Author responses to referee comment 2 – Anonymous referee 2

Review of "High centennial-scale variability in an East Antarctic snow accumulation record spanning the last millennium" by Nilssen et al.

The paper present and discuss snow accumulation data from a 120 m deep ice core drilled from a "semi-coastal" site in the Indian Ocean sector, in East Antarctica. The Mount Brown South (MBS) ice core covers about 1000 years and is from an area where not much data exists from before. Many challenges remain for estimating the Antarctic ice sheet surface mass balance (SMB), which represents a major uncertainty in predictions of future sea-level rise. Validating continental scale studies is hampered by the sparse distribution of in situ data and there are still large error bars on the estimates that do exist. There is generally a lack of high-resolution ice core data from Antarctica and therefore this drill site is particularly interesting. The ice core site has relatively high accumulation rates and thus a good position to date and compare annually resolved data with both instrumental- and model output data. Therefore, the data presented in the paper are important and will add an important piece to these estimates.

In addition to establishing the temporal variability of the snow accumulation, this study aims towards reducing uncertainties in density and strain thinning, something that becomes an issue when reconstructing accumulation at depth. I consider the method description thoroughly handled and think this is one of the strengths of the paper.

Even though the MBS record is a valuable addition to the information from the existing ice cores I don't consider the results groundbreaking (e.g. line 203-205). The great variability of spatially and temporally is precisely why more records are necessary to get a better idea about the SMB of Antarctica.

Thank you for the review of our manuscript.

We agree that the MBS record is a valuable addition and that more records like it are necessary. However, it is unclear to us why the reviewer considers the findings to not be groundbreaking. The centennial variability and significant regime shifts we report for MBS differ significantly from the only other millennial length annual accumulation record for the coastal region spanning Princess Elizabeth Land through to Wilkes Land (i.e. the Law Dome record shown in Fig 5). To address this comment, we will make the implications from our key findings clearer in the discussions. We will describe how the variability in snow accumulation rate as observed at MBS may influence surface mass balance and the Antarctic contribution to sea level rise on timescales ranging from a few years to more than a century. We will add a version of the following paragraph:

Currently, the primary datasets for studies of surface mass balance and ice sheet mass change span at most the satellite era. With this record, we demonstrate that the satellite era is not representative of the true range of natural variability (at least for this

region). This is a key contextualisation that needs to be incorporated into our understanding of surface and even ice sheet mass balance, given that in any other period during this record, the background state of the SMB regime would be significantly different.

There are some questions and concerns that I would like the authors to clarify before the paper is ready for publication.

Major comments

Accumulation is not necessarily equal to snowfall precipitation. SMB is the net result of multiple processes such as surface mass gains from snowfall and deposition and losses from snow sublimation and wind erosion. There is a very brief meeting of the **meteorological conditions** in section 2.1 where previous studies of snow accumulation are mentioned. As I understand, the sites are on the escarpment between the coastal area and the polar plateau, and the wind is likely an important factor. However, the wind direction is mentioned but nothing about the wind speed. Maybe the meteorological conditions are discussed in the previous papers but are fundamental when discussing snow accumulation and must be included in some form also here. I also wonder if there is any contribution of dry deposition?

Figure 2 in Vance et al (2024) shows the seasonal wind speed, frequency (as a percentage of time) and direction at the MBS site during high and low snowfall regimes, using MAR. The mean wind direction is ESE (90-110 degrees), and more accumulation arrives when the flow is more easterly. For high snowfall regimes (> 1 mm water equivalent per day), the wind speed is predominantly (>90% of the time) under 12 ms-1. For low snowfall regimes (< 1 mm water equivalent per day), the wind speeds are under 12 ms-1 for >95% of the time. Wind speeds are greatest in austral winter, and lowest in austral summer.

We will add this description of the wind speeds to the manuscript.

We acknowledge that clear sky precipitation is likely to occur at MBS, but its contribution to the annual accumulation is expected to be small (~ 5 %) relative to synoptic-scale snowfall events at a site with an accumulation rate of approximately 0.3 m yr-1 IE (Dittman et al 2016, Palm & Yang 2025).

We will include this discussion of the likely minimal contribution of clear sky precipitation to the manuscript.

2. Much of the discussion is based on having established a solid annual resolved **chronology**. However, not much is presented in the paper but refers to previous papers. I think that the **dating error** is important to highlight. There are several places in the discussion when specific years are mentioned and/or very brief time spans. To facilitate

the (critical) reader I would suggest including a summary of the dating from Vance et al., 2024.

The MBS chronology is tied via volcanic tie points to Law Dome (Plummer et al., 2012), WAIS divide (Sigl et al., 2016) and Roosevelt Island (Winstrup et al., 2019). The earliest tie point in the MBS record is 1040.6 CE, tied to WAIS divide at 1040.3 ±2. There are also multiple tie points to the well-dated Law Dome record, the earliest being 1172.3 CE (MBS) tied to 1171.4 ±1 (Law Dome) (see Table 3 in Vance et al., 2024).

We will include a summary of the dating paper in the manuscript methods and discussion where appropriate.

3. Accumulation is closely linked to climate and when discussing the **temporal variability** on larger scale as is done here, it would be relevant to present the d18O record. For instance, in section 4.2 a potential connection between volcanic eruptions and low snow accumulation is mentioned. Looking at Fig 3a it seems like there is no major eruption in connection with the higher accumulation regimes. I think there are modelling suggesting that volcanic eruptions influence global precipitation. I understand that this is a purely speculative discussion in the connect of this paper but nevertheless interesting to think how the injection of aerosol at low latitudes could affect the transport of moisture to Antarctica.

Reviewer 2 suggests that water isotopes would help us examine the role of volcanic eruptions. We agree, but note that the latter is not a significant part of the discussion, which emphasises the differences observed at MBS and Law Dome. These differences do not match what we would expect from a global cooling effect from volcanism. The inclusion of water isotopes would require additional work to fully understand the relationship between site temperature and d18O, and then what relationship this has with accumulation variability. We know there is a relationship between accumulation events and d180, but it will need comprehensive analysis to sort out at the annual scale (as presented here), not least because the d18O record is not a straightforward indicator at this site (see Jackson et al., 2023). We tested the correlation between MBS annual accumulation and MBS annual d18O (Gkinis et al., 2024) and found a weak negative correlation ($R = -0.14$, $p<0.01$), further indicating the relationship is not straightforward. Addressing these questions goes beyond the scope of this manuscript.

4. I think it also important to include more about the site specifics when discussing **spatial variability**. I have mentioned the meteorological conditions, but what about radar to resolve the spatial variability of snow layers at different depths?

Unfortunately no snow radar was available for this project, which only had a small team for a few weeks at the site, but ice cap flightlines pass the site and will be accessed

when available. However, icecap is ice radar, not snow radar, so will at best only help with the deepest layers in the ice core, if at all.

I suggest to read Eisen et al. 2008. Ground-based measurements of spatial and temporal variability of snow accumulation in East Antarctica, *Rev. Geophys*., 46, RG2001, http://dx.doi.org/10.1029/2006RG000218 11 April 2008

We will read this paper and include where relevant.

5. The paper **reads well** but when I came to Section 4.1. I am struggling. One problem is that there are many numbers in the text. Here it is important to include CE when referring to years. There are also some unnecessarily long sentences. I also suggest abbreviating ice equivalent throughout the paper.

We will add CE when referring to years, and we will abbreviate ice equivalent to IE throughout the paper. We will revise section 4.1 for clarity and brevity.

Specific and technical comments

Please include CE when years are mentioned. It is not consistent throughout the paper. It sometimes gets confusing when the are several sets of numbers in the same sentence.

Thank you for pointing this out, we will include CE when years are mentioned.

Fig. 1. DSS needs to be explained in the captions.

We will define DSS in the caption.

Line 58. Previous studies… based on what method?

These studies used precipitation from the ERA5 reanalysis (Hersbach et al., 2020) and surface mass balance from the Modèle Atmosphérique Régional (MAR; Agosta et al., 2019). We will note this in the manuscript.

Line 65. As mentioned above – a summary of chronology development is in place here.

We will include a summary of the chronology development here, and will detail our approach using layer counting, comparison between different seasonal species, volcanic tiepoint alignment, and synchronization comparisons to Law Dome and WAIS Divide.

Line 68-73. I found this section hard to follow. If I understand it correctly it might not even be necessary to include?

It is necessary to include this description – the composite record is necessary to extend the main core to the surface at time of drilling. To make this more clear, we will begin

this section with “Here, we construct a composite accumulation record using the annual layer thicknesses from the MBS-Main (873-1989 CE) and the MBS-Charlie (1990-2017 CE) ice core chronologies (Vance et al., 2024)”

Line 174. “The change point in 1461…”. Here is one example of where it is necessary to include CE. Probably rather use “around” and include the dating error.

We will add CE to the year 1461. The dating of this year is quite robust since it is very close to the Kuwae volcanic tie point, identified at 1459.4 at MBS, tied with 1459.7 ±1 at Law Dome.

Line 256. Maybe the expression “basin-wide representation” is somewhat optimistic…

Indeed, we are aware of the spatial variability in surface mass balance and snow accumulation, so we are not attempting to convince the reader that the site-specific snow accumulation reconstruction is actually a basin-wide representation. We are making the point that snow accumulation in this area can exhibit substantial multi-decadal to centennial-scale variability, and other parts of this drainage basin (and indeed East Antarctica) could also exhibit similar temporal variability (which could be investigated with more high resolution paleo records), which could have profound effects on sea level rise and sea level rise estimates.

We will edit this section to make it more clear that we are not suggesting MBS is a basin-wide representation of snow accumulation.

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