion. This text should be part of section 3.1 so that the reader can fully evaluate timescale development with all information in the same place. The apparent PCC peak should be highlighted in the figure. The 3-point smoothing draws the eye away from this prominent peak, but I think it would be helpful to make it more obvious. I would suggest instead of a dashed line, using a thin solid line for the full-resolution data. In the Fig. 2 caption, clarify that summer peaks are defined as Jan. 1 (or alternate date, as chosen by authors). Labeling the years between the lines (rather than directly above each line) is a bit un-intuitive for me, so it would be helpful to define exactly what the black lines represent.

Thank you for all the great suggestions. We have made the updates to figure 2 as suggested.

We have also moved the text from the discussion (4.1) to the main results section (3.1).

I also agree with Reviewer 1's main points, but as these concerns have already been raised, I will address other issues.

Minor comments:

Line 103: Please include actual time spans of data collection. As written, it implies one year – but the actual record is much shorter.

Updated

Line 107: Needs a transition here.

Updated.

Line 120: This seems like a small subset of the major ion suite that is typically measured. Why were Mg^{2+} , Ca^{2+} , NO_3^- , NH_4^+ , Cl^- , etc. excluded from the analysis? Or were they analyzed but not included here? It seems like $nssCa$ could help support the timescale development, so it would be nice to see it included. If the other data are to be presented elsewhere, then include a statement to that effect. Okay, re-reading this I see in section 3.7 that other major ions were also measured. Please update the methods to include all measured ions, and present the data in Fig. 2 to reinforce timescale picks, as suggested above.

The full suite of data was analysed, however, we selected only the optimal data for figure 2. This was to avoid over-crowding and repetition. E.g., most of the cations (Na, Mg, Ca) behaved the same and adding individual curves didn't add much. However, we can either update figure 2 with the additional data or provide a supplementary figure containing all the major ions.

Regarding $nssCa$, this might not be appropriate for an island location. We can assume that the continental fraction would be extremely low.

Line 129: correct to "site's"

Corrected

Line 130: "ensuring" seems like an odd word here. Wouldn't "assuming" be more appropriate? And why is the assumption that the firn core only samples the upper 10% of the ice cap's thickness? I think a little more explanation of the reasoning here would be helpful.

This was to justify the use of the Nye model, which is appropriate for the upper 10% of the ice sheet. I have rephrased the sentence to better reflect this.

Line 131: Include some uncertainty estimates based on other thinning models. How much could that 130 m estimate reasonably change based on the model you use?

This is a little difficult to explore with the limited data that we have. While we accept that the Nye model is probably not appropriate, the lack of in-situ observations (e.g., strain rate) limit the other thinning functions that could be applied. We have attempted to estimate the error range by altering the annual snow accumulation variable. To avoid over-interpreting the short record that we have, we would rather expand on the thinning function using in-situ geophysical information in the future.

Line 161: Should be “provide”

Corrected.

Line 172: the circle appears purple, not blue. For this figure, the black/gray/dark purple coloring makes it a little tricky to visually resolve the lines. I would suggest using more of a royal blue (i.e., more contrast with the black and gray) for the ERA5 data.

The figures have all been updated to RGB colour mode, to provide better contrast (including brighter blues).

Line 191: There is a fragment here that needs revision.

Updated.

Line 179 and the paragraphs that follow: Here it is stated that “a lapse rate is applied.” However, it is unclear what rate is actually used and why. This whole section (3 paragraphs about lapse rates) could use reorganization and clarification.

Paragraphs updated and clarified.

Line 200: This section 3.3 seems like a subheader of the section above, as the two topics are closely related.

These sections can be combined to include 3.3. as a sub-sections of 3.2.

Line 207: Replace “meltiest” with “heaviest melt” or similar

Noted, although it is a good word.

Line 211: There does not appear to be a melt layer in Dec 2006 (Fig2). Can the authors clarify or explain why this may be?

Given the uncertainties of the dating, it is hard to accurately assign melt to a single month. However, there is a melt layer in the summer of 2006/2007. In figure 2, this appears coincident with the year marker (black line), which we assume spans the warmest months of the year (e.g., December 2006/ January 2007).

Line 231: Another fragment here that needs to live its full life as a sentence.

Corrected

Line 239: This paragraph discusses accumulation, which is listed as the header of the next section. This whole part of the discussion needs reorganization in its structure and labeling. Part of this could be to remove the heading of 3.5: Accumulation, and allow the discussion of accumulation to follow smoothly from heading 3.2 as these paragraphs are so closely related.

Agreed, these subsections can be combined.

Line 302: Change “warm-moist” to “warm, moist”

Changed

Fig. 5: Please label the y-axes next to panels d and g “ERA 5 P-E (year range)” so it’s clear exactly what the plots show.

Updated

Line 329: Change “temporality” to “temporally”

Updated

Line 350: Please cite Koffman et al 2017 for the PCC reference horizon

Absolutely, sorry for the omission.

Section 4.1 can be moved into the first discussion of the age scale.

Agreed

Line 365: Would “ice cap” be more appropriate than “ice sheet” here and throughout the paper?

Line 384: looks like a typo in the citation; colon should be a semicolon

Line 403: Correct to “or has been lost”

Line 422: “island’s”

Line 440: awkward syntax in this sentence, please revise

Line 448: Please clarify which continent (South America or Antarctica” and change “to” to “the”

Line 466: Change “is” to “can be”

Line 478: correct to “is a subject for further study”

All points above updated and corrected.

Paragraph lines 477-484: This paragraph needs to be reorganized. Give the information and observations about melt years and then end with saying that the topic will be studied further – but leading with this does not fit well with the additional details given immediately afterward.

Agreed.

References:

Added

Koffman, B. G., Dowd, E. G., Osterberg, E. C., Ferris, D. G., Hartman, L. H., Wheatley, S. D., Kurbatov, A. V., Wong, G. J., Markle, B. R., Dunbar, N. W., Kreutz, K. J., & Yates, M. (2017). Rapid transport of ash and sulfate from the 2011 Puyehue-Cordón Caulle (Chile) eruption to West Antarctica. *J. Geophys. Res. Atmos.*, 122. <https://doi.org/10.1002/2017JD026893>