Author responses to referee comment 1 – Anonymous referee 1

Review of

High centennial-scale variability in an East Antarctic snow accumulation record spanning the last millennium

by Nilssen and others

General

The paper presents a new millennial-length annually-resolved snow accumulation record from Wilkes Land, East Antarctica. Such long accumulation time series data are valuable: they provide important long-term context for observed temporal surface accumulation variability based on (much) shorter time series from e.g. stakes, shallow firn cores and/or atmospheric models. The results presented in this study show significant regime shifts that appear to differ significantly from the (relatively) nearby deep ice core drilled at the coastal site Law Dome. Interesting information is also obtained from using two different ways to correct for strain induced layer thinning. The paper does lack some contextualization and remains a little bit thin on interpretation, so that sometimes it reads more like a data paper. But it is worth publishing as the new time series is important and the analysis robust. Furthermore, the paper is well written (pending some clarifications, see below) and the figures are of good quality. My comments are therefore relatively minor.

Thank you for your comments and feedback on our manuscript, and for recognising the importance of this new accumulation time series. In our investigations, we found nothing clear or obvious from the literature or from previous ice core work that explains the centennial scale variability in accumulation at Mount Brown South. We are very interested in being able to interpret where the variability comes from, and the reasons for the variability are an active area of future research. The level of variability present in this ice core record has likely redirected the focus of my PhD, and future research will include investigating stake measurements and past shallow cores from Princess Elizabeth Land/Wilhelm II Land to investigate the spatial variability of snow accumulation in this region.

To address the comment regarding contextualisation and interpretation, we will expand on the discussion in section 5.3, about the possible implications of temporal variability in snow accumulation and its effects on sea level rise. We will also include more discussion regarding the influence of synoptic-scale weather events on snow accumulation, and possible teleconnections between the MBS climate and southern Australian climate.

Major comments

When discussing the drill site, it would be nice to provide some climatological context, e.g. you could provide maps encompassing the same area as in Fig. 1 to present and briefly discuss annual average temperature, annual accumulation rate (from e.g. ERA5).

The climatological context of the Mount Brown South site has been explored and discussed in previous literature, e.g. Vance et al. (2016; 2024), Crockart et al. (2021), Jackson et al. (2023), Udy et al. (2026). We will add maps encompassing a similar area as in Fig. 1, showing mean annual precipitation and mean temperature, both from ERA5. Seasonal wind direction and precipitation related variability are already presented in Vance et al., 2024, and we will add a description of this to the manuscript.

Maybe I missed it, but can you say something about the ice velocity at this site, i.e. how far the core site travelled horizontally over the period the accumulation record covers? Compared to the regional accumulation gradients, what does this advection imply for the consistency of the accumulation time series in the core (probably little, but good to state an estimate)?

This is a good point, thank you. The MBS ice core site was chosen following an extensive site selection process, which included criteria such as locating for minimal ice advection or elevation change (Vance et al., 2016). Figure 3 in Vance et al. (2016) shows that the MBS site has experienced an elevation change of around 50-100 m in the last 1000 years, and horizontal ice advection of 10-20 km in the last 1000 years. The elevation change would result in a negligible change in temperature (e.g. a maximum of 0.5-1.0 degree Celsius), and the advection at the site would result in a negligible change in annual accumulation. This is reinforced by the 1997/98 short core drilled at a site ~13 km to the west of MBS, which had a virtually identical mean accumulation rate (297 kg m-2 yr-1) (Foster et al., 2006), suggesting minimal accumulation variability in the region.

We will add this discussion to the text.

Figure 2: Can you comment on the fact that the empirical fit appears to systematically overestimate density below a depth of ~150 m?

Thank you for this comment. We acknowledge the apparent divergence of the empirical fit from the recorded core density measurements. We will add the following to the text, in section 5.4:

“There is generally good agreement between the density measurements and the empirical fit, but we note that the empirical fit appears to overestimate density below a depth of around 150 m. The average difference between the measured and modelled densities below 150 m is around 5 kg/m3 (~ 0.5 % difference), with a maximum difference of about 10 kg/m3 (~ 1 % difference). This means that the densities may be overestimated by about 1 % in the bottom half of the main core. We also note that in the

bottom ~ 20 m of the main core, density measurements may have been less accurate due to declining core quality."

line 156: How sensitive are your results to the choice of l (here l = 50)?

Thank you for pointing this out. We will add the following to the text:

"The choice of cut off length *l* is subjective. We tested various values of *l* (e.g. 30, 50, 80, 100), and decided to use *l*=50 to detect regimes on the multidecadal to centennial scale. With lower values of *l* (e.g. 30), the algorithm detected shorter regimes, with more regime shifts, and higher values of *l* returned longer regimes with fewer regime shifts. Many of the same regime shift years were identified in all the runs with differing values of *l*. Discussion of the effects of choice of *l* is discussed in Stirnimann et al. (2019)."

Can you provide an estimate of the (relative) uncertainty in your ice core-derived annual accumulation values?

It is very difficult to isolate the potential uncertainties in dating, density adjustment, and strain thinning adjustment to a single estimate, e.g a percentage. These factors are all independent and vary with depth. In regards to uncertainty in dating and the placement of annual horizons, please refer to the extensive analysis in Vance et al (2024), where the maximum possible error between known volcanic tie points has been estimated. In the composite record we used (MBS-Main 873-1989, MBS-Charlie 1990-2017), there are 131 uncertain counted years (i.e., there is certainty of a year, but the exact placement of the annual horizon is less certain) and 72 uncertain uncounted years (i.e., there was some evidence of an annual cycle in the chemistry and isotope records, but not enough to count a year in the chronology) across the full1145 total counted years of the chronology. However, the 72 uncertain/uncounted years do not imply a potential for 72 year error, as the chronology is well constrained by volcanic tie points – this is more a dating method assessment (Vance et al., 2024)

For uncertainty in density, we are aware that density is most variable and hardest to measure accurately in the shallower parts of the core. Similar to Medley et al (2018), we estimate a maximum uncertainty of 10 % at the surface, 5 % in the top 5 m (~2008 CE) and <2 % at 10 m (~ 1998 CE) and deeper. Better constraining surface density estimates will be a critical aspect of the rest of my PhD project.

For uncertainty in strain thinning, we examine the resulting layer thicknesses when using the upper and lower bounds of the strain rate estimation, −2.59 × 10−4 ± 1.5 × 10−5 year−1, i.e −2.44 × 10−4 year-1 and −2.74 × 10−4 year-1. For the deepest layer in the core, the difference in layer thickness resulting from these upper and lower estimates of strain rate is about 3 %.

We will add some discussion of these uncertainties to the manuscript.

Minor and textual comments

The first sentence of the abstract is inconsistent. Please reformulate, e.g. consider replacing "ice loss at the margins of the Antarctic Ice Sheet" with "ice loss at the margins of the West Antarctic Ice Sheet". Or replace Mass balance" by "Surface mass balance"?

We will reformulate this sentence as per your suggestion.

line 3: "snowfall variability" do you mean "snow accumulation variability"?

We will change this to “snow accumulation variability”

line 25: "Surface mass balance, which is dominated by snowfall variability" suggest "Surface mass balance variability, which is dominated by snowfall variability".

We will add “variability” after “surface mass balance”, as per your suggestion.

line 28: "Ice core records extend the climate record" suggest "Ice core records extend the record of direct accumulation observations". Because ice cores are part of the climate record.

We will update the manuscript as per your suggestion.

line 61 and elsewhere: surface -> shallow (suggestion)

We will replace “surface” with “shallow” when referring to the shallow cores.

line 68: Please clarify "unconstrained" in this context.

We are referring to annual layer thickness with no density adjustment. We will remove the word “unconstrained”, as this is assumed.

line 147: Why "thus"?

We change this sentence to “Our discussion thus focuses on the ApRES-derived result, as we believe this is the better strain thinning adjustment and gives a more accurate result.”

line 274: snowfall accumulation records -> snowfall accumulation records derived from ice cores

We will update the manuscript as per your suggestion.

References

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